C. 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 (*BOLZANO 1817a*)

Bolzano's most significant contribution to mathematics is his epoch-making paper on the foundations of real analysis, the *Rein analytischer Beweis* of 1817. In the years following the appearance of Berkeley's *Analyst*, mathematicians had made various attempts to put the calculus on a firmer foundation. The most common approaches (some of which are recorded above) were to base the calculus on one of the following ideas: on *motion* (Newton, MacLaurin); on *limits* (D'Alembert, L'Huilier); on *ratios of zeros* (Euler); on *infinitesimals* (Leibniz and—with reservations—Carnot). In perhaps the most radical proposal, James (= Jacob/Jacques) Bernoulli proposed amending the laws of logic by abandoning, for infinitesimals, the Euclidean 'common notion' that, if equals are subtracted from equals, the remainders are equal (*Bernoulli, Jacob 1744*, Vol. ii, p. 7). Perhaps the most influential approach was that of Lagrange, who, in his *Théorie des fonctions analytiques* (*1797*), assumed the existence of a Taylor-series expansion for every function; the full title of his work—*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*—shows the scepticism with which he, and many other mathematicians, regarded the notion of limits, and his desire to reduce the calculus to 'l'analyse algébrique'. Bolzano's paper was not the first to attempt to find an analytic foundation for the calculus, nor was it the first to employ the notion of limits (as is shown above by the selections from D'Alembert). But, in contrast to his predecessors, Bolzano employed a limit-concept that was not based upon motion, and that was analytically defined;[a] and, more importantly, he was the first actually to *use* this definition to prove significant mathematical theorems.

Bolzano's announced aim is to prove the intermediate value theorem—in his formulation, that if f and ϕ are continuous functions such that $f(\alpha) < \phi(\alpha)$

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[a] In a footnote in the *Preface* to his *1817b*, Bolzano gives the following definition of an analytic procedure:

A *purely analytic* (also purely arithmetic or algebraic) *procedure* is one by which a certain function is derived from one or more other functions just through certain changes and combinations which are expressed *by a rule completely independent* of the nature of the designated quantities. Thus, for example, the way the function $(1 + x)^n$ is derived from $(1 + x)$ is said to be a purely analytic procedure, for $(1 + x)^n$ is obtained from $(1 + x)$ by making certain changes and combinations in the latter which are given by a rule which is completely independent of the nature of the quantity denoted by $1 + x$.

while $f(\beta) > \varphi(\beta)$, then for some x, $\alpha < x < \beta$, $f(x) = \phi(x)$. This theorem he eventually proves in §15. But he begins with an important critique of previous proofs, and in Part II of the *Preface* he gives the first precise definition of a continuous function. His definition is essentially the same as that given by Cauchy in his *Cours d'analyse* in 1821; whether Cauchy knew of Bolzano's work is uncertain. (Bolzano improved on his definition in his unpublished *Functionenlehre*, written in 1834; there he gives a definition of pointwise continuity and distinguishes between left and right continuity.)

In §7, Bolzano states and attempts to prove the sufficiency of the 'Cauchy condition' for the convergence of an infinite series. A rigorous proof requires a precise definition of the real numbers, which Bolzano did not possess; he himself (in the *Functionenlehre*) later admitted that the §7 proof was incomplete.

Similarly, although Bolzano had a precise definition of continuity, he did not have the modern notion and definition of *function*. Lagrange, in the *Théorie*, had indeed defined a function of one or several quantities to be 'any mathematical expression in which those quantities appear in any manner, linked or not with some other quantities that are regarded as having given and constant values, whereas the quantities of the function may take all possible values'; but in practice he and his successors treated functions as equations. The modern conception did not enter mathematics until Dirichlet's paper, *Über die Darstellung ganz willkürlicher Functionen*, in 1837.

Having defined continuity and stated the Cauchy condition, Bolzano proceeds (§12) to prove a lemma that was eventually to become the cornerstone of the theory of real numbers. This lemma (the greatest lower bound principle) is the first published version of the Bolzano–Weierstrass theorem, which, in modern terminology, says that every bounded infinite point-set has an accumulation point.

Although Bolzano's proofs are incomplete, and although they are somewhat clumsily presented, this paper is a milestone in the history of real analysis. It was the first successful attempt to free the calculus from infinitesimals, and it is the starting point for the modern theory of the continuum; the precision of Bolzano's definitions and the rigour of his deductions mark a break with the mathematics of the past. The project of putting the theory of the real line on a solid, arithmetical foundation was to be carried forward, largely in ignorance of Bolzano's work, throughout the nineteenth century—most notably by Cauchy, Abel, Dirichlet, Weierstrass, Cantor, and Dedekind.

It is important to appreciate the role Bolzano's objective conception of axioms, described above in the *Beiträge* (*Bolzano 1810*), played in the *Rein analytischer Beweis*. Bolzano was not driven by scepticism or by fear of paradox. He makes it clear that he did not doubt the *truth* of the intermediate value theorem; and he was not attempting to place it on firmer or more obvious foundations—for the greatest lower bound principle is, if anything, less evident than the theorem he is trying to prove. Similarly, his criticism of the proofs based upon motion is not that intuitions of motion are unreliable, but the

logical objection that the proofs beg the question. Bolzano's ambition was not so much to attain some superior brand of mathematical certainty as to reveal the objective reasons for the truth of the intermediate value theorem—to uncover its true logical foundations. And *a fortiori* Bolzano's quest for rigour in his *1817a* was not prompted by the 'challenge to geometric intuition' presented by the discovery of continuous nowhere-differentiable functions; on the contrary, it was Bolzano's rigour that made his subsequent discovery of such counter-intuitive phenomena possible. (The discovery is in *Bolzano 1930*, §75; the passage was written in 1834.)

The translation of *Bolzano 1817a* is by Stephen Russ; references should be to the section numbers, which appeared in the original edition.

PREFACE

There are two propositions in the theory of equations of which it could still be said, until recently, that a completely correct proof was unknown. One is the proposition: *that between every two values of the unknown quantity, which give results of opposite sign, there must always lie at least one real root of the equation*. The other is: *that every algebraic rational integral function of one variable quantity can be decomposed into real factors of first or second degree*. After several unsuccessful attempts at proving the latter proposition by *D'Alembert*, *Euler*, *de Foncenex*, *Lagrange*, *Laplace*, *Klügel*, and others, finally *Gauss* supplied, last year, two proofs which hardly leave anything to be desired. Indeed, this outstanding scholar had already presented us with a proof of this proposition in 1799;* but it had, as he admitted, the defect that it proved a purely analytic truth on the basis of a *geometrical consideration* |*Betrachtung*|. But his two most recent proofs** are quite free of this defect; the *trigonometric functions* which occur in them can, and must, be understood in a purely analytical sense.

The other proposition which we mentioned above is not one which has concerned scholars so far to any great extent. Nevertheless, we do find mathematicians of great repute concerned with the proposition and already *different* kinds of proof have been attempted. To be convinced of this one need only compare the various treatments of the proposition which have been given by, for exam-

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* *Demonstratio nova Theorematis, omnem functionem algebraicam rationalem integram unius variabilis in factores reales primi vel secundi gradus resolvi posse. Helmstadii 4° 1799.*

** *Demonstratio nova altera etc., and Demonstratio nova tertia*: both 1816.

ple, *Kästner*,* *Clairaut*,** *Lacroix*,***Metternich*,+ *Klügel*,++ *Lagrange*,+++ *Rösling*,++++ and several others.

However, a more careful examination very soon shows that none of these proofs can be viewed as adequate.

I. The most common kind of proof depends on a truth borrowed from *geometry*, namely, *that every continuous line of simple curvature of which the ordinates are first positive and then negative* (or conversely) *must necessarily intersect the x-axis somewhere at a point that lies in between those ordinates.* There is certainly no objection against the *correctness*, nor indeed against the *obviousness* of this geometrical proposition. But it is equally clear that it is an intolerable offence against *correct method* to derive truths of *pure* (or general) mathematics (i.e. arithmetic, algebra, analysis) from considerations which belong to a merely *applied* (or special) part, namely *geometry*. Indeed, have we not felt and recognized for a long time the incongruity of such μετάβασις εἰς ἄλλο γένος?[a] Have we not already avoided this whenever possible in hundreds of other cases, and regarded this avoidance as a merit?* So if we wish to be consistent must we not try and do the same here?—For in fact, if one considers that the proofs of the science should not merely be *confirmations* ‖*Gewissmachungen*‖, but rather *justifications* ‖*Begründungen*‖, i.e. presentations of the objective reason for the truth concerned, then it is self-evident that the strictly scientific proof, or the objective reason, of a truth which holds equally for *all* quantities, whether in space or not, cannot possibly lie in a truth which holds merely for quantities which are in *space*. On this view it may, on the contrary, be seen that such a *geometrical* proof is, in this as in most cases, really circular. For while the geometrical truth to which we refer is (as we have already said), extremely *evident*, and therefore needs no *proof* in the sense of *confirmation*, it none the less needs *justification*. For its component concepts are obviously so combined that one cannot hesitate for a moment to say that it is not one of those simple truths which are called *basic propositions* ‖*Grundsätze*‖, or *basic truths*, because they are only the *ground* for other truths and never themselves consequences. On the contrary, it is a *theorem* or *consequential truth* ‖*Folgewahrheit*‖, i.e. such a truth as has its basis in certain other truths; and

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* *Anfangsgründe der Analysis endlicher Grössen*. 3rd ed. §316 [*Kästner 1794*].
** *Elémens d'Algèbre*. 5th ed. Supplémens. Chap. I no. 16 [*Clairault 1797*].
*** *Elémens d'Algèbre*. 7th ed. [cf. *Lacroix* c.*1805*].
+ In his *translation* of the above work of Lacroix. Mainz. 1811. §211 [*Lacroix 1811*].
++ In his *Mathematisches Wörterbuch* Vol. 2, p. 447 ff. [Klügel 1803-31].
+++ *Traité de la résolution des équations numériques de tous les degrés*. Paris. 1808 [Lagrange 1808].
++++ *Grundlehren von den Formen, Differenzen, Differentialien und Integralien der Functionen*. Part 1, §49 [Rösling 1805].
[a] ['crossing to another kind' A similar phrase occurs in Aristotle's *Posterior Analytics* 75a 38: 'One cannot, therefore, prove by crossing from another kind—for example, something geometrical by arithmetic.'‖
*The papers of Prof. Gauss quoted before provide an example.

therefore, in the science, it must be proved by a derivation from these other truths.* Consider now the objective reason why a line in the above-mentioned circumstances intersects the x-axis. Everyone will, no doubt, see very soon that this reason lies in nothing other than that general truth, as a result of which every continuous function of x which is positive for one value of x, and negative for another, must be zero for some intermediate value of x. And this is just the truth which is to be proved. It is therefore quite wrong to have allowed the latter to be derived from the former (as happens in the kind of proof we are examining). Rather, conversely, the former must be derived from the latter if we wish to represent the truths in the science in the same way as they are linked to each other in their objective coherence.

II. No less objectionable is the proof which some have constructed from the concept of the *continuity* of a function with the inclusion of the concepts of *time* and *motion*. 'If two functions fx and ϕx,' they say, 'vary according to the law of continuity and if for $x = \alpha$, $f\alpha < \phi\alpha$, but for $x = \beta$, $f\beta > \phi\beta$, then there must be some value u, lying between α and β for which $fu = \phi u$. For if one imagines that the variable quantity x in both these functions gradually takes all values between α and β, and the same value is always taken by them both at the same moments, then at the *beginning* of this continuous change in x, $fx < \phi x$, and at the *end*, $fx > \phi x$. But since both functions, by virtue of their continuity, must first go through all intermediate values before they can reach a higher value, there must be some *intermediate moment* at which they are both equal to one another.'—This is further illustrated by the example of the *motion* of two bodies, of which one is initially *behind* the other and later *ahead* of the other. It necessarily follows that at one time it must have been going *beside* the other.

No one will deny that the concepts of *time* and *motion* are just as foreign to general mathematics as that of *space*. Nevertheless, if these two concepts were only introduced here for the sake of *clarification* we would have nothing against them. For we are in no way party to such an exaggerated *purism* as demands, in order to keep the science free from everything alien, that in its exposition one can never use an *expression* borrowed from another field, even if only in a metaphorical sense and with the purpose of describing a fact more briefly and clearly than could be done by a strictly literal description: not even if it is used just to avoid the jarring of the constant repetition of the same word, or so as to remember, by the mere name given to a thing, an example which would serve to confirm the assertion. Thus it may be noted that we do not regard *examples* and *applications* as detracting in the least from the perfection of a scientific exposition. On the other hand, we strictly require only this: that examples never be put forward instead of *proofs*, and that the essence

* Compare on all this my *Beyträge zu einer begründeteren Darstellung der Mathematik, Ist edition*, Prague 1810. Section II §§2, 10, 20, 21, where the logical concepts, which I have assumed here as known, are developed further.

[Wesenheit] of a deduction never be based on the merely metaphorical use of phrases or on their accompanying ideas, so that the deduction itself would become void as soon as these were changed.

In accord with these views, the inclusion of the concept of *time* in the above proof may still perhaps be excused, because no conclusion is based on phrases which contain it, which would not also hold without it. But in no way can the last *illustration* about the *motion* of a body be viewed as anything more than a mere *example*, which does not prove the proposition but rather is only to be proved by it.

(a) So let us drop this example and examine the rest of the reasoning. Let us *first* notice that this is based on an incorrect concept of *continuity*. According to a *correct definition*, the expression *that a function fx varies according to the law of continuity for all values of x inside or outside 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* ω *can be taken as small as we please.*[b] With the notation I introduced in §14 of *Der Binomische Lehrsatz*, etc. (Prague 1816) this is, $f(x + \omega) = fx + \Omega$. But, as assumed in this proof, the continuous function is one which never reaches a higher value without first going through all lower values, i.e. $f(x + n\Delta x)$ can take every value between fx and $f(x + \Delta x)$ as n takes arbitrary values between 0 and 1. That is certainly a very *true* assertion, but it cannot be viewed as a *definition* of the concept of continuity: it is rather a *theorem* about continuity. Indeed, it is a theorem which can only be proved on the assumption of the proposition which here it was desired to prove by the theorem. For if M is some quantity between fx and $f(x + \Delta x)$, then the assertion that there is a value of n between 0 and 1 for which $f(x + n\Delta x) = M$ is only a special case of the general truth that if $fx < \phi x$, and $f(x + \Delta x) > \phi(x + \Delta x)$, then there must be some intermediate value $x + n\Delta x$ for which $f(x + n\Delta x) = \phi(x + n\Delta x)$. The first assertion comes from this general truth in the case when the function ϕx has constant value M.

(b) But even supposing one could prove this proposition in another way, the proof which we are examining would have yet another error. From the fact that $f\alpha > \phi\alpha$ and $f\beta < \phi\beta$ it would only follow that, if u was some value lying between α and β for which ϕu is $> \phi\alpha$ but $< \phi\beta$, then fx becomes equal to ϕu in going from $f\alpha$ to $f\beta$, i.e. for *some* x lying between α and β, $fx = \phi u$. But

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* There are functions which vary continuously for *all* values of their root, e.g. $\alpha x + \beta x$. [Probably a typesetter's misprint for $\alpha + \beta x$.] 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 $+2$.

[b] [There was no standard symbol at this time for absolute value or modulus. Bolzano always regards inequalities as applying to the magnitudes of each side regardless of sign. Thus $x > -1$ in Bolzano's usage would be expressed today by $x < -1$.]

whether this happens for *just the same* value of x which $= u$, i.e. (because u can be any arbitrary value lying between α and β which makes $\phi u > \phi\alpha$ and $< \phi\beta$) whether there is some value of x lying between α and β for which both functions fx and ϕx are equal to one another, this would still not follow.

(c) The deceptiveness of the whole proof rests mainly on the inclusion of the concept of *time*. For if this were omitted it would be seen straight away that the proof was nothing but a repetition in different words of the proposition to be proved. For to say that the function fx, before it passes from the state of being smaller than ϕx to that of being greater, must first go through the state of being equal to ϕx, is to say, without the concept of time, that among the values that fx takes if one puts in every value of x between α and β there is one that makes $fx = \phi x$. This is exactly the proposition to be proved.

III. Others prove our proposition on the basis of the following (either quite without proof or just supported by some examples borrowed from geometry): '*Every variable quantity can pass from a positive state to a negative one only through the state of being zero or infinity.*' Now since the value of an equation cannot be *infinitely large* for any finite value of the root, they conclude that any transition here must occur through *zero*.—

(a) If we wish to detach from the above proposition the metaphorical idea of a *transition*, which contains the concept of a change in *time* and *space*, thereby also omitting the senseless expression of *a state of non-existence*, then we eventually arrive at the following proposition: '*If a variable quantity which depends on another quantity x is positive for* $x = \alpha$, *and negative for* $x = \beta$, *there is always a value of x lying between* α *and* β *for which the quantity is zero or one for which it is infinite.*' Now everyone surely perceives that such a compound assertion is not a basic truth, but would have to be proved, and that its proof could hardly be easier than the very proposition which we wish to establish.

(b) Indeed, a closer examination shows that the assertion is fundamentally *identical* with the proposition. For it must not be forgotten that this assertion is actually true only if it refers to *quantities which vary continuously*. Thus for example, the function $x + \sqrt{(x-2)(x+1)}$ is *positive* for $x = +2$ and negative for $x = -1$, yet because it does not vary within these limits according to the law of continuity there is no value of x between 2 and -1 for which the function is zero or infinite. However, if we confine the assertion simply to quantities which vary continuously we must also exclude those functions which become *infinite* for a certain value of their root. For a function such as $\frac{a}{b-x}$ does not actually vary continuously for *all* values of x, but only for all values which are $>$ or $< b$. For the function does not have any *determinate* value when $x = b$, but it becomes what is called *infinitely great*. So it cannot be said that the values which it takes for $x = b + \omega$, all of which are *determinate*, can come

as close as desired to the value it has for $x = b$. And this is a part of the concept of continuity (II.a). Now let us add the concept of continuity to the above assertion while omitting the case of the function's *becoming infinite*. It then becomes, word for word, the original proposition we had to prove: namely that every continuously variable function of x which is positive for $x = \alpha$, and negative for $x = \beta$, must be zero for some value between α and β.

IV. In some works the following conclusion may be found: '*Because fx is positive for* $x = \alpha$ *and negative for* $x = \beta$, *there must be between* α *and* β, *two quantities a and b at which the transition from the positive values to the negative values of fx takes place, so that between a and b no more values of x occur for which fx would still be positive or negative.*' Etc.—This error scarcely needs refuting, and would not be introduced here if it did not serve to prove how unclear are the concepts of even some reputable mathematicians on this subject. It is well enough known that between any two nearby values of an *independent variable*, such as the root x of a function, there are always infinitely many *intermediate* values. And in the same way that there is no *last x* for a continuous function which makes it positive, and no *first x* which makes it negative, so there are no numbers a and b as here described!

V. The failure of these attempts to prove *directly* the proposition with which we are concerned, leads to the idea of deriving it from the *second* proposition which we mentioned at the beginning, namely from *the divisibility of every function into certain factors*. There is also no doubt, that if the latter is conceded, the former can be concluded from it. But the fact is that such a derivation could not be called a strictly scientific *justification*, in that the second proposition clearly expresses a much *more complex* truth than the first. The second can therefore certainly be based on the first, but not, conversely, the first on the second. No one has really succeeded yet in proving the second without presupposing the first. With regard to the proofs, the unacceptability of which *Gauss* has already shown in his paper of 1799, it is because they have already been shown to be unacceptable that it is unnecessary to investigate whether or not they are based on our present proposition. The proof of *Laplace*,* likewise, has its faults, which we need not point out here, because it is explicitly based on our present proposition. And in the same way, we need pay no regard to the *first* proof of *Gauss*, because it relies on *geometrical* considerations. Moreover, it would be easy to show that even in that proof our proposition is implicitly accepted, in that the geometrical considerations which are employed in it are quite similar to those which we mentioned in I.—So it all depends on the *Demonstratio nova altera* and *tertia* of *Gauss*. The former refers explicitly to our proposition, when it presupposes on p. 30: *aequationem*

* In the Journal de l'école normale, or also in *Lacroix, Traité du Calcul différéntiel et intégral*. T.I. no. 162, 163.

ordinis imparis certo solubilem esse.[c] An assertion which is well known to be nothing but an easy consequence of our proposition. It is not so obvious that the *Demonstratio nova tertia* depends on our proposition. It is based among other things on the following theorem: *If a function remains positive for all values of its variable quantity x which lie between* α *and* β*, then its integral taken from* $x = \alpha$ *to* $x = \beta$ *has a positive value.* Now in the proof given by *Lagrange*[†] for this theorem no explicit reference to our proposition is to be found. However, this proof of Lagrange still has a gap. It is required that the quantity i be taken sufficiently small that,

$$\frac{f(x+i)-fx}{i} - f'x < \frac{f'x + f'(x+i) + f'(x+2i) + \ldots + f'(x+(n-1)i)}{n}$$

where the product $i.n$ is to remain equal to a *given* quantity and the well-known notation $f'x$ represents the first derived function of fx. Now the question arises here whether it is possible to satisfy this requirement? However small one takes i, so as to diminish the difference $\frac{f(x+i)-fx}{i} - f'x$, the divisor of the right-hand side, n, must be taken that much greater if $i.n$ is to remain constant. Now also the set of terms in the numerator increases, but whether this increase in the numerator grows in proportion with the denominator, or whether the value of the whole fraction is decreased by the decrease in i, by as much as, or by more than, the expression $\frac{f(x+i)-fx}{i} - f'x$—this is yet to be shown. Now if this gap is to be filled, it can surely only be done with reference to our present proposition, since we had to refer to the latter for the proof of the theorem,* which although much simpler, is related to this one of *Lagrange*.

Thus all the proofs so far of the proposition which forms the title of this paper are defective. Now the one which I put forward here for the judgement of scholars contains, I flatter myself, not a mere *confirmation*, but the objective *justification* of the truth to be proved, i.e. it is strictly scientific.**

The following is a short summary of the method adopted.

The truth to be proved, that between any two values α and β which give results of opposite sign there always lies at least one real root, clearly rests on

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[c] ‖'an equation of odd degree is certainly soluble.'‖

[†] *Leçons sur le Calcul des fonctions. Nouvelle Edition, Paris 1806* Leç. 9, p. 89 ‖*Lagrange 1806*‖.

* Namely the proposition §29 in the paper: *Der Binomische Lahrsatz*, etc.

** Yet it is not to be expected that I comply here with *all* the rules which I myself set out in the *Beyträge zu einer begründeteren*, etc. (Pt. II) for the construction of a *strictly scientific* exposition. For though I am completely convinced of the correctness of these rules, to follow them precisely is only possible when one begins the exposition of a science from its *first* propositions and concepts, but not where one is only dealing with some theories taken out of the context of the whole. This observation obviously also applies to the paper *on the binomial theorem.*

the more general truth, that, if two continuous functions of x, fx and ϕx, have the property that for $x=\alpha$, $f\alpha<\phi\alpha$, and for $x=\beta$, $f\beta>\phi\beta$, there must always be some value of x lying between α and β for which $fx = \phi x$. However, if $f\alpha < \phi\alpha$, then by the law of continuity it is possible that $f(\alpha + i) < \phi(\alpha + i)$, if i is taken small enough. *The property of being smaller* therefore belongs to the function of i represented by the expression $f(\alpha + i)$, for all values smaller than a certain value. Nevertheless this property does not hold for *all* values of i without restriction, namely not for an i which is $= \beta - \alpha$, for $f\beta$ is already $> \phi\beta$. Now the *theorem* holds that whenever a certain property M belongs to all values of a variable quantity i which are smaller than a given value, and yet not for *all values in general*, then there is always some greatest value u, for which it can be asserted that all i which are $<u$ possess the property M. For this value of i itself $f(\alpha + u)$ cannot be $<\phi(\alpha + u)$ because then by the law of continuity $f(\alpha + u + \omega) < \phi(\alpha + u + \omega)$ if ω were taken small enough. And consequently it would not be true that u is the greatest of the values for which the assertion holds that all lower values of i make $f(\alpha + i) < \phi(\alpha + i)$; for $u + \omega$ would be a still greater value for which this holds. But still less can it be that $f(\alpha + u) > \phi(\alpha + u)$, for then also $f(\alpha + u - \omega) > \phi(\alpha + u - \omega)$ would be true if ω is taken sufficiently small, and consequently it would not be true that for all values of i which are $<u$, $f(\alpha + i) < \phi(\alpha + i)$. So therefore it must be that $f(\alpha + u) = \phi(\alpha + u)$, i.e. there is a value of x lying between α and β, namely $\alpha + u$, for which the functions fx and ϕx are equal to one another. It is now only a question of the proof of the *theorem* mentioned. This we prove by showing that those values of i for which it can be asserted that all smaller values possess property M and those of which this cannot be asserted can be brought as near one another as desired. Whence it follows for everyone who has a correct concept of *quantity*, that the idea of an i, which is the greatest one of which it can be said that all below it possess property M, is the idea of a real, i.e. *actual*, quantity.

Before I finish this preface I may be allowed to make a confession and a request which apply not only to the *present*, but to all my writings, also, God willing, to my *future* ones.

A careful reader could already have gathered from the few writings which have appeared *so far*, but particularly from that outline of a *new logic* which is supplied in the first issue of *Beyträge zu einer begründeteren Darstelling der Mathematik* in its *second* section, headed, '*Über die mathematische Methode*', that I hold certain views which, if they are not found to be completely incorrect, must lead to a *complete reorganization in all pure* a priori *sciences*. The greatest and most important part of these views I have already examined for such a long time and with so much impartiality, that it is not premature for me to venture to speak more openly about them. Views which embrace the whole domain of one or more sciences can be made known in two ways. They can either be stated once in a connected form or partially in single papers. The *first* way is, up till

now, by far the most usual, and it is without doubt the way any one must adopt if he just wants to achieve in the shortest time a reputation among the learned section of his contemporaries. But for the perfection of the sciences, I regard the *second* procedure to be much more advantageous for the following reasons:

Firstly, because in this way the discoverer of new views runs much less danger of being rushed. For the partial presentation of his opinions allows him to postpone to a later time his explanation of points on which he was initially in doubt himself. He can learn from the criticisms which work already published receives, and still correct some things which had been incorrect.

Secondly, with such a serial development of his views, he can also expect a far stricter examination on the part of the reader. For whoever presents an already completed system, offers for our attention all at once, a larger number of new assertions than we could hope anyone to examine with as much care as if they had been presented singly. Whoever supplies a complete theory, shows, or at least, should show, how those truths which common sense knows with undeniable certainty can be derived from his *unusual* premisses. But this reconciles us to those premisses straight away, and we concede to them much more rapidly than if he had presented them singly and had allowed us to be in doubt as to whether, and to what extent, they agree with the rest of what we hold to be true. Finally, it is surely not to be denied that the mere sight of a *large*, *thick book* which promises a complete system of this or that science, instils into us a kind of reverence before we have even read it. Now if we discover, through reading it, a certain coherence in the assertions, if the structure of human knowledge which is sketched out here has a pleasing form, if everything is laid out according to size and number and symmetry, then our judgement is affected, and we even begin to wish that here, at last, might be *that single correct system* which we have sought for so long. And the least that occurs is that because of the coherence observed, we imagine that we are only free either to accept or reject the entire system, when in fact neither the one nor the other should happen!

These were the main reasons for which I decided in 1804 not to begin in any science with the publication of a *complete textbook*, but to first make known these unusual concepts of mine in single papers. And if, after much correction, these have found favour with a part of the public, only then should the preparation of an entire system be considered. That is, unless death does not oblige us to leave this latter task to others.

I began my written output with a paper concerning *mathematics* under the title, *Betrachtungen über einige Gegenstände der Elementargeometrie* (Prague, C. Barth, 1804). In this I put forward, as well as several other views, *a new theory of parallels.** Some years later I resolved to publish all my views in the area

* This theory might deserve attention for at least two reasons: *firstly*, because it is the only one in which no obvious error has been detected; *then* because the greatest living French geometer, *Legendre*, hit upon just the same view of things quite independently of me in the *tenth* edition of his *Elémens de Géométrie*, Paris, 1813 |cf. *Legendre 1794*|

of *mathematics* in serial form under the title, *Beyträge zu einer begründeteren Darstellung der Mathematik*. But the *first* of these issues, (Prague, C. Widtmann, 1810) 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. For this purpose there appeared in 1816 the paper already mentioned, *Der binomische Lehrsatz*, etc. (Prague, Enders) [*Bolzano 1816*]. My wish is that the present paper should also serve this purpose; besides, its publication was necessary because in that earlier paper I already referred to the proposition proved here. Some *other* papers are also written out ready for printing, for example one with the following title, *Die drey Probleme der Rectification, der Complanation und der Cubirung, ohne Betrachtung des unendlich Kleinen, ohne die annahmen des Archimedes und ohne irgend eine nicht streng erweisliche Voraussetzung gelöst*. These still await their publisher.[d]

If I should continue this way, which seems to me the most advantageous, then the only *favour* of the public for which I must *ask* is that this single paper be not overlooked on account of its small extent, but rather examined with all possible strictness, and the results of this examination made known publicly. So that what is perhaps unclear may be clearly explained and what is quite incorrect will be retracted. The sooner truth and correctness gain general assent, the better.

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§1.

Convention. Suppose that for a *series of quantities* the special case does not occur that all the terms after a certain term are *zero*, as happens for example in the *binomial series* after the $(n+1)$th term for every positive integer exponent n. Then it is obvious that the *value of this series*, that is the quantity resulting from summing its terms, cannot always remain the same if the set of terms is arbitrarily increased. On the contrary, this value must certainly change every time the number of terms is increased by a *single one* which is not zero. Hence the value of a series depends not only on the *rule* which determines the construction of the individual terms but also on their *number*. So the value represents a *variable quantity*, even though the *form* and *magnitude* of the individual terms remain unchanged. With this in mind, we denote a *function* of x, which consists of an arbitrarily long series of terms, and whose value therefore depends, apart from x, on the *number of terms*, r, by $\overset{(r)}{F}(x)$ or $\overset{r}{F}x$. So, for example, $A + Bx + Cx^2 + \ldots + Rx^r = \overset{r}{F}x$, and on the other hand $A + Bx + Cx^2 + \ldots + Rx^r + \ldots + Sx^{r+s} = \overset{(r+s)}{F}x$.

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[d] [The paper quoted was published later in 1817 in Leipzig. One of the others referred to must have been *Versuch einer Begründung der Lehre von den drey Dimensionen des Raumes*, published in 1845, but according to its Preface already written in 1815.]

§2.

1st Lemma. The *change in value*, i.e. the *increase* or *decrease* in value, which occurs in a series by increasing its number of terms by a *definite set* (for example by one) can be, according to circumstances, a *constant* quantity (if, namely, the terms of the series are all equal), but also may be variable. In the latter case, the change may be a quantity which increases for a while and decreases for a while, or one which increases or decreases steadily. Thus the change in the series

$$1 + 1 + 1 + 1 + 1 + 1 + \ldots$$

if it is increased by *one* term is a *constant* quantity. The change in the series

$$a + ae + ae^2 + ae^3 + \ldots$$

by the increase of *one* term is a variable quantity, provided e is not $= 1$. It becomes ever larger if $e > \pm 1$, and ever smaller if $e < \pm 1$.

§3.

2nd Lemma. If the change (*increase* or *decrease*) in a series due to the increase in its set of terms by a definite number (for example one) always remains the *same* or even always increases—and if in both cases it retains the *same sign*—then it is clear that the *value* of this series will become *greater than any given quantity* if it is continued far enough. For suppose the growth in the series by the increase of every n terms is $=$ or $> d$ and it is required to make the series larger than a given value D. Then one need only take a whole number r which is $=$ or $> D/d$ and extend the series by $r.n$ terms, thereby obtaining an increase which is

$$= or > (r.d = or > (D/d).d = D).$$

§4.

3rd Lemma. But there are also series whose value, however far they may be continued, *never exceeds a certain quantity.* Of this kind is the series,

$$a - a + a - a + \ldots$$

whose value, however far it is continued, is always either 0 or a, and therefore never exceeds a.

§5.

4th Lemma. Particularly interesting among such series is the class of those series having the property that the *change* in value (*increase* or *decrease*) *due to any further continuation* of terms, always remains *smaller* than a certain quantity, which itself can be taken *as small as desired* provided the series has been continued far enough beforehand. That there *are* such series is proved not only by

those whose terms after a certain point are all *zero* (and which therefore really have no continuation *after* this term, and are no more capable of changing value than the *binomial series* of §1); but also by the fact that also of this kind are all series in which the terms decrease at the same rate as, or faster than, the terms of a *geometric progression* with ratio a proper fraction. For the value of the geometric series,

$$a + ae + ae^2 + \ldots + ae^r$$

is well known to be $= a.\frac{1 - e^{r+1}}{1 - e}$. And if this series is extended by s terms, then the increase is

$$ae^{r+1} + ae^{r+2} + ae^{r+3} + \ldots + ae^{r+s} = ae^{r+1}.\frac{1 - e^s}{1 - e}.$$

Now if $e < \pm 1$ and r is taken sufficiently large, then this increase remains smaller than any given quantity, however large s becomes. For because e^s always remains $< \pm 1$ then $ae^{r+1}.\frac{1 - e^s}{1 - e}$ is obviously always *smaller* than $ae^{r+1}.\frac{2}{1 - e}$. But this latter can be made smaller than any given quantity by increasing r, because the value it takes for the next larger value of r is just the previous result multiplied by e, a constant proper fraction (see *Der binomische Lehrsatz*, §22). Therefore every geometric progression whose ratio is a proper fraction can be continued so far that the increase caused by every further continuation must remain smaller than some given quantity. So much more must this hold for series whose terms decrease more rapidly than those of a decreasing geometric progression.

§6.

5th Lemma. If the values of the sums of the first n, $n + 1$, $n + 2$, ..., $n + r$ terms of a series like those of §5 are denoted (§1) respectively by $\overset{n}{F}x$, $\overset{n+1}{F}x$, $\overset{n+2}{F}x$, ..., $\overset{n+r}{F}x$, then we regard the quantities

$$\overset{1}{F}x, \overset{2}{F}x, \overset{3}{F}x, \ldots, \overset{n}{F}x, \ldots, \overset{n+r}{F}x, \ldots$$

as a *new* series (called the *series of sums* of the previous one). The assumption has been made in this of the special property that the difference between its nth term $\overset{n}{F}x$ and every later term $\overset{n+r}{F}x$ (no matter how far from that nth term) stays smaller than any given quantity if n has been taken large enough in the first place. This difference is the increase to the *original* series by the continuation beyond the nth term, and this increase can remain, by assumption, as small as desired if n has been taken large enough in the first place.

§7.

Theorem. If a series of quantities

$$\overset{1}{F}x,\ \overset{2}{F}x,\ \overset{3}{F}x,\ \ldots,\ \overset{n}{F}x,\ \ldots,\ \overset{n+r}{F}x,\ \ldots$$

has the property that the difference between its nth term $\overset{n}{F}x$ and every later one $\overset{n+r}{F}x$, 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.

Proof. That such a series as the theorem describes is possible is clear from §6. The hypothesis that there is the quantity X which the terms of this series approach as close as desired through ever further continuation certainly contains nothing impossible provided it is not assumed that this quantity be *unique* and *invariable.* For if it is a quantity which can vary then it can, of course, always be taken so that it is suitably near the term $\overset{n}{F}x$ with which it is being compared—even exactly the same as it. But the assumption of an *invariable* quantity with this property of proximity to the terms of our series contains no impossibility, because with this assumption it is possible to determine the quantity as accurately as desired. For suppose it is required to determine X so accurately that the difference from the true value of X does not exceed a small given quantity d. Then one simply looks in the given series for a term $\overset{n}{F}x$ with the property that every term $\overset{n+r}{F}x$ following it has a difference less than $\pm d$. There must be such an $\overset{n}{F}x$ by assumption. I now say the value of $\overset{n}{F}x$ differs from the true value of X by at most $\pm d$. For if r is increased arbitrarily, with the same n, the difference $X - \overset{n+r}{F}x = \pm\omega$ can be made as small as desired, but the difference $\overset{n}{F}x - \overset{n+r}{F}x$ always remains $< \pm d$, however large r is taken. Therefore the difference,

$$X - \overset{n}{F}x = (X - \overset{n+r}{F}x) - (\overset{n}{F}x - \overset{n+r}{F}x)$$

must always remain $< \pm(d + \omega)$. But since for the same n this is a *constant* quantity, and ω can be made as small as required by increasing r, then $X - \overset{n}{F}x$ must be $=$ or $< \pm d$. For if it were *greater*, for example $= \pm(d + e)$, it would be impossible for the relation $d + e < d + \omega$, i.e. $e < \omega$, to hold if ω is further reduced. The true value of X therefore differs from the value which the term $\overset{n}{F}x$ has, by at most d, and can thus be determined as accurately as required, since d can be made arbitrarily small. There *is* therefore a *real quantity* which the terms of the series approach as closely as desired if it is continued far enough. But there is only *one* such quantity. For suppose that besides X there

is another *constant* quantity Y which the terms of the series approach as close as desired if it is continued far enough, then the differences $X - \overset{n+r}{F}\, x = \omega$ and $Y - \overset{n+r}{F}\, x = \overset{1}{\omega}$, can be made as small as desired if r is taken large enough. Therefore this also holds for their own difference, i.e. for $X - Y = \omega - \overset{1}{\omega}$, which, if X and Y are *constant* quantities, is impossible unless one assumes $X = Y$.

§8.

Remark. If one tries to determine the value of the quantity X in the way described in the previous paragraph, namely through one of the terms from which the given series is composed, then one will never determine X completely *accurately* unless the terms of this series from some point on are all equal. But one must beware of concluding from this that the quantity X is always *irrational*. For if we consider the series

$$0.1;\ 0.11;\ 0.111;\ 0.1111;\ \ldots$$

(which is the series of sums of the geometrical progression

$$1/10;\ 1/100;\ 1/1000;\ 1/10\,000;\ \ldots)$$

then the quantity which the terms approacn as ciose as desired is not irrational, but the fraction 1/9. So from the fact that a quantity cannot be determined accurately in *a certain way*, it does not follow that it cannot be completely determined in any *other* way, and is therefore *irrational*.

§9.

Lemma. If therefore some given series has the property that every single term is *finite*, but the change which it undergoes on every further continuation is smaller than any given quantity, provided only that the number of terms taken in the first place is large enough, then there is always one and only *one constant quantity* which comes as close to the value of this series as desired if it is continued far enough. For such a series is of the kind described in §5, and hence the values which are the sums of the n, $n + 1$, $n + 2$, ... terms form a series like those of §6 and §7: therefore such a series has also the property proved in §7.

§10.

Remark. It is not to be thought that in the above proposition of §9 the condition, '*that the change*, (increase or decrease) *which the series undergoes on every continuation must remain smaller that any given quantity if it has been continued far enough beforehand*', is superfluous, and that the proposition could perhaps be expressed with greater generality: '*If it is possible to make the terms*

of a series when continued ever smaller and as small as desired, then there is always a constant quantity which the value of the series, when continued, approaches as near as desired.' This assertion would be contradicted immediately by the following example. The terms of the series

$$1/2 + 1/3 + 1/4 + 1/5 + \ldots$$

can be made as small as desired, and yet it is a well-known truth, from the properties of a *rectangular hyperbola* (but also derivable from purely arithmetic considerations), that the value of this series can become greater than any given quantity if it is continued far enough.

§11.

Preliminary note. In investigations of applied mathematics the case often arises that one learns that a definite property M belongs to *all* values of a *variable quantity* x which are *smaller* than a certain u, without at the same time knowing whether this property does or does not belong to all values which are *greater* than u. In such a case there may perhaps be some u' that is $> u$ for which, in the same way as it holds for u, all values of x lower ‖than u'‖ possess property M. This property M may even belong to *all values of x without exception*. But if *this* is known, *that M does not belong to all x in general*, then by combining these two conditions it will be correct to conclude: *there is a certain quantity U which is the greatest of those for which it is true that all smaller values of x possess property M*. This is proved in the following theorem.

§12.

Theorem. If a property M does not belong to *all* values of a variable x but does belong to *all* values which are *smaller* than a certain u, then there is always a quantity U which is the greatest of those of which it can be asserted that all smaller x have property M.

Proof. 1. Because the property M holds for all x *smaller* than u but not for *all x in general* there is some quantity $V = u + D$ (where D represents something positive) for which it can be said that M does not belong to all x which are $< V = u + D$. If I then ask the question, whether *M belongs to all x which are* $< u + D/2^m$ where the exponent m is first 0, then 1, then 2, then 3, etc.: I am sure the *first* of my questions will *have to be denied*. For the question whether M belongs to all x which are $< u + D/2^0$ is the same as whether M belongs to all x which are $< u + D$, which is denied by assumption. So it is a matter of whether all the *succeeding* questions as m is put larger and larger will be denied. Should this be the case, it is clear that u *itself* is the greatest value for which the assertion holds that all smaller x have property M. For if there were a greater, for example $u + d$, the assertion would hold that all x which are $< u + d$ have the property M; but then it is clear that if I take m large enough,

$u + D/2^m$ will at one time be $=$ or $< u + d$, and consequently if M belongs to all x which are $< u + d$, it also belongs to all x which are $< u + D/2^m$; therefore we would not have denied this question, but must have affirmed it. Thus it is proved that in this case (when all the above questions are denied) there is a certain quantity U (namely u itself) which is the greatest for which the assertion holds that all x below it possess the property M.

2. However, if one of the above questions is *affirmed* and m is the definite value of the exponent for which it is *first* affirmed (m can be 1 but, as we have seen, not 0), then I now know that the property M belongs to all x which are $< u + D/2^m$ but not to all x which are $< u + D/2^{m-1}$. But the difference between $u + D/2^m$ and $u + D/2^{m-1}$ is $= D/2^m$. If I therefore do the same with this as before with the difference D, i.e. I ask the question whether M belongs to all x which are $< u + D/2^m + D/2^{m+n}$, and here the exponent n is first 0, then 1, then 2, etc.: then I am again sure that at least the *first* question will have to be denied. For to ask whether M belongs to all x which are $< u + D/2^m + D/2^{m+0}$ is just the same as to ask whether M belongs to all x which are $< u + D/2^{m-1}$, which had already been denied. But if all my *succeeding* questions are denied as I make n larger and larger, then as before $u + D/2^m$ is that greatest value, or the U, for which the assertion holds that all x below it possess the property M.

3. However, if one of these questions is affirmed and this happens first for the definite value n, then I now know M belongs to all x which are $< u + D/2^m + D/2^{m+n}$, but not to all x which are $< u + D/2^m + D/2^{m+n-1}$. The difference between these two quantities is $D/2^{m+n}$, and I repeat with this as before with $D/2^m$, etc.

4. If I continue this way as long as desired it may be seen that the result that I finally obtain must be one of two things.

(a) I find a value of the form $u + D/2^m + D/2^{m+n} + \ldots + D/2^{m+n+\ldots+r}$ which is the greatest for which the assertion holds that all x below it possess a property M. This happens in the case when the questions whether M belongs to all x which are

$$< u + D/2^m + D/2^{m+n} + \ldots + D/2^{m+n+\ldots+r+s}$$

are denied for every value of s.

(b) I find that M belongs to all x which are

$$< u + D/2^m + D/2^{m+n} + \ldots + D/2^{m+n+\ldots+t}$$

but not to all x which are

$$< u + D/2^m + D/2^{m+n} + \ldots + D/2^{m+n\ldots+r-1}$$

Here I am always free to make the number of terms in these two quantities even greater through new questions.

5. Now if the *first* case occurs the truth of the theorem is already proved. In the *second* case it may be noticed that the quantity

$$u + D/2^m + D/2^{m+n} + \ldots + D/2^{m+n\ldots+r}$$

represents a series whose number of terms I can increase arbitrarily, and which belongs to the class described in §5. Because, according as $m, n, \ldots, r$ are either all $= 1$, or some are greater than 1, it decreases at the same rate, or more rapidly than, a geometric progression whose ratio is the proper fraction $\frac{1}{2}$. From this it follows that it has the property of §9, i.e. there is a certain *constant quantity* which it can approach as near as desired if the set of its terms is sufficiently increased. Let this quantity be U; then I claim the property M holds for all x which are $< U$. For if it did not hold for some x that is $< U$, for example for $U - \delta$, then the quantity

$$u + D/2^m + D/2^{m+n} + \ldots + D/2^{m+n+\ldots+r},$$

must always keep the distance δ from U, because for all x that are smaller than it, the property M should apply. For every x that is

$$= u + D/2^m + D/2^{m+n} + \ldots + D/2^{m+n+\ldots+r} - \omega,$$

however small ω is, possesses the property M. On the other hand, M does not belong to the $x = U - \delta$, therefore,

$$U - \delta > u + D/2^m + D/2^{m+n} + \ldots + D/2^{m+n+\ldots+r} - \omega$$

or

$$U - [u + D/2^m + D/2^{m+n} + \ldots + D/2^{m+n+\ldots+r}] > \delta - \omega.$$

Hence the difference between U and the series cannot be made as small as desired, since $\delta - \omega$ cannot be made smaller than any given quantity.—But just as little can M hold for all x which are $< U + \varepsilon$. For the value of the series

$$u + D/2^m + D/2^{m+n} + \ldots + D/2^{m+n+\ldots+r-1}$$

can be brought as close to the value of the series

$$u + D/2^m + D/2^{m+n} + \ldots + D/2^{m+n+\ldots+r}$$

as desired because the difference between the two is only $D/2^{m+n+\ldots+r}$. Further, because the value of the latter series can be brought as close as desired to U, so also can the value of the first series come as close to U as desired. Therefore

$$u + D/2^m + D/2^{m+n} + D/2^{m+n+\ldots+r-1}$$

can certainly become $< U + \varepsilon$. But now by assumption M does not hold for all x which are $< u + D/2^m + D/2^{m+n} + \ldots + 2^{m+n+\ldots r-1}$; so much less therefore does M hold for all x which are $< U + \varepsilon$. Therefore U is the greatest value for which the assertion holds that all x below it possess the property M.

§13.

1st Remark. The last theorem is of the greatest importance, and is used in all areas of mathematics, as much in analysis as in the applied parts, in geometry, chronometry, and mechanics. Not infrequently in the past this false proposition has served instead: '*If a property M holds not for all x but for all smaller than a certain value, then there is always some greatest x to which the property M belongs.*' This I say is *false* in consequence of the theorem just proved. For if there is some quantity U which is the greatest of those of which it can be said that all x below them have property M then there is *no greatest x* to which this property belongs *provided x is either a freely or a continuously variable quantity.* For it is well known that for a quantity that varies *freely or according to the law of continuity* there is never a *greatest* value that is smaller than a certain limit U, because however close it may be to this limit it can always be brought closer.—In order to illustrate this by an example, consider a *rectangular hyperbola*, and take one of its asymptotes as the x-axis and take the origin, not at the centre c, but at another point a on this asymptote which is at a distance D from c. Now let us define the direction ac as the *positive x-axis* and the direction ab which is the *perpendicular* ordinate at a as the positive y-axis. Then *every* x-coordinate which is *smaller* than a certain one, say smaller than $D/2$, has the property that *its corresponding ordinate is positive.* However, this property (M) will not hold for *all* positive x-coordinates, namely not for those which are greater than D. Now is there here a *greatest* x-coordinate, or a greatest value of x, to which the property M belongs? In no way, but there is certainly a U, i.e. an x-coordinate which is the greatest among those of which it can be said that all smaller than it have positive ordinates, i.e. possess the property M. This x-coordinate is $+D$.

§14.

2nd Remark. Perhaps someone might have the idea that the proof of the theorem in §12 could have been accomplished quite briefly in the following way: 'If there were no greatest U of which the assertion holds that all x below it possess the property M, one would always be able to take *u greater and greater* and so as great as one desired and consequently M must hold for *all x without exception.*'—However, this would be a very mistaken conclusion, because it is based on the concealed assumption: '*that a quantity that can always be taken greater than it already is can become as great as desired*'.—How false this is may be shown, for example, by the well known series $\frac{1}{2} + \frac{1}{4} + \frac{1}{8} + \ldots$, whose value can always be made greater than it already is and nevertheless always remains < 1!—We would not even mention such an easily discernible error if it did not happen from time to time that mathematicians are guilty of it—as one was only recently in his '*complete theory of parallels*'.

§15.

Theorem. If two functions of x, fx and ϕx, vary *according to the law of continuity* either for all values of x or just for all which lie between α and β, and

if $f\alpha < \phi\alpha$ and $f\beta > \phi\beta$ then there is always a certain value of x between α and β for which $fx = \phi x$.

Proof. We must remember that in this theorem the values of the functions fx and ϕx are to be compared with one another simply in their *absolute* values, i.e. without regard to signs or as though they were quantities incapable of being of opposite signs. But the signs which α and β have are important.

I. 1. Firstly assume that α and β are both positive and that (without loss of generality) β is the *greater* of the two, so $\beta = \alpha + i$, where i denotes a positive quantity. Now because $f\alpha < \phi\alpha$, if ω denotes a positive quantity which can be taken as small as desired, then also $f(\alpha + \omega) < \phi(\alpha + \omega)$. For because fx, ϕx vary continuously for all x between α and β, and $\alpha + \omega$ lies between α and β whenever $\omega < i$, then $f(\alpha + \omega) - f\alpha$ and $\phi(\alpha + \omega) - \phi\alpha$ must be able to be made as small as desired if ω is taken small enough. Hence if Ω and Ω' denote quantities which can be made as small as desired, $f(\alpha + \omega) - f\alpha = \Omega$, and $\phi(\alpha + \omega) - \phi\alpha = \Omega'$. Hence,

$$\phi(\alpha + \omega) - f(\alpha + \omega) = \phi\alpha - f\alpha + \Omega' - \Omega.$$

But $\phi\alpha - f\alpha$ equals, by assumption, some positive quantity of constant value A. Therefore,

$$\phi(\alpha + \omega) - f(\alpha + \omega) = A + \Omega' - \Omega,$$

which remains positive if Ω and Ω' are taken small enough, i.e. if ω is given a very small value, and this also holds for all smaller values. Therefore for *all* values of ω which are *less* than a certain one it can be asserted that the two functions $f(\alpha + \omega)$ and $\phi(\alpha + \omega)$ stand in the relation of smaller quantity to larger. Let us designate this property of the variable ω by M. Then we can say that *all* ω that are smaller than a certain value possess the property M. But nevertheless it is clear that this property M does not belong to all values of ω, namely not for the value $\omega = i$, because $f(\alpha + i) = f\beta$ which by assumption is not $<$ but $> \phi(\alpha + i) = \phi\beta$. Consequently the theorem of §12 gives a certain U which is the greatest of those for which it can be asserted that all ω which are $< U$ have the property M.

2. This U must lie *between* 0 and i. For *firstly* it cannot be $= i$, because this would mean $f(\alpha + \omega) < \phi(\alpha + \omega)$, whenever $\omega < i$, and however near it comes to the value i. But in exactly the same way that we have just proved that from the assumption $f\alpha < \phi\alpha$ the consequence $f(\alpha + \omega) < \phi(\alpha + \omega)$ may be drawn if ω is taken small enough, so one can also prove that from the assumption $f(\alpha + i) > \phi(\alpha + i)$, the consequence $f(\alpha + i - \omega) > \phi(\alpha + i - \omega)$ follows if ω is taken small enough. Therefore it is not true that the two functions fx and ϕx stand in the relation of smaller to greater for all values of x which are $< \alpha + i$.—*Secondly*, still less can it be the case that $U > i$, because then i would be one of the values of ω which are $< U$, and hence also $f(\alpha + i) < \phi(\alpha + i)$, which directly contradicts the assumption of the theorem. Therefore, since it is *positive*, U surely lies between 0 and i, and consequently $\alpha + U$ lies between α and β.

3. It may now be asked what relation holds between fx and ϕx for the value $x = \alpha + U$? In the *first* place, it cannot be that $f(\alpha + U) < \phi(\alpha + U)$, for this would also give $f(\alpha + U + \omega) < \phi(\alpha + U + \omega)$ if ω is taken small enough, and so $\alpha + U$ would not be the greatest value for which it can be asserted that all x below it have the property M.—Just as little, *secondly*, can it be that $f(\alpha + U) > \phi(\alpha + U)$, because this would also give $f(\alpha + U - \omega) > \phi(\alpha + U - \omega)$ if ω is taken small enough, and would therefore be against the assumption that the property M is preserved for all x which are below $\alpha + U$. There therefore remains nothing else than that $f(\alpha + U) = \phi(\alpha + U)$, and so it is proved that there is a value of x lying between α and β, namely $\alpha + U$, for which $fx = \phi x$.

II. The same proof is also applicable to the case when α and β are both *negative* if one takes ω, i, and U as negative quantities, because then in the same way $\alpha + \omega$, $\alpha + i$, $\alpha + U$, $\alpha + U - \omega$ represent quantities between α and β.

III. If $\alpha = 0$ and β is *positive* then just take $i(=\beta)$, ω, U positive; and if β is *negative* take these others negative and the proof (I). can be used word for word.

IV. Finally, if α and β are of opposite sign and (without loss of generality) for example α is negative and β positive, then the assumption of the theorem in respect of the continuity of the functions fx and ϕx states that this continuity refers to all values of x which, if negative, are $<\alpha$, and, if *positive* are $<\beta$. Then among these is the value $x=0$. One therefore investigates what relationship holds between $f(0)$ and $\phi(0)$. If $f(0) = \phi(0)$ then the theorem is already proved. But if $f(0) > \phi(0)$, then since $f\alpha < \phi\alpha$ we have by III. a value between 0 and α, and if $f(0) < \phi(0)$ a value between 0 and β, for which $fx = \phi x$. Therefore in every case there is a value of x between α and β which makes $fx = \phi x$.

§16.

Remark. It is in no way claimed here that there is *only a single value* of x which makes $fx = \phi x$. Namely, if $f\alpha < \phi\alpha$ and $f(\alpha + U) = \phi(\alpha + U)$ we must indeed have $f(\alpha + U + \omega) > \phi(\alpha + U + \omega)$ if ω is taken small enough, i.e. the function fx which before was *smaller* than ϕx must, after they are *equal* to one another, soon become greater than ϕx. However, with ever *greater* increase in ω it is certainly possible that before $\alpha + U + \omega$ is made $= \beta$, there is a value for which *again* $fx < \phi x$. In such a case, it follows directly from our theorem, that there must be two other values of x apart from U, between α and β which make $fx = \phi x$. For if $f(\alpha + U + \chi) < \phi(\alpha + U + \chi)$, then because $f(\alpha + U + \omega)$ was already $> \phi(\alpha + U + \omega)$, there must be a value of x between $\alpha + U + \omega$ and $\alpha + U + \chi$, i.e. between α and β, for which $fx = \phi x$. And in the same way, because $f(\alpha + i)$, or $f\beta$, is again $> \phi\beta$, there is also a value of x between $\alpha + U + \chi$ and β which makes $fx = \phi x$. In this way it

becomes clear that there must in general always be *an odd number* of values of x which make $fx = \phi x$.

§17.

Theorem. Every function of the form $a + bx^m + cx^n + \ldots + px^r$, in which $m, n, \ldots, r$ designate whole positive exponents, is a quantity which varies *according to the law of continuity* for all values of x.

Proof. If x changes to $x + \omega$ the change in the function is clearly

$$=b[(x+\omega)^m - x^m] + c[(x+\omega)^n - x^n] + \ldots + p[(x+\omega)^r - x^r]$$

—a quantity for which it can easily be shown that it can be made as small as desired if ω is taken small enough. For by the *binomial theorem*, whose validity for whole positive powers (§8 of *Der binom. Lehrs.*) is independent of the investigations with which the present paper is concerned, this quantity is

$$= \omega \left\{ \begin{array}{l} mbx^{m-1} + m.\dfrac{m-1}{2} bx^{m-2}\omega + \ldots + b\omega^{m-1} \\ + ncx^{n-1} + n.\dfrac{n-1}{2} cx^{n-2}\omega + \ldots + c\omega^{n-1} \\ + \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots \\ + rpx^{r-1} + r.\dfrac{r-1}{2} px^{r-2}\omega + \ldots + p\omega^{r-1} \end{array} \right\}$$

The set of terms, of which the factor contained in the brackets consists, is known to be always *finite* and *independent* of the values of the quantities x and ω. Since these occur only with *positive powers* the value of every single term, and consequently the whole expression, for every value of x (also for $x = 0$) is always *finite*. But if for the same x, the value of ω is diminished, then the terms in which ω appears decrease, while the others remain unchanged. Let us therefore designate by S the quantity arrived at by putting a definite value, say $\overset{1}{\omega}$, for ω in all the single terms of the expression, then summing as if they all had the same sign. Then the actual value this expression has for the same $\overset{1}{\omega}$ is certainly not $> S$, but that which it takes for every *smaller* ω is surely $< S$. Hence if it is required to make the change which the function $a + bx^m + cx^n + \ldots + px^r$ undergoes $< D$, then take an ω that is $< \overset{1}{\omega}$ and also $< D/S$. Then $\omega.S$, and even more so the product of ω with a quantity which is $< S$, must be $< D$.

§18.

Theorem. If a function of the form

$$x^n + ax^{n-1} + bx^{n-2} + \ldots + px + q,$$

in which n denotes a whole positive number, is *positive* for $x = \alpha$ and *negative* for $x = \beta$ then the equation

$$x^n + ax^{n-1} + bx^{n-2} + \ldots + px + q = 0$$

has at least one *real root* lying between α and β.

Proof. 1. If α and β both have the same sign (either both positive or both negative) then it is clear that just the same terms of the function which are positive or negative for $x = \alpha$ keep this sign also for $x = \beta$ and for all values of x which lie in between α and β. Suppose now the value of the function is positive for $x = \alpha$ but negative for $x = \beta$. This change can only arise because the sum of the positive terms in it turn out *greater* than that of the negative ones for $x = \alpha$ but *smaller* than that of the negative ones for $x = \beta$. But the sum of the former, as well as of the latter, is of the form

$$a + bx^m + cx^n + \ldots + px^r$$

of §17, i.e. a continuous function. Let us therefore designate the one by ϕx and the other by fx. Then because $f\alpha < \phi\alpha$ and $f\beta > \phi\beta$, *by* §15 there must be some value of x lying between α and β for which $fx = \phi x$. But for this value $fx - \phi x$, i.e. the given function, is zero; therefore this value is a root of the equation

$$x^n + ax^{n-1} + bx^{n-2} + \ldots + px + q = 0$$

2. But if α and β are of opposite sign, consider the value the given function takes for $x = 0$. If this is zero, then this already shows that the given equation has a real root lying between α and β, namely $x = 0$. But if this value (the quantity q) is *positive* then it is now known that the given function is positive for $x = 0$ but negative for $x = \beta$, and because the same terms which are positive or negative for $x = \beta$ retain these signs also for all values lying between 0 and β one can prove, through the same arguments as in no. 1, that there must be a value of x lying between 0 and β which makes the function zero. Finally, if q is *negative*, then what we have just said holds if α is put instead of β. Now since a value lying between 0 and β or between 0 and α also lies between α and β, if they are of opposite sign, then the truth of our theorem is proved for every case.