The Rise of non-Archimedean Mathematics and the Roots of a Misconception I: The Emergence of non-Archimedean Systems of Magnitudes[1,2]

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> As a matter of fact, it is by no means impossible to build up a consistent "non-Archimedean" theory of magnitudes in which the axiom of Eudoxus (usually named after Archimedes) does not hold.
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> Hermann Weyl
> *Philosophie der Mathematik und Naturwissenschaft*
> 1927, p. 36

Introduction

In his paper *Recent Work On The Principles of Mathematics*, which appeared in 1901, Bertrand Russell reported that the three central problems of traditional mathematical

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Throughout the text we follow the convention of only providing the original German, French and Italian texts in the cases of translations of substantial quotations.

[2] The companion to the present paper referred to in the text is a continuation of the present work that will focus on the emergence of non-Archimedean geometry. It is the author's intention to submit the latter paper for publication in the Archive in the not too distant future.

philosophy – the nature of the infinite, the nature of the infinitesimal, and the nature of
the continuum – had all been "completely solved" [1901, p. 89]. Indeed, as Russell went
on to add: "The solutions, for those acquainted with mathematics, are so clear as to leave
no longer the slightest doubt or difficulty" [1901, p. 89]. According to Russell, the struc-
ture of the infinite and the continuum were completely revealed by Cantor and Dedekind,
and the concept of an infinitesimal had been found to be incoherent and was "banish[ed]
from mathematics" through the work of Weierstrass and others [1901, pp. 88, 90]. These
themes were reiterated in Russell's often reprinted *Mathematics and the Metaphysician*
[1918],[3] and further developed in both editions of Russell's *The Principles of Mathe-
matics* [1903; 1937], the works which perhaps more than any other helped to promulgate
these ideas among historians and philosophers of mathematics. In the two editions of the
latter work, however, the banishment of infinitesimals that Russell spoke of in 1901 was
given an apparent theoretical urgency. No longer was it simply that "nobody could dis-
cover what the infinitely little might be," [1901, p. 90] but rather, according to Russell, the
kinds of infinitesimals that had been of principal interest to mathematicians were shown
to be either "mathematical fictions" whose existence would imply a contradiction [1903,
p. 336; 1937, p. 336] or, outright "self-contradictory," as in the case of an infinitesimal
line segment [1903, p. 368; 1937, p. 368]. In support of these contentions Russell could
cite no less an authority than Georg Cantor, the founder of the theory of infinite sets.[4]

Having accepted along with Russell that infinitesimals had indeed been shown to be
incoherent, and that (with the possible exception of constructivist alternatives) the nature
of the infinite and the continuum had been essentially laid bear by Cantor and Dedekind,
following the development of nonstandard analysis in 1961, a good number of historians
and philosophers of mathematics (as well as a number of mathematicians and logicians)
readily embraced the now commonplace view that is typified by the following remarks:

> In the nineteenth century infinitesimals were driven out of mathematics once and for all,
> or so it seemed. [P. Davis and R. Hersh 1972, p. 78]

But ...

> The German logician Abraham Robinson (1918–1974), who invented what is known as
> *non-standard analysis*, thereby eventually conferred sense on the notion of an infinitesimal
> greater than 0 but less than any finite number. [Moore 1990; 2001, p. 69]

Indeed ...

> nonstandard analysis ..., created by Abraham Robinson in the early 1960s, used tech-
> niques of mathematical logic and model theory to introduce a rigorous theory of both

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[3] Russell's *Mathematics and the Metaphysician* is a reprinting of [Russell 1901] with newly
added footnotes that were introduced because "some points in this essay require modification
in view of later work" [Russell 1918, p. 7]. For additional historical remarks on this paper, see
[Russell 1993, pp. 363–365].

[4] Russell was willing to accept that "lengths of bounded straight lines are infinitesimal as
compared to areas, and these again as compared to volumes of polyhedra." However, according
to Russell, "such genuine cases of infinitesimals ... are always regarded by mathematicians as
magnitudes of a different kind . . . " [1903, p. 337].

[non-Cantorian] infinite and infinitesimal numbers. This, in turn, required a reevaluation of the long-standing opposition, historically, among mathematicians to infinitesimals in particular. [Dauben 1992a, pp. 113–114][5]

All of this is of course well known to historians and philosophers, of mathematics alike. What is not so well known in these communities, however, is that *whereas most late nineteenth- and pre-Robinsonian twentieth-century mathematicians banished infinitesimals from the calculus, they by no means banished them from mathematics*. Indeed, contrary to what the above remarks plainly suggest, between the early 1870s and the appearance of Abraham Robinson's work on nonstandard analysis in 1961 there emerged a large, diverse, technically deep and philosophically pregnant body of consistent non-Archimedean mathematics of the (non-Cantorian) infinitely large and the infinitely small. Unlike non-standard analysis, which is primarily concerned with providing a treatment of the calculus making use of infinitesimals, most of the former work either emerged from the study of the rate of growth of real functions, or is concerned with geometry and the concepts of number and of magnitude or grew out of the natural evolution of such discussions. What may surprise many historians and philosophers even more than the existence of these two bodies of literature is that the latter such body contains constructions of systems of finite, infinite and infinitesimal numbers that are not only sophisticated by contemporary mathematical standards but which are rich enough to embrace the corresponding number systems employed in nonstandard analysis.[6]

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[5] These references only begin to indicate just how widespread this misconception is in the literature–in the English-language literature, in particular. A further indication that this view has reached the status of orthodoxy in the English-language literature is evidenced by the following remarks from Graham Priest's otherwise informative article "Number" from the *Rutledge Encyclopedia of Philosophy*.

> Despite the fact that Cauchy possessed the notion of a limit, he mixed both infinitesimal and limit terminology, and it was left to Weierstrass, later in the century, to replace all appeals to infinitesimals by appeals to limits. At this point infinitesimal numbers disappeared from mathematics (though they would return, as we shall see).

Indeed, as a result of the

> development ... due to Robinson ... called 'nonstandard analysis' ... infinitesimals have been rehabilitated as perfectly good numbers ... the existence of nonstandard models of analysis made infinitesimals legitimate. [Priest 1998, pp. 51–52]

[6] We are not, of course, referring to the sophisticated hyperreal number systems that are frequently employed in non-standard analysis [cf. Keisler 1994], but merely to their underlying non-Archimedean ordered "number" fields and to the systems of infinitesimals contained therein. In particular, we are referring to the fact that each non-Archimedean ordered field is isomorphic to a subfield of a *Hahn field*, the latter being a distinguished type of non-Archimedean ordered field introduced by Hans Hahn in his great pioneering work *Über die nichtarchimedischen Grössensysteme* [1907]. Moreover, since this embedding theorem had already emerged as a folk theorem among knowledgeable field-theorists by the early 1950s [cf. Conrad 1954, p. 328], it is quite likely that Robinson, who was thoroughly conversant with the literature on ordered algebraic systems, was well aware of this when he published his pioneering work on non-standard analysis [1961].

This of course is not to deny that there were late nineteenth-century mathematicians whose calls for the banishment of infinitesimals went beyond the familiar Weierstrassian admonitions to purge the calculus of infinitesimals. Indeed, there were. Cantor, in particular, essentially called for such a banishment–believing that infinitesimals are the "cholera-bacillus of mathematics" [December 13, 1893]. In addition, there were mathematicians such as Killing [1885], Peano [1892] and Pringsheim [1898–1904] who implicitly advocated a restricted form of such a banishment, believing along with Cantor [1887] that the concept of an infinitesimal line segment is provably incoherent. And there were other mathematicians such as Stolz [1883; 1888] and Vivanti [1891a; 1891b] who having essentially equated the straight line of geometry with the continuous straight line of Dedekind also denied the possibility of infinitesimal line segments. But these challenges never went unanswered; those of Killing [1895–96; 1897; 1898], Stolz [1891; 1891a] and Vivanti [1893–94, 1898; 1908] were essentially withdrawn; and unlike the influence that those of Cantor and Peano had on their philosophical champion, Bertrand Russell, from whose writings large numbers of historians and philosophers learned of the debate, their influence on the mathematical community was never deeply pronounced and all but disappeared in the years following the publication of Hilbert's *Grundlagen der Geometrie* [1899], the work that made infinite and infinitesimal line segments and numbers to measure them an integral part of standard mathematics.

Hilbert's *Grundlagen der Geometrie* together with Hahn's *Über die nichtarchimedischen Grössensysteme* [1907] constitute the *loci classici* of the twentieth-century theories of non-Archimedean ordered algebraic and geometric systems. However, as Hahn [1907, p. 601] aptly emphasized, this mathematically profound and philosophically significant body of work grew out of the late nineteenth-century pioneering investigations of non-Archimedean *Grössensysteme* of Stolz [1883, 1884, 1885, 1891], Bettazzi [1890] and Veronese [1889], on the one hand, and the subsequent late nineteenth-century pioneering investigations of non-Archimedean geometries of Veronese [1891, 1894] and Levi-Civita [1892–93, 1898], on the other. Work on the rate of growth of real functions by du Bois-Reymond [cf. 1870–71; 1873; 1875; 1877; 1882] and Thomae [1870; 1872; 1873] also played substantive roles in providing both inspiration and examples for the just-cited non-Archimedean contributions of Bettazzi and Stolz; and Schur's [1899] own early development of portions of geometry independent of the Archimedean axiom also deserves mention in connection with those of Veronese and Levi-Civita.

In this and a companion paper–building on the work of [Ehrlich 1994a; 1995]–we will explore the origins and development of this important body of work in the decades bracketing the turn of the twentieth century as well as the reaction of the mathematical community thereto. Besides helping to fill an important gap in the historical record, it is our hope that these papers will collectively contribute to exposing and correcting the misconceptions regarding non-Archimedean mathematics alluded to above and to shedding light on the mathematical, philosophical and historical roots thereof. In the present paper we will focus our attention on the emergence of theory of non-Archimedean

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For a discussion of Hahn fields and their influence in the twentieth century, see [Ehrlich 1995]; and for a proof of the just-mention embedding theorem for ordered fields that appeared more recently than those already mentioned in [Ehrlich 1995], see [Dales and Wooden 1996, pp. 49–50].

Grössensysteme (systems of magnitudes) in the years prior to the development of non-Archimedean geometry. It was this now largely forgotten pioneering work that laid the groundwork for the modern theory of non-Archimedean magnitudes–the branch of late 19th- and early 20th-century mathematical philosophy that would, in the decades that followed, evolve into the *theories of non-Archimedean ordered groups and semigroups*, and the *theory of non-Archimedean ordered algebraic systems*, more generally.

1. The emergence of non-Archimedean systems of magnitudes I: Setting the stage

Even before Cantor [1872] and Dedekind [1872] had published the modern theories of real numbers that would be employed to all but banish infinitesimals from late 19th- and pre-Robinsonian, 20th-century analysis, Johannes Thomae [1870] and Paul du Bois-Reymond [1870 – 71] were beginning the process that would in the years bracketing the turn of the century not only *establish a consistent and relatively sophisticated algebraic theory of infinitesimals in mainstream mathematics*, but make it, and especially the closely related subject of non-Archimedean Geometry, the focal point of great interest and a mathematically profound and philosophically significant research program. Out of the same body of work of the early 1870s there also emerged a largely parallel development of du Bois-Reymond's *Infinitärcalcül* (calculus of infinities) that led in the same period to the famous works of Hardy [1910; 1912], and some less well known though distinguished work of Hausdorff [1906; 1907; 1909].

By the early 1880s, Otto Stolz [1882; 1883; 1884] had already introduced a pair of number systems and a fledgling theory thereof that Abraham Robinson aptly described as "a modest but rigorous theory of non-Archimedean systems" [1967, p. 39]. The first is based on du Bois-Reymond's system of *orders of infinity* that emerged in connection with the latter's aforementioned analytic work on the *rate of growth of real functions*; and the second and closely related system of so-called *moments of functions*–named after Newton's (infinitesimal) moments–was introduced in part to show that it is possible to model some of the more contentious practices of the early differential calculus using late nineteenth-century algebraico-analytic means.

The two number systems were given large audiences through their incorporation into Stolz's widely used textbook *Vorlesungen über allgemeine Arithmetik* (Lectures on General Arithmetic) [1885], "a work which," as E. V. Huntington wrote in 1902, "has long since proved indispensable to all who desire a systematic and rigorous development of the fundamental elements of modern arithmetic" [1902, p. 40]. Their role in the textbook, however, has little to do with analysis, standard or otherwise; rather, among other things, they are offered as examples of systems of magnitudes that, unlike the continuous system of real numbers, fail to satisfy the Archimedean axiom. With his *Allgemeine Arithmetik* Stolz was thereby able to rapidly spread the word of a number of his important early discoveries regarding the Archimedean axiom including the following two:

> Whereas (i) systems of absolute magnitudes that are continuous in the sense of Dedekind are Archimedean, (ii) there are systems of absolute magnitudes that are non-Archimedean.

In modern parlance, systems of absolute magnitudes in Stolz's sense are systems whose equivalence classes of elements together with the induced order and addition constitute ordered Abelian semigroups that are strictly positive, naturally ordered, strictly monotonic, and divisible.[7] These systems, as Stolz himself shows [1885, pp. 79–80], are (what are today called) strictly positive cones of additively written divisible ordered Abelian groups. It was with these and related discoveries that Stolz laid the groundwork for the *modern theory of magnitudes*. Given the historical significance of these results as well as the mathematical, historical and philosophical significance of the papers with which Stolz originally brought them to the attention of the mathematical community, we will begin our historical overview with discussions of the latter.

Although Stolz first named and stated the importance of the Archimedean axiom in 1881 in the Appendix to his historically important paper that brought Bolzano's contributions to the calculus to the attention of 19th-century mathematicians [Stolz 1881, p. 269], it was in his paper *Zur Geometrie der Alten, insbesondere über ein Axiom des Archimedes* (On the Geometry of the Greeks, in Particular, on the Axiom of Archimedes) [1882, 1883][8] that Stolz first stated and offered purported proofs of (i) and (ii) above.[9]

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[7] For thorough discussions of these and related conceptions as well as the relations between them, the reader may consult [Clifford 1958], [Fuchs 1963] and [Satyanarayana 1979]. For the sake of convenience, however, definitions of some of the pertinent conceptions as well as statements of some of the relations between them are summarized below where, as the reader will notice, "ordered" always means "totally ordered." This convention is employed throughout the paper for ordered rings and fields as well as for ordered groups and semigroups.

A structure $\langle A, + \rangle$ is a *semigroup* (written additively) if A is a set and $+$ is an associative binary operation on A. $\langle A, +, < \rangle$ is an *ordered semigroup* if $\langle A, + \rangle$ is a semigroup and $<$ is a *total ordering* of A which collectively satisfy the following *compatibility* condition for all $a, b, c \in A$: if $a \leq b$, then $a + c \leq b + c$ and $c + a > c + b$. If $\langle A, +, < \rangle$ is an ordered semigroup and $a, b, c \in A$, then $\langle A, +, < \rangle$ is said to be *positively (strictly positively) ordered* if $a + b \geq a$, $b(a + b > a, b)$; it is said to be *right-naturally ordered (naturally ordered)* if it is positively ordered, and *right-solvable*, i.e., $a > b$ implies $a = b + x$ for some $x \in A$ (if it is positively ordered, and *solvable*, i.e., $a > b$ implies $a = b + x = y + b$ for some $x, y \in A$); it is said to be *strictly monotonic* if $a > b$ implies $a + c > b + c$ and $c + a > c + b$; and it is said to be *divisible* if for each a and each positive integer n, there is an $x \in A$ such that $nx = a$. Every right-naturally ordered Abelian semigroup is, of course, naturally ordered, since every right-solvable ordered Abelian semigroup is solvable. Moreover, the solvability condition for ordered Abelian semigroups may be written in the strong form: $a > b$ implies $a = b + x = x + b$ for some $x \in A$. Furthermore, an ordered semigroup is strictly monotonic if and only if it is *cancellative*, i.e., if it obeys the *cancellation laws* $a + c = b + c$ implies $a = b$ and $c + a = c + b$ implies $a = b$ [cf. Clifford 1958, p. 305].

The *strictly positive cone (positive cone)* of an (additively written) ordered Abelian group is the collection of elements of the group greater than (greater than or equal to) the additive identity (i.e., the zero element) of the group together with the addition and order relations restricted thereto.

[8] [Stolz 1883] is a revised and expanded version of [Stolz 1882].

[9] At roughly the same time, Pasch, in his historically important axiomatization of projective geometry, also employed the Archimedean axiom [1882, p. 105 (IV. Grundsatz)]. Unlike Stolz,

Stolz sets the stage for these ideas by remarking that:

> It has often been noted that Euclid implicitly used the principle: *a magnitude can be so often multiplied that it exceeds any other of the same kind.* (See, in particular, X. Proposition 1). Archimedes employed this principle as an explicit axiom in some of his works .... For brevity, we will therefore henceforth refer to this principle as *the Axiom of Archimedes*. To investigate whether or not this is a necessary proposition, requires us first to have agreement on a characterization of the concept of "magnitude."[10]

Such agreement was required for, as Stolz emphasized two years later in the opening lines of his *Allgemeine Arithmetik*:

> The term "magnitude ($\mu\acute{\epsilon}\gamma\epsilon\theta o\varsigma$)" occurs in Euclid's *Elements*, but he nowhere explains the concept.[11]

Like a number of other mathematicians of the period, Stolz draws inspiration for his conception of magnitude from the following passage from Herman Grassmann's influential *Lehrbuch der Arithmetik* (Handbook of Arithmetic):

> Magnitude is anything that may be said to be equal to or not equal to another. Two things are said to be equal, if in each statement you can substitute the one for the other.[12]

However, Grassmann's conception–which has its roots in Aristotle's conception of quantity and Leibniz's conception of identity–is, according to Stolz, "as general as you can imagine" [1883, p. 507]. Thus, being primarily interested in systems of magnitudes exemplified by the collection of nontrivial bounded segments of the Euclidean line with order, addition and "equality" (i.e., congruence) classically defined–*systems of absolute magnitudes*, as he calls them[13]–Stolz proceeds to outline what appears to be the first

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however, Pasch neither attempted to prove (i) and (ii) above, nor did he raise the issue of the possibility of non-Archimedean systems of magnitudes.

[10] [Stolz 1883, p. 504]: "Es ist schon öfter hervorgehoben worden, dass Euclid implicite den *Grundsatz* gebraucht: *eine Grösse kann so oft vervielfältigt werden, dass sie jede andere, ihr gleichartige, übertrifft.*(Vgl. insbesondere X. prop. 1). Bei Archimedes begegnet man einer ausdrücklichen *Annahme*, welche mit diesem Grundsatze übereinstimmt .... Derselbe mag daher kurz als *das Axiom des Archimedes* bezeichnet werden. Eine Untersuchung, ob der in Rede stehende Satz als Grundsatz zu gelten hat oder nicht, erfordert zunächst, dass man sich über den Begriff "Grösse" verständige."

[11] [Stolz 1885, p. 1]: "Die Bezeichnung "Grösse ($\mu\acute{\epsilon}\gamma\epsilon\theta o\varsigma$)" kommt in Euclid's Elementen vor, doch ihren Begriff erklärt er nirgends."

[12] [Grassmann 1861, p. 1]: "Grösse heisst jedes Ding, welche einem andern gleich oder ungleich gesetzt werden soll. Gleich heissen zwei Dinge, wenn man in jeder Aussage statt des einen das andere setzen kann."

[13] Besides systems of absolute magnitudes, Stolz studied systems of absolute magnitudes supplemented with a zero element, as well as systems of so-called *relative* magnitudes, that is, systems of absolute magnitudes supplemented with a zero element as well as negative members [1883; 1885, pp. 79–80]. Systems of relative magnitudes are systems whose equivalence classes of elements with addition and order suitably defined constitute divisible ordered Abelian groups. For our purpose here, however, we need only consider the absolute case.

reasonably sophisticated attempted axiomatization of such a system,[14] an axiomatization that Stolz presents in greater detail and with minor modifications in his *Allgemeine Arithmetik* [1885, p. 70; and pp. 4–5] as follows.

According to Stolz, the order relation and so-called equality can be defined arbitrarily provided that the following conditions are satisfied:

(i) If $A = B$, then $B = A$;

(ii) If $A > B$, then $B < A$ (and conversely);

(iii) $A = B$ or $A > B$ or $A < B$ [it being understood that precisely one is the case];

(iv) If $A = B$ and $B = C$, then $A = C$;

(v) If $A = B$ and $B > C$, then $A > C$;

(vi) If $A > B$ and $B > C$, then $A > C$.

In addition, Stolz tells us that + must be defined in such a manner that $A + B$ is in the system whenever A and B are, and it must satisfy the rules that govern the "*absolute whole numbers*" (i.e., the strictly positive integers), namely: for all members A, B, and C of the system

(vii) $A + (B + C) = (A + B) + C$;

(viii) $A + B = B + A$;

(ix) $A + B = A' + B'$ if $A = A'$ and $B = B'$;[15]

(x) $A + B > A' + B'$ if $A > A'$ and $B = B'$;

(xi) $A + B > A$;

(xii) $A = B + X$ for some X in the system whenever $A > B$.

To complete the postulate set Stolz adds the following condition which du Bois-Reymond appealed to in his definition of a linear magnitude (see Appendix I), a condition that has its roots in Book VI of Euclid's *Elements* and which in contemporary algebraic parlance is frequently called the condition of *divisibility*:

(xiii) For each member A of the system and each positive integer n, there is an X in the system such that $nX = A$.[16]

Like other authors writing on similar topics prior to the influence of the writings of Peano [1889; 1894; 1900], Stolz did not explicitly list the reflexivity of the equality relation (i.e., $A = A$ for all A) as an axiom. We hasten to add, however, that in his preliminary remarks on magnitudes contained in the Introduction to his *Allgemeine Arithmetik* Stolz does assert that "Each magnitude is equal to itself" is one of the basic presuppositions of his discussion of magnitudes [1885, p. 2]. Also mentioned in this regard is that: "One can posit each magnitude arbitrarily often .... So even after *a* is combined with *b*, *a* can

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[14] Stolz apparently drew some inspiration for his axiomatiztion of an Archimedean system of absolute magnitudes from du Bois-Reymond's conception of a *linear magnitude* [1882, pp. 43–48]. For a statement of du Bois-Reymond's conception and a brief discussion of its relation to Stolz's axiomatization, see Appendix I.

[15] There is a typographical error in Stolz's presentation of condition (ix): instead of "$A + B = A' + B'$" we find "$A + B = A' + B$." The error is corrected in [Stolz and Gmeiner 1902, p. 100].

[16] Proposition 9 of Book VI of Euclid's *Elements* reads: "From a given straight line to cut off a prescribed part" [Heath 1956 Volume 2, p. 211]. This is understood to mean that any line segment can be divided into n equal parts for each positive integer n.

nevertheless also be combined with b'; a can also be combined with itself" [1885, p. 2]. With these presuppositions Stolz's axiomatization, while lacking elegance, is entirely adequate to the task at hand and constitutes, in our opinion, sufficient reason for regarding Stolz as the father of the modern theory of extensive magnitudes, an honor usually conferred on Helmholtz [1887] or Hölder [1901] (cf. [Krantz *et al.* 1971, p. 71]).

2. The emergence of non-Archimedean systems of magnitudes II: du Bois-Reymond's orders of infinity of functions and Stolz's ordered algebraic system thereof

With his system of axioms for absolute magnitudes thus at hand, Stolz turns to his attempt to establish their independence from the Archimedean axiom. As we alluded to above, to establish the non-Archimedean case he appeals to a fragment of du Bois-Reymond's calculus of infinities, a system that two years earlier he had implicitly suggested could be employed for just such a purpose [1881, p. 269]. To prepare the way for Stolz's construction, we begin with a brief summary of the relevant features of du Bois-Reymond's system.

Du Bois-Reymond laid the groundwork for his theory in his paper *Sur la grandeur relative des infinis des functions* (On the relative size of the infinities of functions) [1870–71], and developed it in more than a dozen other works [cf. 1873; 1875; 1877] culminating in his mathematico-philosophical treatise *Die Allgemeine Functiontheorie* (General Function Theory) [1882].[17] Much of the primary motivation for du Bois-Reymond's *Infinitärcalcül* is neatly encapsulated by the following remarks with which G. H. Hardy begins his celebrated monograph on du Bois-Reymond's system.

> The notions of the 'order of greatness' or 'order of smallness' of a function $f(n)$ of a positive integral variable n, when n is 'large', or of a function $f(x)$ of a continuous variable x, when x is 'large' or 'small' or 'nearly equal to a', are important even in the most elementary stages of mathematical analysis. We learn there that x^2 tends to infinity with x, and moreover that x^2 tends to infinity *more rapidly than* x, i.e. that the ratio x^2/x tends to infinity also; and that x^3 tends to infinity more rapidly than x^2, and so on indefinitely. We are thus led to the idea of a 'scale of infinity' (x^n) formed by the functions $x, x^2, \ldots, x^n, \ldots$. This scale may be supplemented and to some extent completed by the interpolation of non-integral powers of x. But there are functions whose rates of increase cannot be measured by any of the functions of our scale, even when thus completed. Thus $\log x$ tends to infinity more slowly, and e^x more rapidly, than *any* power of x; and $x/(\log x)$ tends to infinity more slowly than x, but more rapidly than any power of x less than the first.
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> As we proceed further in analysis, and come into contact with its modern developments, such as the theory of Fourier's series, the theory of integral functions, or the theory of singular points of analytic functions in general, the importance of these ideas becomes greater and greater. It is the systematic study of them, the investigation of general

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[17] For a complete list of du Bois-Reymond's writings on his *Infinitärcalcül* and a good survey of the contents thereof, see [Fisher 1981].

theorems concerning them and ready methods of handling them, that is the subject of Paul du Bois-Reymond's *Infinitärcalcül* or 'calculus of infinities'. [Hardy 1910, pp. 1–2][18]

Du Bois-Reymond erects his calculus primarily on families of increasing functions from $\mathbb{R}^+ = \{x \in \mathbb{R} : x > 0\}$ to $\mathbb{R}$ such that for each function f of a given family, $\lim_{x \to \infty} f(x) = +\infty$, and for each pair of functions f and g of the family, $0 \leq \lim_{x \to \infty} f(x)/g(x) \leq +\infty$.[19] He assigns to each such function f a so-called *infinity*, and defines an ordering on the infinities of such functions by stipulating that for each pair of such functions f and g:

$f(x)$ has an infinity *greater than* that of $g(x)$, if $\lim_{x \to \infty} f(x)/g(x) = \infty$;

$f(x)$ has an infinity *equal to* that of $g(x)$, if $\lim_{x \to \infty} f(x)/g(x) = a \in \mathbb{R}^+$;

$f(x)$ has an infinity *less than* that of $g(x)$, if $\lim_{x \to \infty} f(x)/g(x) = 0$.

To represent these states of affairs du Bois-Reymond employs the symbolic expressions "$f(x) \succ g(x)$", "$f(x) \sim g(x)$,", and "$f(x) \prec g(x)$", respectively.

Du Bois-Reymond was not always as clear as one would hope about the precise contents of the families of functions with which he was concerned; nor did he make any real use of arithmetic operations on the infinities arising from such families and attempt thereby to bring algebra to his infinities as Stolz [1883], Pincherle [1884], Borel [1899; 1902; 1910] and others later would. He also mistakenly assumed (during the early stages of the development of the theory) that each pair of increasing functions f and g from $\mathbb{R}^+ = \{x \in \mathbb{R} : x > 0\}$ to $\mathbb{R}$ for which $\lim_{x \to \infty} f(x) = +\infty$ and $\lim_{x \to \infty} g(x) = +\infty$ could be compared in the manner described above, something that Pincherle [1884], Cantor [1895], Borel [1898] and Hausdorff [1907; 1909] emphasized is not the case.[20] On the other hand, he did establish a number of *order-theoretic* results regarding his "infinities"–some of which are illustrated by remarks made by Hardy above–which provided intimations of their inability to be adequately represented by ordered sets of real numbers and, hence, by ordered subsets of Archimedean ordered groups. In particular, he established the existence of families of functions whose open densely ordered sets of infinities contain subsets having the order type of $\mathbb{R}$ but for which there are no infinitary analogs of the following properties of $\mathbb{R}$: (i) $\mathbb{R}$ contains a cofinal subset of order type ω;

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[18] It is interesting to note that in 1778, in his "*De infinities infinitis gradibus tam infinite magnorum quam infinite parvorum*," Euler had undertaken an analogous study of what he called "degrees" of infinity and arrived at results analogous to some of those of du Bois-Reymond. However, Euler's comparisons were based upon treating expressions such as x^2 and $\log x$ not as designating functions that go to infinity as x goes to infinity but rather as the square and logarithm of an infinite magnitude x. For an overview of Euler's discussion, see [Bos 1974, pp. 84–86].

[19] In some of his works (cf. [1870–71; 1875]), du Bois-Reymond also considers functions f such that $\lim_{x \to \infty} f(x) = +0$. Moreover, as we shall later see, some authors such as Thomae also consider functions f such that $\lim_{x \to \infty} f(x) = c$ where $0 < c < \infty$. On this matter, also see [Hardy, 1910, p. 4; 1924, p. 4].

[20] That is, it can happen that f/g neither tends to $+\infty$ nor to $+0$ nor remains between positive bounds. Others who apparently followed du Bois-Reymond in failing to recognize this possibility are Bettazzi [1890, § 66; 1893, p. 55] and Vivanti [1891a, p. 147].

(ii) $\mathbb{R}$ contains a coinitial subset of order type ${}^*\omega$; (iii) for each $a \in \mathbb{R}$, $\{x \in \mathbb{R} : x < a\}$ contains a cofinal subset of order type ω, and $\{x \in \mathbb{R} : x > a\}$ contains a coinitial subset of order type ${}^*\omega$.[21,22] Indeed, it was the following violations of infinitary analogs of (ii) and (iii) that du Bois-Reymond appealed to in his writings to illustrate that his system of infinities constitutes a "broader idea of the *numerical continuum*" than the Cantor-Dedekind continuum of real numbers [du Bois-Reymond 1877, p. 150].

> I. Giving any series $\phi_1, \phi_2, \dots, \phi_n, \dots$ of increasing functions such that $\dots \prec \phi_n \prec \dots \prec \phi_2 \prec \phi_1$, we can always find a function f that increases more slowly than any member of the series, i.e., which satisfies the relation $f \prec \phi_n$ for all positive integers n. [Du Bois-Reymond, 1875, p. 153]
>
> II. Given a function Φ and a series of functions $\phi_1, \phi_2, \dots, \phi_n, \dots$ such that $\phi_1 \prec \phi_2 \prec \dots \prec \phi_n \prec \dots \prec \Phi$, there is a function f such that $\phi_n \prec f \prec \Phi$ for each positive integer n; moreover, given a function Φ and a series of functions $\phi_1, \phi_2, \dots, \phi_n, \dots$ such that $\Phi \prec \dots \prec \phi_n \prec \dots \prec \phi_2 \prec \phi_1$, there is a function f such that $\Phi \prec f \prec \phi_n$ for each positive integer n. [Du Bois-Reymond 1877, p. 153]

Du Bois-Reymond was also aware of the following ascending formulation of I, though it was Borel who first established this formulation of what he, and later Hardy, called *Du Bois-Reymond's Theorem*.

> III. Giving any series $\phi_1, \phi_2, \dots, \phi_n, \dots$ of increasing functions such that $\phi_1 \prec \phi_2 \prec \dots \prec \phi_n \prec \dots$, we can always find a function f that increases more rapidly than any member of the series, i.e., which satisfies the relation $\phi_n \prec f$ for all positive integers n. [Borel 1898, pp. 112–114]

Of course, simply pointing to the existence of a family of the said functions whose open densely ordered set of infinities satisfies I, II, and III and contains a subset having the order type of $\mathbb{R}$ does not alone constitute a proof that the family is in some *significant sense* a richer densely ordered set than $\mathbb{R}$. Arguably, it was Hausdorff [1908; 1909] who first provided such a proof when he demonstrated that, unlike $\mathbb{R}$, the ordered set $I(P)$ of infinities of functions comprising a *pantachy P* (i.e., a maximal family of pairwise comparable such functions) satisfies the condition: for all finite or denumerably infinite subsets X and Y of $I(P)$ where every member of X precedes every member of Y, there is an infinity in $I(P)$ that lies strictly between those in X and those in Y.[23] On the other

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[21] If Y is a subset of an ordered set X, then Y is said to be a *cofinal (coinitial)* subset of X if for each $x \in X$ there is a $y \in Y$ such that $y \geq x (y \leq x)$. Moreover, an ordered set X is said to have order type $\omega ({}^*\omega)$ if X is order-isomorphic to the ordered set of positive (negative) integers in their natural order.

[22] Although the general concepts of cofinal and coinitial subsets of an ordered set were first introduced by Hausdorff in his early twentieth-century investigations of ordered sets, the basic contents of the above result were certainly well known to mathematicians by the mid to late 1870s; indeed, it would have been evident to any mathematician of the day that the ordered set of positive integers instantiates (i), that the ordered set of negative integers instantiates (ii), and that for each real number a, any strictly increasing (decreasing) Cauchy sequence of members of $\mathbb{R}$ whose limit is a instantiates (iii).

[23] In Hausdorff's terminology, which remains in place today, whereas $I(P)$ is an η_1 - *set* or an η_1 - *ordering*, $\mathbb{R}$ is merely an η_0-ordering (cf. [Rosenstein, 1982]). Prior to the work of Hausdorff,

hand, du Bois-Reymond's results certainly persuaded some of the most distinguished mathematicians of his day that there are ordered subsets of his infinities that are in some sense richer than the continuum of real numbers. Poincaré, for example, was thus persuaded. Indeed, writing in response to the question "Is the creative power of the mind [to construct dense systems of numbers] exhausted by the creation of the mathematical continuum?" Poincaré wrote: "The answer is in the negative. And this is shown in a very striking manner by the work of Du Bois-Reymond" [1893 in 1952, p. 28].

Otto Stolz was also persuaded that there are families of du Bois-Reymond's infinities that are richer than the ordered set of real numbers, and this together with his vague understanding of the maximal nature of $\mathbb{R}^+$ as an Archimedean, absolute system of magnitudes may have been what led him to proclaim (without explanation) that it was du Bois-Reymond's system that was responsible for the recognition–presumably his own–of the significance of the Archimedean axiom [1885, pp. iii–iv]. On the other hand, what may have motivated Stolz's remark is the far more elementary observation that insofar as e^x increases more rapidly than any power of x, if one could find a family of functions including e^x and $x, x^2, \ldots, x^n, \ldots$ whose infinities are closed under addition where the sum of the infinities of two such functions is defined as the infinity of the product of the functions, one would have an immediate violation of the Archimedean condition. Indeed, as we shall now see, whatever Stolz's motivation might have been, it is precisely the latter observation that lies both at the heart of Stolz's construction and his proof of the non-Archimedean nature of the resulting system.

To be more specific, Stolz considers the set of all functions $f : \mathbb{R}^+ \to \mathbb{R}^+$ formed by means of finite combinations of the operations $+, -, \cdot,$ and $\div$ from positive rational powers of the functions $x, \ln x, \ln(\ln x), \ldots ; e^x, e^{e^x}, e^{e^{e^x}}, \ldots$ where $\ln x$ is the natural logarithm of x and e is the base of the natural logarithm. Following du Bois-Reymond, Stolz assigns to each such function f an *infinity*, which he denotes by "$\mathfrak{ll}(f)$", and defines an ordering on the infinities of such functions by stipulating that for each pair of such functions f and g:

$$\mathfrak{ll}(f) > \mathfrak{ll}(g), \text{ if } \lim_{x \to +\infty} f(x)/g(x) = +\infty,$$

$$\mathfrak{ll}(f) = \mathfrak{ll}(g), \text{ if } \lim_{x \to +\infty} f(x)/g(x) = a \in \mathbb{R}^+,$$

$$\mathfrak{ll}(f) < \mathfrak{ll}(g), \text{ if } \lim_{x \to +\infty} f(x)/g(x) = 0.$$

To complete the construction, Stolz defines addition and subtraction of the infinities by the rules:

Borel [1898] showed that there are ordered sets of infinities of such pairwise comparable functions that contain no countable cofinal well-ordered set. This together with Cantor's [1882, p. 117] result that $\mathbb{R}$ contains no uncountable well-ordered set showed that there is no order preserving embedding of such a family of infinities into $\mathbb{R}$, thereby showing that in some sense a pantachy is a richer ordered set than the reals. Hausdorff's result goes farther, however; Hausdorff's result implies Borel's but not vice versa. Borel's result may be obtained from Hausdorff's by letting X be an arbitrary countable infinite subset and Y be empty. Similarly, by letting X be empty and Y be an arbitrary countable infinite set, one may show that a pantachy has no countable coinitial subset.

$$\mathfrak{U}(f) + \mathfrak{U}(g) = \mathfrak{U}(f \cdot g)$$

$$\mathfrak{U}(f) - \mathfrak{U}(g) = \mathfrak{U}(f/g), \text{ if } \mathfrak{U}(f) > \mathfrak{U}(g).$$

With the construction thus at hand, Stolz correctly observes that the resulting system can be shown to satisfy his aforementioned axioms for absolute magnitudes, and that since $\lim_{x \to \infty}(e^x/x^n) = +\infty$ and $n\mathfrak{U}(x) = \mathfrak{U}(x^n)$ for each positive integer n, it follows that while $\mathfrak{U}(x) < \mathfrak{U}(e^x)$, there is no positive integer p such that $p\mathfrak{U}(x) > \mathfrak{U}(e^x)$, which establishes the system's non-Archimedean nature.

Having exhibited an instance of a non-Archimedean system of absolute magnitudes, Stolz turns his attention to proving that systems of absolute magnitudes that are continuous are Archimedean. For this purpose Stolz employs the more general conditions of density and absence of a least element in place of divisibility.[24] Moreover, arguing that Cantor's definition of a linear continuum–*connected* and *perfect*–presupposes the Archimedean condition,[25] Stolz makes use of Dedekind's conception for this end. Following a purported proof of the result, he concludes:

> Thus, it is proved that the axiom of Archimedes for *segments* in the sense of ancient geometry appears as a corollary if one adds continuity to the properties set forth by Euclid. Should, however, this assumption be omitted, then the basic proposition in question for *the system of segments* (as also *for that of the angles*) must be assumed among the axioms of geometry. [Stolz 1883, p. 512: (Translation Fisher 1981, p. 129)]

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[24] Thus, it appears that Stolz was not aware that density is equivalent to the absence of a least element in the case of systems of magnitudes satisfying his axioms (i)–(xii).

[25] According to Cantor, a subset T of $\mathbb{R}^n$– the n-dimensional Cartesian space with distance defined in the standard fashion–is *connected* if for any two points t and t' of T, there are always a finite number of points $t_1, t_2, \ldots, t_v$ of T such that the distances $\bar{t}t_1, t_1\bar{t}_2, \ldots, t_v\bar{t}'$ are all less than any given arbitrarily small number $\varepsilon \in \mathbb{R}^+$; and T is *perfect* if T coincides with its first derivative [Cantor 1883 in Ewald 1996, p. 906]. The latter condition, as Cantor later showed, is equivalent to the assertion that: every convergent series in T has a limit in T and every member of T is the limit of a convergent series in T.

Although Cantor's definition was given for a subset of $\mathbb{R}^+$, the basic idea could be applied to systems of magnitudes, more generally, as Stolz was well aware. However, as we noted above, Stolz chose not to use Cantor's condition for his purpose because "it presupposes" the Archimedean axiom; in particular, argued Stolz, Cantor's connectivity condition presupposes the Archimedean axiom. In fact, an absolute system of magnitudes in Stolz's sense is Archimedean if and only if it satisfies Cantor's connectivity condition (suitably formulated). The nontrivial portion of the proof is straightforward and was given by Stolz himself [1883, p. 509; Note*] as follows. Let Π_0 be a system of magnitudes satisfying Stolz's axioms together with Cantor's connectivity condition. Also let P and Q be magnitudes in Π_0 such that $P < Q$, and further suppose D is an arbitrary magnitude in Π_0 for which $D < Q - P$. By connectivity, there is a finite number of magnitudes $R_1, R_2, \ldots, R_n$ in Π_0 such that the differences $R_1 - P, R_2 - R_1, \ldots, Q - R_n$ are all smaller than D. But then $Q - P < (n + 1)D$, which suffices to prove the Archimedean condition.

As our reference to Stolz's "purported proof" of the Archimedean nature of Dedekind continuous systems of absolute magnitudes suggests, the proposed proof is flawed.[26] Among its shortcomings is its reliance on a defective characterization of continuity in the sense of Dedekind [1883, p. 509]. The latter shortcoming was corrected (without mention) by Stolz in his *Allgemeine Arithmetik* [1885, p. 83] but, as Veronese [1889, p. 602] and Hölder [1901, p. 10; 1996, p. 248] later indicated, difficulties in the proof remained. In response to the criticisms by Veronese, Stolz returned to the matter in his *Ueber das Axiom des Archimedes* [1891], but by then Rudolfo Bettazzi [1890, pp. 42–44] had already presented an adequate proof of the result [cf. Vivanti, 1891, p. 58]. The proofs of Bettazzi and Stolz, however, were soon eclipsed by the proof of Hölder [1901, p. 13]–the proof that Stolz and J. A. Gmeiner incorporated into their *Theoretische Arithmetik* [1902, p. 115], the work that replaced Stolz's *Allgemeine Arithmetik* as the most highly respected and widely used textbook on real and complex numbers of its day.[27]

3. The emergence of non-Archimedean systems of magnitudes III: Stolz's moments of functions

Stolz and Gmeiner's *Theoretische Arithmetik* is a revised version of portions of Stolz's *Allgemeine Arithmetik*, and the axioms for systems of absolute magnitudes employed in the earlier work remain essentially the same. On the other hand, du Bois-Reymond's infinitary calculus, which was by then quite well known, is merely mentioned in passing in the latter work as an example of a non-Archimedean system of absolute magnitudes [1902, p. 99]. In addition, Stolz's systems of moments of functions (and particular extensions thereof) which serve as additional illustrations of non-Archimedean

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[26] Stolz himself later acknowledged the point on more than one occasion [1891; 1902, p. 114: Note 2]. It is therefore evident that Gordon Fisher is mistaken when in reference to Stolz's purported proof he says: "It seems likely that Stolz is the first to have given a satisfying proof of this theorem" [Fisher 1891, p. 129; also see, p. 133].

[27] The following quotation from G. H. Hardy's review of Stolz and Gmeiner's *Theoretische Arithmetik* provides a good indication of the high esteem in which the mathematical community continued to hold Stolz's text.

> This book is a new enlarged edition of certain chapters of Dr. Stolz's well-known *Allgemeine Arithmetik*. The merits of the latter are universally recognized, and no praise could be higher than to say that in lucidity and thoroughness the present volume is an improvement on it. [Hardy 1903, p. 808]

In fact, Stolz and Gmeiner's book continued to be the standard reference work on real and complex numbers into the third decade of the twentieth century when it was replaced by the textbooks on abstract algebra by Haupt [1929] and van den Waerden [1930; 1937], both of which contain sections with references on non-Archimedean ordered fields as well as definitions of the infinitesimal elements of such fields.

systems of magnitudes in the earlier work are now replaced by lexicographically ordered systems of "*complex numbers having n units*" [1902, p. 280]. The latter systems, which have their roots in the work of Johannes Thomae [1871], will be one of the foci of our attention when we come to the work of Bettazzi [1890]. For the present, however, we turn to the systems of moments of Stolz and his extensions thereof.

As we emphasized in the Introduction, and the above has already begun to show, while late nineteenth- and pre-Robinsonian twentieth-century mathematicians banished infinitesimals from mainstream analysis, they by no means banished them from mathematics. In fact, even the banishment of infinitesimals from analysis was never quite as complete as the standard histories (cf. [Boyer 1949; Edwards 1979; Bottazzini 1986]) might lead one to surmise. Indeed, as Robinson [1961, p. 433; 1974, p. 278] himself was well aware, from time to time during the period separating the arithmetization of analysis and the emergence of nonstandard analysis, there had been mainstream mathematicians who would not rule out the possibility of a logically satisfactory alternative foundation for analysis based on infinitesimals. Perhaps the best known examples of such mathematicians are Schmieden and Laugwitz in connection with their work *Eine Erweiterung der Infinitesimalrechnung* (An Extension of the Infinitesimal Calculus) [1958; also see Laugwitz 1961, 1961a]. However, to their voices one may add those of Neder [1941–43; 1941–43a], Fraenkel [1928, pp. 116–117; 1953, p. 165], Klein [1911/1939, pp. 218–219], Levi-Civita [1892–93], Bettazzi [1891] and, as we have already mentioned, Stolz.[28]

Stolz expressed this opinion, if only implicitly, in his mathematico-philosophical essay *Die unendlich kleinen Grössen* (Infinitely Small Magnitudes) [1884]. Stolz begins his discussion by remarking that:

> Bossut said in one of his essays on the general history of mathematics, that he had asked Fontaine for clarification on some of the theorems of the infinitesimal calculus and received the answer: "Take the infinitely small as a hypothesis, study the application of the calculus, and belief will come to you." It seems annoying that also in mathematics the power of faith should be called for. However, this seems really necessary; since the introduction of infinitely small magnitudes by Newton and Leibniz the infinitesimal calculus has met with partial support and has aroused partial opposition. Currently the argument on this seems to be turning more and more to its disadvantage; yes, it appears to be already decided. Because it is possible without difficulty to present higher analysis with its applications to geometry and mechanics without the use of the infinitely small, this invention is for now dispensable. Such a conclusion, however, requires us ... to ask the question whether or not they are permissible in mathematics although they no longer have fundamental significance. Indeed, since for two centuries we have

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[28] In addition, some advocates of *Smooth Infinitesimal Analysis* have recently pointed out there were differential geometers such as E. Cartan who made use of (nilpotent) infinitesimals in some of their works. See, for example, the Introduction to [Moerdijk and Reyes 1991].

calculated with infinitely small magnitudes just like real numbers, we cannot at all avoid this investigation.[29, 30]

As one would expect from his earlier work on the geometry of the ancients, Stolz sees no reason for banishing infinitesimals from mathematics. In fact, despite the wording of his query, Stolz's paper is not really concerned with whether infinitesimals should be allowed in mathematics *per se*, but rather with pointing out among other things that given the then current state of knowledge one could not rule out the possibility of a rigorous alternative development of the calculus based on infinitesimals. Moreover, in an attempt at a modest contribution to the construction of such a calculus, Stolz shows that it is possible to develop systems containing infinitesimals in which–contrary to the well-known critiques of Berkeley and others–the derivative equals the ratio of differential (infinitesimal) magnitudes. Moreover, in the course of so doing, he shows that it is possible to model the equally contentious claim of the early differential calculus that

$$x + dx = x$$

or, more generally, that $a + A = a$ if A is infinitely small relative to a.[31] It is the latter absorptive aspect of Stolz's addition that permits terms involving higher order differentials to be eliminated in the process of differentiation that underlies Stolz's treatment of the derivative.

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[29] [Stolz 1884, pp. 21–22]: "Bossut erzählt in seinem Essai sur l'histoire générale des mathématiques, er habe Fontaine um Aufklärung über einige Sätze der Infinitesimalrechnung gebeten und von ihm die Antwort erhalten: "Nehmen Sie die unendlich Kleinen als eine Hypothese an, studiren Sie die Anwendung der Rechnung und der Glaube wird Ihnen kommen". Manchen wird es befremden, dass auch in der Mathematik die Kraft des Glaubens angerufen wird. Es muss das aber wirklich nöthig sein; denn seit der Einführung der unendlich kleinen Grössen durch Newton und Leibniz hat die Infinitesimalrechnung theils Anerkennung gefunden, theils Widerspruch erregt. Gegenwärtig scheint sich der Streit über dieselbe mehr und mehr zu ihrem Nachtheil zu wenden, ja bereits in diesem Sinne entschieden zu sein. Denn wenn es ohne irgend eine Schwierigkeit möglich ist, die höhere Analysis mit ihren Anwendungen auf die Geometrie und Mechanik ohne Gebrauch des unendlich Kleinen vorzutragen, so muss diese Erfindung vorläufig als entbehrlich erscheinen. Eine solche Wahrnehmung enthebt uns jedoch nicht der Pflicht, über den wahren Sinn der quantitates infinitesimae nachzudenken d. h. die Frage uns vorzulegen, ob sie überhaupt in der Mathematik zulässig seien, wenn ihnen auch eine fundamentale Bedeutung nicht mehr beigelegt werden kann. Ja, wir können u[n]s der angeregten Untersuchung gar nicht entschlagen gegenüber der Thatsache, dass seit zwei Jahrhunderten mit den unendlich kleinen Grössen gerechnet wird und zwar so, wie mit den reellen Zahlen."

[30] It is of interest to note that in his review of Stolz's paper for the *Jahrbuch über die Fortschritte der Mathematik*, E. R. Hoppe [1884] criticized Stolz for claiming that the debate over the use of infinitesimals in the calculus appears to have been settled to their disadvantage. As far as Hoppe was concerned, the debate was still alive.

[31] Although Stolz names his infinitesimals after Newton's "moments," he frames the discussion in terms of Leibniz's differentials. With respect to the issues at hand, however, the conceptions can be used interchangeably. For discussions of these matters, see [Guicciardini 1989] and [Bos 1974].

Following a discussion of these matters as well as a discussion of the algebraic virtues and limitations of his systems, Stolz draws the following rather uncertain conclusion about the future of the general program.

> As is mentioned in the Introduction, the infinitely small is not at all required for the Differential- and Integral-calculus. Already for Cauchy the term infinitely small magnitude serves only to indicate for short a variable magnitude which approaches the limit zero and could be fully suppressed without leaving a gap. Cauchy's presentation, which has now been accepted nearly everywhere, is not invulnerable; however, the improvements on his work are creating only a more precise formulation of his ideas, as he has in part acknowledged. With respect to this, one cannot expect any more from any kind of infinitely small magnitudes. Whether or not the theories developed above will have any significance in mathematics cannot be decided without doubt. With even less certainty, can one assume whether or not they can be replaced by another, more powerful theory.[32]

Although Stolz's systems of 1884 were hardly adequate for the development of analysis, they not only provided further examples of non-Archimedean systems of magnitudes, but ones that have modest multiplicative structures as well. In the parlance of contemporary mathematics, Stolz's systems have all the properties of commutative semirings with the exception that their additive operations are not *regular*, i.e., each of Stolz's systems contain elements a and b for which the equations $a + x = b$ and $x + a = b$ need not have at most one solution in the system.[33] The failure of regularity occurs because in his systems the equation $a + x = a$ has more than one solution–indeed, infinitely many solutions–when the variable x takes on values infinitesimal relative to a [Stolz 1884, pp. 30, 34–35;1885, pp. 212–213].[34]

In his *Allgemeine Arithmetik* Stolz motivates his presentation of these systems with the following observation that hints at their analytic roots.

> One says, that a variable x becomes infinitely small if it has the limit 0. Furthermore, if $\lim f(x) = 0$ when $\lim x = a$, then $f(x)$ becomes infinitely small together with $x - a$. Then the quotient $f(x) : (x - a)$ can have a finite limit b .... This applies particularly for

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[32] [Stolz 1884, p. 36]: "Wie Eingangs erwähnt, bedarf man des unendlich Kleinen in der Differential- und Integralrechnung gar nicht. Schon bei Cauchy dient dieses Wort nur zur Abkürzung–eine unendlich kleine Grösse ist eine Veränderliche, die sich dem Grenzwerthe Null nähert–und könnte ohne eine Lücke zu hinterlassen, völlig unterdrückt werden. Cauchy's Darstellung, die gegenwärtig fast überall angenommen ist, ist zwar nicht unanfechtbar; allein die daran angebrachten Verbesserungen bilden nur eine genauere, zum Theil von ihm selbst anerkannte Formuli[e]rung seiner Ideen. In dieser Beziehung ist also von irgend welcher Art unendlich kleiner Grössen nichts mehr zu erwarten. Ob die im Vorstehenden entwickelten Theorien derselben dennoch eine Bedeutung für die Mathematik haben, ist vorderhand nicht mit Sicherheit zu entscheiden. Noch weniger lässt sich angeben, ob nicht etwa eine andere, mehr leistende Theorie an ihre Stelle gesetzt werden könne."

[33] In the context of Stolz's systems, the failure of regularity is equivalent to the failure of the cancellation laws for addition (see Note 7). For the definition of a semiring and a discussion of regularity and its relation to the cancellation laws, see [Redei 1967, pp. 36 and 45].

[34] The discussion of Stolz's moments in [Fisher 1981, pp. 131–132] is somewhat misleading since it suggests that the failure of regularity only occurs in Stolz's extensions of his systems of moments, the latter of which will be discussed below.

the quotient $\{g(x) - g(a)\} : (x - a)$, where $g(x)$ is a continuous function for $x = a$–the consideration of which the differential calculus is concerned .... However, insofar as the denominator $x - a$ is zero for $x = a$, this quotient loses all meaning for $x = a$. Therefore, if under these circumstances the number b should nevertheless arise by means of a division, then the dividend and divisor cannot be real numbers, but must be something essentially different from them, magnitudes of a new kind suitable for calculation.[35]

To provide an analytic representation of such quotients, Stolz requires a number system containing real numbers as well as the just-cited "magnitudes of a new kind." Following a cautionary remark to the effect that he does not regard such a number system "to be indispensable in analysis," Stolz presents a construction of such a system in two stages beginning with the "magnitudes of a new kind," i.e., the infinitesimals or, moments of functions as he calls them.

In general, Stolz [1884, pp. 27–28; 1885, pp. 206–207] constructs his systems of moments of functions on selected families of increasing functions from $\mathbb{R}$ to $\mathbb{R}$ such that for each function f of a given such family, $\lim_{x \to a+0} f(x) = +0$ where a is a fixed value such that $-\infty \le a \le +\infty$, and for each pair of functions f and g of the family $0 \le \lim_{x \to a+0} f(x)/g(x) \le +\infty$. In his *Allgemeine Arithmetik*, in particular, for an arbitrarily selected and fixed value a such that $-\infty \le a \le +\infty$, Stolz considers the set of all such functions that can be formed by means of finite combinations of the operations $+, -, \cdot$, and $\div$ from positive rational powers of the functions

$$x - a, -1/\ln(x - a), -1/\ln(-1/\ln(x - a)), \ldots ; 1/e^{\frac{1}{x-a}}, 1 \Big/ e^{e^{\frac{1}{(x-a)}}}, \ldots$$

where again $\ln x$ is the natural logarithm of x and e is the base of the natural logarithm. Stolz assigns to each such function f a *moment*, $\mathfrak{u}(f)$, and defines an ordering on the moments by stipulating that for each pair of such functions f and g:

$$\mathfrak{u}(f) > \mathfrak{u}(g), \text{ if }, 1 < \lim_{x \to a+0} f(x)/g(x) \le +\infty,$$

$$\mathfrak{u}(f) = \mathfrak{u}(g), \text{ if }, \lim_{x \to a+0} f(x)/g(x) = 1,$$

$$\mathfrak{u}(f) < \mathfrak{u}(g), \text{ if } 0 \le \lim_{x \to a+0} f(x)/g(x) < 1.$$

To complete the construction, sums and products of moments and subtraction of moments for which $\mathfrak{u}(f) > \mathfrak{u}(g)$ are defined by the rules:

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[35] [Stolz 1885, pp. 205–206]: "Man sagt, dass eine Veränderliche x unendlich klein wird, wenn sie den Grenzwerth 0 hat. Ferner: Ist lim $f(x) = 0$ bei lim $x = a$, so wird $f(x)$ zugleich mit $x - a$ unendlich klein. Dabei kann der Quotient $f(x) : (x - a)$ einen endlichen Grenzwerth b haben .... Das gilt insbesondere von den Quotienten $\{g(x) - g(a)\} : (x - a)$, worin $g(x)$ eine für $x = a$ stetige Function bedeutet, –mit deren Betrachtung die Differentialrechnung sich beschäftigt .... Da aber der Nenner $x - a$ für $x = a$ Null ist, so verliert dieser Quotient für $x = a$ jede Bedeutung. Soll dennoch unter den obwaltenden Umständen die Zahl b durch eine Division entstehen, so können Dividend und Divisor nicht reelle Zahlen sein, sondern müssen etwas wesentlich davon Verschiedenes, zum Rechnen geeignete Grössen einer neuen Art sein."

$$\mathrm{u}(f) + \mathrm{u}(g) = \mathrm{u}(f + g);$$

$$\mathrm{u}(f) \cdot \mathrm{u}(g) = \mathrm{u}(f \cdot g);$$

$$\mathrm{u}(f) - \mathrm{u}(g) = \mathrm{u}(f - g).$$

To establish the non-Archimedean nature of the system, Stolz implicitly appeals to the fact that there are pairs f, g of the said functions (e.g., $f(x) = x^2$ and $g(x) = x$) such that $\lim_{x \to a+0} f(x)/g(x) = 0$, and observes that when $\lim_{x \to a+0} f(x)/g(x) = 0$, it is also the case that $\lim_{x \to a+0} pf(x)/g(x) = 0$ for each positive integer p; in such instances while $\mathrm{u}(f) < \mathrm{u}(g)$, it is nevertheless the case that $p\mathrm{u}(f) = \mathrm{u}(f) < \mathrm{u}(g)$ for each positive integer p [1884, p. 30; 1885, p. 210].

In modern parlance, Stolz's proof of the non-Archimedean nature of his systems of moments shows that $\mathrm{u}(f)$ is infinitesimal relative to $\mathrm{u}(g)$, if $\lim_{x \to a+0} f(x)/g(x) = 0$; in fact, for $\mathrm{u}(f)$ to be infinitesimal relative to $\mathrm{u}(g)$ it is also necessary that $\lim_{x \to a+0} f(x)/g(x) = 0$. While Stolz does not expressly unpack the import of his condition in these terms, its import is implicit in a number of critical junctures in his discussions of his moments including the following where the issue of the absorptive nature of his addition first arises.

Since the additive structures of Stolz's systems of moments are not regular, they can not be systems of absolute magnitudes in Stolz's sense. However, rather than appeal to lack of regularity to establish this, Stolz shows that they satisfy all the axioms for a system of absolute magnitudes with the exception of two: in place of $A + B > A$, they satisfy $A + B \geq A$; and in place of $A + B > A' + B'$ if $A > A'$ and $B = B'$, they satisfy $A + B \geq A' + B'$ if $A > A'$ and $B = B'$. More specifically, Stolz [1884, p. 29; 1885, p. 208] shows:

$\mathrm{u}(f) + \mathrm{u}(g) \geq \mathrm{u}(f)$, it being the case that $\mathrm{u}(f) + \mathrm{u}(g) = \mathrm{u}(f)$ if and only if

$\lim_{x \to a+0} g(x)/f(x) = 0$; and

$\mathrm{u}(f) + \mathrm{u}(g) \geq \mathrm{u}(f_1) + \mathrm{u}(g_1)$ if $\mathrm{u}(f) > \mathrm{u}(f_1)$ and $\mathrm{u}(g) = \mathrm{u}(g_1)$, it being

the case that $\mathrm{u}(f) + \mathrm{u}(g) = \mathrm{u}(f_1) + \mathrm{u}(g_1)$ if and only if

$\lim_{x \to a+0} f(x)/g(x) = 0$ and $\lim_{x \to a+0} f_1(x)/g_1(x) = 0$.

In virtue of our earlier remarks, the first of the above two conditions may be interpreted as asserting: $\mathrm{u}(f) + \mathrm{u}(g) \geq \mathrm{u}(f)$, it being the case that $\mathrm{u}(f) + \mathrm{u}(g) = \mathrm{u}(f)$ if and only if $\mathrm{u}(g)$ is infinitesimal relative to $\mathrm{u}(f)$; and the second condition may be interpreted similarly.

In the system of moments of functions thus constructed division is only possible in isolated cases that collectively are not adequate to model the quotients with which Stolz is concerned. To overcome this limitation, Stolz extends the system using techniques from the analytic theory of rational numbers beginning with the introduction of new

elements as follows [1884, pp. 33–34; 1885, pp. 211–212]. For each pair of moments $\mathfrak{u}(f)$ and $\mathfrak{u}(g)$ where $\lim\limits_{x \to a+0} f(x)/g(x) = r$ for some real number $r > 0$ or for $r = +\infty$ a new element is formally introduced as the ratio, $\mathfrak{u}(f) : \mathfrak{u}(g)$, of the given moments in compliance with the equation

$$\mathfrak{u}(g) \{\mathfrak{u}(f) : \mathfrak{u}(g)\} = \mathfrak{u}(f).$$

This in essence sets $\mathfrak{u}(f) : \mathfrak{u}(g) = r$ (up to equivalence). Thus introduced, the new elements are ordered according to the condition

$$\mathfrak{u}(f) : \mathfrak{u}(g) \gtreqless \mathfrak{u}(f_1) : \mathfrak{u}(g_1) \text{ if } \mathfrak{u}(f) \cdot \mathfrak{u}(g_1) \gtreqless \mathfrak{u}(f_1) \cdot \mathfrak{u}(g);$$

or, what is equivalent,

$$\mathfrak{u}(f) : \mathfrak{u}(g) \gtreqless \mathfrak{u}(f_1) : \mathfrak{u}(g_1) \text{ if } \lim\limits_{x \to a+0} f(x)g_1(x)/f_1(x)g(x) \gtreqless 1.$$

The ordering of the old elements in the extended system is understood to remain the same, and by appealing to the fact $\mathfrak{u}(f) > \mathfrak{u}(g)$ when $\lim\limits_{x \to a+0} f(x)/f_1(x)g(x) = +\infty$, Stolz observes that each new element $\mathfrak{u}(f) : \mathfrak{u}(g)$ is greater than each moment $\mathfrak{u}(f_1)$, thereby rendering the moments–which he describes as "infinitely small"–infinitesimal relative to the finite elements (insofar as the extended system contains all the positive rational numbers). To complete the construction, sums and products are defined by the following pairs of rules, the first parts of which apply to sums and products of new elements with old elements and the latter parts of which apply to sums and products of new elements:

$$\mathfrak{u}(f) : \mathfrak{u}(g) + \mathfrak{u}(h) = \mathfrak{u}(f + gh) : \mathfrak{u}(g),$$

$$\mathfrak{u}(f) : \mathfrak{u}(g) + \mathfrak{u}(f_1) : \mathfrak{u}(g_1) = \mathfrak{u}(fg_1 + f_1g) : \mathfrak{u}(gg_1).$$

$$\{\mathfrak{u}(f) : \mathfrak{u}(g)\} \cdot \mathfrak{u}(h) = \mathfrak{u}(fh) : \mathfrak{u}(g),$$

$$\{\mathfrak{u}(f) : \mathfrak{u}(g)\} \cdot \{\mathfrak{u}(f_1) : \mathfrak{u}(g_1)\} = \mathfrak{u}(ff_1) : \mathfrak{u}(gg_1).$$

   In the system thus obtained division is possible when (and only when) $\lim\limits_{x \to a+0} f(x)/ g(x) \neq 0$; in particular, the derivative $F'(a)$ is equal to the ratio $\mathfrak{u}(f) : \mathfrak{u}(g)$ of infinitesimal magnitudes where $f(x) = F(a+x) - F(a)$ and $g(x) = x - a$. Moreover, extending one of the already cited results governing the addition of moments, Stolz shows that in the extended system: "A finite magnitude [i.e., a real number] remains unchanged when an infinitely small one [i.e., a moment] is added to it" [1885, p. 212].

4. Killing's "proof" of the impossibility of infinitesimal line segments

Given the soundness of Stolz's various constructions of non-Archimedean systems of magnitudes along with the cogency of his proofs of their non-Archimedean natures, one would think that the existence of non-Archimedean ordered algebraic systems would have been universally embraced by Stolz's contemporaries. However, while the vast majority of Stolz's contemporaries do indeed appear to have embraced their existence, there was of course at least one important exception, and a rather vocal one at that, Georg Cantor.[36] However, two years before Cantor published his attempted proof of the impossibility of such systems a more narrowly focused such "proof" appeared in Wilhelm Killing's treatise *Die Nicht-Eukidischen Raumformen in analytischer Behandlung* [1885]. Although Killing's argument is explicitly concerned with the system of nondirected line segments of a Euclidean space, its general structure is applicable to any non-Archimedean system of magnitudes that is either absolute in Stolz's sense or that differs from such a system by the failure of divisibility.

Killing's "proof" occurs as part of his more general attempt to establish the possibility and uniqueness of measurement of Euclidean magnitudes by real numbers. "For line segments," says Killing,

> this proof rests on the assertion:
>
> Either two segments can be brought into coincidence or the first is congruent to a part of the second, or the second is congruent to a part of the first; these three cases being mutually exclusive . . . .
>
> It should be mentioned . . . that in accordance with this proposition, whose proof depends on the axiom of the circle [i.e., Euclid's third postulate: *a circle may be described with any center and distance*], segments are compared to be equal, greater and smaller. Before we prove the possibility and uniqueness of measurement, however, it is appropriate to consider two propositions, each of which is a direct consequence of the other, and each of which following the designation of Mr. Stolz is named the Axiom of Archimedes:
>
> If the points B and C lie in the same direction from A, and C does not belong to the segment AB, one can compose from a finite number of parts, each of which is equal to AB, a segment AD, that contains the point C, and conversely under the same assumption the segment AC can be decomposed into a finite number of equal parts, so that at least one point of division lies between A and B.
>
> If the first part of the sentence [i.e., the Archimedean Axiom] is assumed to be incorrect . . . there would have to be in the direction of AB a point R such that a finite number of multiples taken of AB would not pass beyond R, though they would pass over each point situated between A and R. Now choose on AR a segment $SR = AB$; since in virtue of the assumption of the addition of the segment to itself one arrives at a point that lies between S and R, and hence also by the same [finite] process beyond R, the presumption of a bound [having the properties attributed to R] is not permitted, whereby the proof is produced.

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[36] While we suspect Cantor was not alone, we are not aware of any other of Stolz's contemporaries who explicitly criticized Stolz's systems.

$$A......B..........C...........S......R$$

Figure 1

Hereafter the measurement of line segments offers no difficulty; it amounts to the same process which leads in analysis from the number one to all the real numbers.[37]

In other words, according to Killing, if the Archimedean axiom fails, there should be a point R (see Fig. 1) such that no finite multiple of (A, B) should surpass (A, R), while for each point C contained strictly between A and R, there is a finite multiple n of (A, B) such that $n(A, B) \geq (A, C)$. Moreover, by appealing to the "axiom of the circle," there is a segment SR on AR such that SR is congruent to AB. But then, by hypothesis, there is a positive integer n such that $n(A, B) \geq (A, S)$, from which it follows that $(n + 1)(A, B) \geq (A, R)$, contrary to hypothesis.

Unlike Cantor's purported proof, Killing's does not appear to have been discussed or even referred to in the literature outside certain writings of Veronese [1889, p. 603; 1891, p. 132; 1894, p. 705; 1896, p. 424]. It is quite possible that among the reasons for this is that, unlike Cantor's "proof," Killing's is neither shrouded in obscurity nor appeals to unstated theorems, which enabled the reader to unambiguously interpret the challenge and readily isolate the limitations thereof. In fact, in his first contribution to

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[37] [Killing 1885, pp. 46–47]: "Dieser Nachweis gründet sich für gerade Stecken auf den Satz: Zwei Strecken lassen sich entweder zur Deckung bringen oder die erste ist einem Teil der zweiten, oder die zweite einem Teile der ersten kongruent; diese drei Fälle schliessen sich vollständig aus; wenn also durch irgend eine Bewegung die eine Strecke mit der andern zur Deckung gebracht werden kann, so ist es nicht möglich, sie durch eine andere Bewegung in Deckung zu bringen mit einem Teile der zweiten.

Dieser Satz, dessen Nachweis sich, wie beiläufig bemerkt werden soll, auf das Axiom des Kreises stützt, gestattet, Strecken nach gleich, grösser und kleiner zu vergleichen. Ehe wir nun die Möglichkeit und Eindeutigkeit der Messung beweisen, schicken wir zwei Sätze voraus, von denen jeder eine unmittelbare Folge des andern ist und welche also lauten (Axiom des Archimedes, nach der Bezeichnung des Herrn Stolz):

Liegen die Punkte B und C von A aus in derselben Richtung und gehört C der Strecke AB nicht an, so lässt sich aus einer endlichen Anzahl von Teilen, deren jeder gleich AB ist, eine Strecke AD zusammensetzen, welche den Punkt C enthält, und umgekehrt kann man unter derselben Voraussetzung die Strecke AC derartig in eine endliche Zahl gleicher Teile zerlegen, so dass mindestens ein Teilpunkt zwischen A und B liegt.

Angenommen, der erste Teil des Satzes sei nicht richtig und man könne nicht durch fortgesetzte Bildung einer neuen Strecke aus Teilen, welche sämtlich einer gegebenen Strecke AB gleich sind, zu einer neuen Strecke gelangen, welche grösser ist als eine zweite gegebene Strecke, so müsste sich auf der Richtung AB ein Punkt R finden, so dass Vielfache von AB in endlicher Zahl genommen, nicht über R hinausführen, während sie über jeden zwischen A und R gelegenen Punkt führen. Nun wähle man auf AR eine Strecke $SR = AB$; da man nach der Voraussetzung durch Addition der Strecke AB zu sich selbst zu einem Punkte gelangt, welcher zwischen S und R liegt, so führt derselbe Prozess auch über R hinaus, und die Annahme einer Grenze ist nicht gestattet, wodurch der Beweis erbracht ist.

Hiernach bietet die Messung der geraden Strecke keine Schwierigkeit; dieselbe kommt auf denjenigen Prozess hinaus, welcher in der Analysis aus der Eins zu den sämtlichen reellen Zahlen führt."

non-Archimedean mathematics Veronese apparently felt sufficiently at ease to simply dismiss the argument as circular [1889, p. 603]. He did, however, return to Killing's challenge in his *Fondamenti di Geometria* [1891; 1994], where he attempted to draw attention to the source of the circularity.

To fully appreciate the contents of Veronese's critique we require the idea of a line segment (AA_1) that is *infinitesimal relative to* a line segment (BB_1), a conception that was developed in Veronese's *Fondamenti di Geometria*.[38] For the purpose at hand, however, it suffices to note that according to Veronese's definitions a segment (BB_1) is *infinite with respect to* a segment (AA_1) if $n(AA_1) < (BB_1)$ for each positive integer n, and (AA_1) is *infinitesimal with respect* to (BB_1), if (BB_1) is infinite with respect to (AA_1). Moreover, to say that a segment of a geometrical space is *infinitesimal* is to say that it is infinitesimal with respect to another segment of the space; that is, the idea of an infinitesimal segment for Veronese is a relativized notion.

Having introduced these conceptions earlier in his text [1891, pp. 86–87], Veronese begins his critique of Killing's argument by noting that: "He [i.e. Killing] says, if the first property [i.e., the Archimedean Axiom] is not true, there should be a point R such that no finite multiple of (A, B) should surpass (A, R), *while it reaches every point contained between A and R*" [1891, p. 132]. Soon thereafter he goes on to add that: "this hypothesis [i.e., the above assertion italicized by Veronese] contained in the proof of Killing, which excludes infinitesimal segments, is, in essence, that which he wants to prove" [1891, p. 132]. In other words, Veronese appears to be saying that insofar as Killing is assuming that for each point C lying strictly between A and R there is a finite multiple n of (A, B) such that $n(A, B) \geq (A, C)$, Killing is tacitly assuming that (A, B) is not infinitesimal relative to (A, R)–"that which he wants to prove" [1891, p. 132].

Veronese is, in fact, on firm ground here; for given any points A, B and C on a line (of an *elementary Euclidean space*)[39] where B lies strictly between A and C, (A, B) is *not* infinitesimal relative to (A, R) if and only if for each point C contained strictly between A and R there is a finite multiple n of (A, B) such that $n(A, B) \geq (A, C)$. Indeed, if (A, B) were infinitesimal relative to (A, R), then (A, B) likewise would be infinitesimal relative to (A, S), and so there would be no positive integer n such that $n(A, B) \geq (A, S)$, contrary to Killing's assumption.

Whether Veronese's remarks on Killing's "proof" had any influence on Killing's thinking on the matter, we are in no position to say; however, within a decade of its publication Killing implicitly renounced his purported proof of the Archimedean axiom, as Veronese himself happily observed [1896, p. 424]. Indeed, in a series of works appearing in the 1890s [1895–96; 1897; 1898], Killing refused to rule out the possibility of a

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[38] Also see the discussion below of the Appendix to [Vivanti 1891a] for another source of the idea of infinitesimal relative to. In addition, see [Abeles 2001] for a discussion of Charles L. Dodgson's use of this notion in his [1888; 1890].

[39] By an elementary (2-dimensional) Euclidean space, the reader may understand a model of Hilbert's axioms of *order, incidence*, *congruence* and *parallelism* for classical Euclidean geometry, that is, all of Hilbert's axioms for classical Euclidean geometry less his continuity axioms (which includes the Archimedean axiom). An elegant alternative axiomatization and algebraico-metamathematical investigation of these spaces was provided by Tarski. For references and a discussion of a wide variety of non-Archimedean models of these spaces, see [Ehrlich 1997a].

non-Archimedean geometry despite the fact that he was critical of aspects of Veronese's contributions to the subject. Moreover, while he does not appear to have ever publicly withdrawn his criticisms of Veronese's work, in his *Handbuch des Mathematischen Unterrichts* [1910, pp. 25, 39] (co-authored with H. Hovestadt) Killing unequivocally embraced the non-Archimedean geometries of Hilbert [1899] and Dehn [1900].

Before concluding our discussion of Killing's argument, however, it will be instructive to consider a non-Archimedean ordered algebraic system in which Killing's "hypothesis" holds. Besides helping to bring into sharper focus the weakness of Killing's argument, this will also provide us with the opportunity to introduce a number of concepts that will play a role in the subsequent discussion.

In the second part of his *Beiträge zur Begründung der transfiniten Mengenlehre* (Contributions to the Founding of Transfinite Numbers), Cantor showed that every non-zero ordinal $\alpha < \omega_1$ has a *normal form*, i.e. a unique representation of the form

$$\omega^{\alpha_0} n_0 +_c \omega^{\alpha_1} n_1 +_c \ldots +_c \omega^{\alpha_i} n_i$$

where $i, n_0, n_1, \ldots, n_i$ are positive integers, $\alpha_0, \alpha_1, \ldots, \alpha_i$ is a descending sequence of ordinals, and $+_c$ is the familiar Cantorian sum of ordinals [1897, p. 237; 1915, p. 187]; and soon thereafter, Hessenberg extended the result to non-zero ordinals more generally [1906, § 72]. It follows from Hessenberg's result that by suitably expanding their respective normal forms with "dummy terms" having zeros for coefficients, every pair of non-zero ordinals α and β may be uniquely represented in the following fashion

$$\alpha = \omega^{\gamma_0} n_0 +_c \omega^{\gamma_1} n_1 +_c \ldots +_c \omega^{\gamma_k} n_k, \quad \beta = \omega^{\gamma_0} m_0 +_c \omega^{\gamma_1} m_1 +_c \ldots +_c \omega^{\gamma_k} m_k$$

where $\gamma_0, \gamma_1, \ldots, \gamma_k$ is the unique decreasing sequence formed from the union of the sets of exponents in the normal forms of α and β. Using the above, Hessenberg defined what he called the *natural sum* of α and β, henceforth written $+_H$, by the rule

$$\alpha +_H \beta = \omega^{\gamma_0}(n_0 + m_0) +_c \omega^{\gamma_1}(n_1 + m_1) +_c \ldots +_c \omega^{\gamma_k}(n_k + m_k)$$

where $n_0 + m_0, n_1 + m_1, \ldots, n_k + m_k$ are the familiar sums of non-negative integers [Hessenberg 1906, § 75]. If $\alpha = 0$ or $\beta = 0$, then $\alpha +_H \beta = \beta$ or $\alpha +_H \beta = \alpha$, respectively. The natural sum of ordinals, or the *Hessenberg sum* of ordinals, as it is frequently called, stands in sharp contrast to the familiar Cantorian sum. For example, unlike the Cantorian sum, the Hessenberg sum is commutative and strictly monotonic with respect to the standard ordering of ordinals. Moreover, its existence together with the existence of Hausdorff's closely related *natural product* of ordinals [1927, pp. 68–69; 1957, pp. 80–81] contradict Cantor's long held view that not only were his operations on ordinals the only legitimate such operations, but "the laws governing them [i.e. the Cantorian laws] can be derived from immediate inner intuition with apodictic certainty" [Cantor 1883 in 1932, p. 170; Cantor 1883 in Ewald 1996, p. 886]. For non-zero ordinals α and β, Hausdorff's natural product, $\alpha \cdot_H \beta$, is obtained by summing together *à la* Hessenberg all terms of the form

$$\omega^{\alpha_s +_H \beta_t} n_s m_t,$$

where $\omega^{\alpha_s} n_s$ and $\omega^{\beta_t} m_t$ are component terms from the respective normal forms

$$\omega^{\alpha_0} n_0 +_c \omega^{\alpha_1} n_1 +_c \ldots +_c \omega^{\alpha_i} n_i, \quad \omega^{\beta_0} m_0 +_c \omega^{\beta_1} m_1 +_c \ldots +_c \omega^{\beta_k} m_k$$

of α and β, and $n_s m_t$ is the familiar product of the positive integers n_s and m_t. If $\alpha = 0$ or $\beta = 0$, then $\alpha \cdot_H \beta = 0$. Unlike the Cantorian product, the natural product is commutative and satisfies both distributive laws (with respect to natural addition). As a result of a generous remark by Hausdorff [1927, pp. 68–69; 1957, pp. 80–81], Hausdorff's natural product is frequently called "the Hessenberg product."[40]

Returning now to Killing's argument, let $\langle K, +_K, <_K \rangle$ be the structure consisting of the set K of all formal sums $\alpha \oplus r$ (where α is an ordinal $< \omega^2$, and r is a positive real number if $\alpha = 0$ and a nonnegative real number, otherwise)[41] with $+_K$ and $<_K$ defined by the conditions:

$$(\alpha \oplus r) +_K (\alpha' \oplus r') = (\alpha +_H \alpha') \oplus (r + r')$$

$$(\alpha \oplus r) <_K (\alpha' \oplus r'), \text{ if either } \alpha < \alpha' \text{ or } \alpha = \alpha' \text{ and } r < r'$$

where $\alpha +_H \alpha'$ is the Hessenberg sum of ordinals, $r + r'$ is the familiar sum of reals, and $\alpha < \alpha'$ and $r < r'$ are the familiar ordering relations between ordinals and real numbers, respectively. It is a simple matter to show that $\langle K, +_K, <_K \rangle$ is a continuously ordered system that satisfies all of Stolz's axioms for an absolute system of magnitudes except for divisibility and right-solvability, the latter being Stolz's condition (xii). For example, though $\omega \in K$ there is no $X \in K$ such that $X + X = \omega$ (in violation of divisibility), and while $1 < \omega$, there is no $X \in K$ such that $1 + X = \omega$ (which violates right-solvability). On the other hand, insofar as K contains an element x, namely ω, such that for each $y < x$, x is the least element of K that is infinitely large relative to y, it follows that Killing's "hypothesis" is satisfied by the members of K less than x, that is: for all $a, b \in K$ where $a < b < \omega$, there is a positive integer n such that $na > b$, and

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[40] Actually, the situation is worse for Cantor's view than the existence of the Hessenberg sum and Hausdorff product of ordinals suggest. Building on the work of Hessenberg and Hausdorff, Carruth [1942] later defined general conceptions of natural sums and natural products of ordinals and showed that the operations of Hessenberg and Hausdorff are distinguished instances of these general classes of operations on ordinals.

Although natural sums and products of ordinals have been fixtures in textbooks on classical set theory throughout the twentieth century (cf. [Hausdorff 1927, pp. 68–69; 1957, pp. 80–81; Bachmann 1967, pp. 107–111; Sierpinski 1965, pp. 323–324; Kuratowski and Mostowski 1968, pp. 259–260; Fraenkel 1976, pp. 214–215; and Levy 1979, p. 130]) their existence and history, let alone their significance, has been overlooked by historians and philosophers of mathematics, alike. In a separate paper we intend to rectify this matter. For the time being, we note that their properties have been carefully investigated in a number of works including [Carruth 1942] and [Zuckerman 1973], and their significance for the theory of ordered algebraic systems was brought to the attention of the mathematical community by Carruth [1942], Sikorski [1948], Klaua [1959/1960] and Sankaran and Venkataraman [1962]. Today these operations play an important role in the study of surreal numbers (cf. [Conway 1976, p. 28; Ehrlich 2001; and van den Dries and Ehrlich 2001]).

[41] Formally speaking, a formal sum $\alpha \oplus r$ is an ordered pair (α, r) written as a sum.

$ma < \omega$ for all $a < \omega$ and all positive integers m. Indeed, it is not difficult to see that for any non-Archimedean structure that satisfies all of Stolz's axioms for an absolute system of magnitudes save divisibility and right-solvability the existence of such an element x is both a necessary and a sufficient condition for the satisfaction of Killing's "hypothesis." It is also not difficult to see that the addition of right-solvability and/or divisibility is sufficient to preclude the existence of such an element x. Killing, of course, realized that the geometrical formulation of solvability is incompatible with the existence of such an element x–this was the basis from which he derived his contradiction.[42] Where he went astray was in assuming that the denial of the Archimedean condition implied the existence of such an x. An assumption analogous to Killing's "hypothesis" appears to have also played a role in leading Cantor astray in at least one of his early criticisms of infinitesimals. It is to these early criticisms of Cantor to which we now turn.

5. Cantor's early antipathy to infinitesimals

Unlike Killing, Cantor appears to have remained an opponent of infinitesimals of one sort or another throughout most of his life. Already in a letter to Dedekind dated December 29, 1878, Cantor described the idea of "numbers . . . that are smaller than every conceivable [positive] real number, yet different from zero" as "*horribile dictu*," i.e., "*too horrible to even say*" [Meschkowski and Nilson, 1991, p. 50]; and four years later, in the third part of his *Über unendliche lineare Punktmannigfaltigkeiten* (Infinite Linear Point Manifolds) [1882 in 1932, p. 156], he reiterated his doubts in print, albeit in a more moderate form. The following year he developed the latter qualms further in his *Grundlagen einer allgemeinen Mannigfaltigkeitslehre* (Foundations of a General Theory of Manifolds [1883]). The remarks from his *Grundlagen* take on added significance because they also show that contrary to what is sometimes asserted (cf. [Dauben 1979, pp. 233–236; Fisher 1981, p. 118]) Cantor was not a lifelong opponent of infinite numbers other than his cardinal and ordinal numbers. While it is certainly true that Cantor vigorously attacked and ultimately rejected the systems of infinite and/or infinitesimal numbers put forth by Stolz, du Bois-Reymond, Thomae and Veronese, and, by the 1890s, apparently in response to the challenge posed by Veronese's infinite numbers, he seemed to suggest that his cardinals and ordinals were the only legitimate infinite numbers [cf. Cantor November 17, 1890; Cantor April 5, 1895; Cantor July 27, 1895; Cantor 1895a, pp. 300–301; Cantor 1915, p. 117], for at least a period of time during the 1880s he appears to have been a bit more open minded about the possibility of other infinite numbers as is evident from the following remark from his *Grundlagen* with which he broaches the topic of infinitesimals.

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[42] The Euclidean proposition that immediately implies the geometric version of solvability is Proposition 3 of Book 1 of Euclid's *Elements*: *Given two unequal straight lines, to cut off from the greater a straight line equal to the less*. As Killing intimates in the passage quoted above, this proposition is proved in the *Elements* using Euclid's third postulate: *a circle may be described with any center and distance*.

The extended sequence of integers [i.e., the ordinals] can, if one wishes, be completed without further ado into a continuous set of numbers by adjoining to every integer all real numbers that are greater than zero and less than one.

Perhaps at this point the question will arise whether, since in this manner we have achieved a determinate extension of the domain of real numbers into the infinitely large, one cannot with equal success define determinate infinitely small numbers, or, what might come to the same thing, define finite numbers which do not coincide with the rational and irrational numbers (which later appear as the limiting values of the sequence of rational numbers), but which might be inserted into supposed gaps amidst the real numbers, just as the irrational numbers are inserted into the chain of the rational numbers, or the transcendental numbers into the structure of the algebraic numbers. [Cantor 1883 in Cantor 1932, pp. 171–172; Cantor 1883 in Ewald 1996, p. 887; (Translation Ewald)]

In modern parlance, Cantor is considering the lexicographically ordered class of all "numbers" of the form $\alpha + x$ where α is an ordinal and x is a real number such that $0 < x < 1$.[43,44] Henceforth, we shall refer to this structure as *Cantor's transfinite line* and designate it by "L_c." We suspect it is no accident that he regarded this system as a *completion* of the ordinals since it is the richest totally ordered system of "numbers" containing the ordinals he appears to have ever embraced; and even this, as we noted, may have been relatively short-lived. As to the idea of expanding the system (and hence the reals) by inserting infinitesimals "into supposed gaps amidst the real numbers," however, he was already prepared to express doubts. Indeed, as he continues:

The question of the establishment of such interpolations, on which some authors have expended much effort, can, in my opinion, only be clearly and distinctly answered with the help of our new numbers–in particular, with the general concept of the Anzahl [number] of well-ordered sets. The previous attempts, it seems to me, partly rest on an erroneous confusion of the improper infinite [the potential infinite] with the proper infinite [the actual infinite], and partly have been constructed on a thoroughly insecure and unstable foundation.

The improper infinite has often been called by recent philosophers a 'bad' infinite, in my opinion unjustly, since it has proved itself to be a very good and highly useful instrument in mathematics and the natural sciences. The infinitely small quantities have, so far as I know, until now in general been usefully developed *only* in the form of the improper-infinite, and are thus capable of all those differences, modifications, and relations which are found in infinitesimal analysis and in the theory of functions, and which are used to establish the rich profusion of analytic truths. But all attempts to force this infinitely small into a *proper* infinite must finally be given up as pointless. If proper-infinitely small quantities exist at all, that is, are definable, then they certainly stand in no

[43] The reader will notice that in the passage from [Cantor 1883] quoted above, Cantor refers to this collection as a "set." Indeed, it was not until the end of the 1890s that Cantor came to the conclusion that the collection of ordinals is *not* a set [cf. Hallett 1884, Ch. 4].

[44] Cantor's observation that the just-cited structure is continuous extends to an arbitrary lexicographically ordered class consisting of all elements of the form (α, x) where α is an element of a non-empty well-ordered class and x is a real number such that $0 < x < 1$ (cf. [Kelly 1955, p. 64]). Formally speaking, sums of the form "$\alpha + x$" are such ordered pairs written as formal sums (see Note 41).

direct relationship to the familiar quantities which *become* infinitely small. [Cantor 1883 in Cantor 1932, p. 172; Cantor 1883 in Ewald 1996, pp. 887-888; (Translation Ewald)]

Soon thereafter, at least in his published work, Cantor grew even more resolute in his opposition to infinitesimals. In his review of Hermann Cohen's *Das Princip der Infinitesimalmethode und seine Geschichte* [1883] he unabashedly asserted that "the so-called infinitely small numbers or differentials do not belong to the sphere of the being" [1884, p. 267][45]; and in his *Über die verschiedenen Standpunkte in bezug auf das aktuelle Unendliche*, which is an excerpt of a letter to G. Eneström dated November 4, 1885, he declared that they are "illegitimate actual infinities" [1886 in 1932, p. 374]. One could therefore appreciate just how taken aback Cantor must have been when Benno Kerry, in his review of Cantor's *Grundlagen einer allgemeinen Mannigfaltigkeitslehre*, not only embraced actual infinitesimal numbers but suggested that they could be defined using Cantor's own transfinite ordinals. As Kerry put it:

> in my opinion a *formal definition* of definite, infinitely small numbers is indeed given in fixing the greatest of such numbers as one which produces the sum 1 by adding itself to itself ω times; the next smaller is then the one which produces 1 by adding itself to itself $\omega + 1$ times, etc. The definite, infinitely small numbers would accordingly be denoted as:

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[45] On the other hand, in the following passage from a letter to Cohen's disciple, Kurt Lasswitz, dated December 27, 1884, Cantor appears to take a more cautious position–one closer to the position just quoted from [Cantor 1883]. Indeed, writes Cantor:

> This does not preclude that in a later state of analysis means can be found to define *different* quantities that would deserve the name infinitely small quantities because they would be smaller than the quantities used until now; however, these properly infinitely small quantities will surely bear no relationship to our differentials.

> [Cantor, December 27, 1884]: "Damit ist nicht ausgeschlossen, dass in einem späteren Zustand der Analysis Mittel gefunden werden können, *verschiedene* Grössen zu definiren, die den Namen: unendlichkleine Grössen verdienen, weil sie kleiner wären, als jede der bisher gebrauchten Grössen; diese eigentlich-unendlichkleinen Grössen werden aber dann sicherlich in keiner Beziehung zu unseren Differentialen stehen."

Detlef Laugwitz has raised the following as a possible explanation for the moderate tone of Cantor's letter to Lasswitz.

> Hermann Cohen had just published his book [Cohen 1883] on the principle of the infinitesimal method, and in his letters to Lasswitz Cantor refers to this book. Cohen (1842–1918), the founder of the neo-Kantian school at Marburg, was one of the outstanding philosophers of the time, and Lasswitz was one of his followers. Cantor may have felt it advisable to be on friendly terms with Lasswitz who could serve as a mediator between philosophy and the sciences. Also, he was the author of influential popular books on foundational questions of mathematics. All this may explain the rather moderate tone in the letters to Lasswitz. [Laugwitz 2002, p. 111]

$$\frac{1}{\omega}, \frac{1}{\omega+1}, \ldots, \frac{1}{2\omega}, \ldots, \frac{1}{\omega^2}$$

etc.[46]

6. Cantor's argument against the possibility of infinitesimals of the form $\dfrac{1}{\omega}$

Judging from the picture that emerges from Cantor's published works together with the now published portions of his *Nachlass*, it may very well have been in response to Kerry's just-cited contention that Cantor began to work out his purported proof of the impossibility of actual infinitesimals. Indeed, in a letter written by Cantor to Kerry, dated February 4, 1887, we find what appears to be a forerunner to Cantor's published "proof" worked out for the special case where the purported infinitesimal has the form $1/\omega$. Like the published "proof," the former "proof" makes use of the concept of a "linear magnitude," a concept, which as Cantor's critics repeatedly emphasized, he never defined (at least not in print). On the other hand, in the letter he does draw attention to those aspects of the notion that he believes are relevant to the "proof" contained therein. For this reason, and because the "proof" in the letter may contain helpful clues about how Cantor interpreted the more cryptic general "proof" that was composed soon thereafter and appeared in print that year, the letter deserves more serious attention than it has heretofore been given (see Note 57).

Cantor begins the portion of the letter regarding his "proof" by remarking:

> The proof that numerical magnitudes [Zahlgrössen] of the kind $\frac{1}{\omega}$ are self-contradictory emerges from the most minimal conditions that the idea of linear magnitudes has to satisfy, and shows that they cannot be brought in harmony with the demands expressed by the symbol $\frac{1}{\omega}$.[47]

Cantor's argument is presented in terms of a portion of his transfinite line that we shall henceforth denote "$L_{(0,\omega_1)}$." $L_{(0,\omega_1)}$ is the non-zero portion of the structure known to contemporary topologists as *the long line*, the latter being the lexicographically ordered

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[46] [Kerry 1885, p. 212]: "meines Erachtens ist eine *formelle Definition* bestimmt-unendlichkleiner Grössen schon dadurch gegeben, dass man die grösste derartige Grösse als eine solche fixirt, die ω-mal zu sich selbst adddirt als Summe: 1 ergiebt; die nächst kleinere ist dann diejenige, die $\omega+1$ -mal zu sich selbst addirt als Summe: 1 ergiebt u. s. w. Zu bezeichnen wären demnach die bestimmt-unendlichkleinen Grössen als:

$$\frac{1}{\omega}, \frac{1}{\omega+1}, \ldots, \frac{1}{2\omega}, \ldots, \frac{1}{\omega^2}$$

etc."

[47] [Cantor February 4, 1887]: "Der Beweis, daß Zahlgrössen von der Art $\frac{1}{\omega}$ sich selbst widersprechen, geht von den minimalsten Bedingungen aus, welchen lineare Grössenbegriffe zu genügen haben und zeigt, dass sie nicht in Einklang mit der in dem Symbol $\frac{1}{\omega}$ ausgedrückten Forderung zu bringen sind."

class of all numbers of the form $\alpha + x$ where α is an ordinal $< \omega_1$ and x is a real number such that $0 \leq x < 1$ (cf. [Munkres 1975, p. 159]).[48] Indeed, writes Cantor:

> Let us accept for the sake of argument the domain of the positive rationals and irrationals i.e., all of the so-called positive real magnitudes, as well as the domain of the transfinite numbers α of the second-class [the infinite ordinals $< \omega_1$] and the domain of those numbers $\alpha + x$ emerging from both (where here $x < 1$). . . .[49]

Having done so, says Cantor,

> the question therefore arises which conditions *other* positive numbers $\zeta, \zeta', \zeta'', \ldots$ necessarily have to satisfy if they may still claim the right to be called linear and stand in relation to the known magnitudes with a fixed order of greater and less than.[50]

The "*other* positive numbers" Cantor is referring to are presumably infinitesimals; and he answers his question by asserting:

> To these conditions belong in any case the following:
> 1. The addition of a finite number of such magnitudes is always possible and satisfies the associative law. In particular, finite multiples $\zeta.\nu$ of each magnitude $\zeta \neq 2^n$ [51] are possible, when ν is a finite integral *multiplier*.
> 2. Also the summand magnitudes in a prescribed ordinary infinite series $\zeta_1, \zeta_2, \zeta_3, \ldots$ must have a determined sum s, where s belongs either to the old or the expanded domain.
>
>   So that however s be the sum of that infinite series, if s' is any magnitude (known to be) smaller than s, there must be a finite whole number n such that $\zeta_1 + \zeta_2 + \ldots + \zeta_n > s'$. For if for each finite whole number n: $\zeta_1 + \zeta_2 + \ldots + \zeta_n \leq s'$, it would also have to be the case that $\zeta_1 + \zeta_2 + \zeta_3 + \ldots$ in inf. $\leq s'$, [52] which is incompatible with the assumptions that $\zeta_1 + \zeta_2 + \ldots$ in inf. $= s$ and $s' < s$.[53]

In a subsequent letter from Cantor to Kerry dated March 18, 1887 (see below), Cantor makes it clear that the above two "axioms" while stated for the "*other* positive numbers"

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[48] The long line has long been of interest to topologists since while it is path connected and locally homeomorphic to the ordered set of reals it cannot be embedded in the reals. Though apparently introduced by Veblen [1905, p. 169; 1906], it was given a wide audience through its appearance in [Huntington 1917/1955, p. 56].

[49] [Cantor February 4, 1887]: "Setzen wir das Gebiet der positiven rationalen und irrationalen d.h. aller sogen. pos. reellen Grössen als gegeben voraus, ferner desgleichen das Gebiet der transfiniten Zahlen zweiter Classe α und das Gebiet der aus jenen beiden hervorgehenden Zahlen $\alpha + x$ (hierbei kann $x < 1$ festgesetzt sein) . . . ."

[50] [Cantor February 4, 1887]: "so fragt es sich also zunächst, welche Bedingungen *andere* positive Zahlen $\zeta, \zeta', \zeta'', \ldots$ nothwendig zu genügen haben, wenn sie noch Anspruch auf die Bedeutung linearer mit den schon bekannten und unter einander in fester Beziehung des Grösser u. Kleinerseins stehenden Grössen machen sollen."

[51] The assumption that $\zeta \neq 2^n$ is both curious and puzzling, and appears to play no role whatsoever in Cantor's argument.

[52] The expression "$\zeta_1 + \zeta_2 + \zeta_3 + \ldots$ in inf. $\leq s'$" indicates that the infinite sum is $\zeta_1 + \zeta_2 + \ldots +$ is $\leq s'$. The remaining assertions in Cantor's letter employing the abbreviation "in inf." are analogously understood.

[53] [Cantor February 4, 1887]: "Zu diesen Bedingungen gehören jedenfalls folgende:

are understood to be applicable to linear magnitudes more generally. Accordingly, given a natural assumption about the compatibility of the ordering relation and the addition (see Note 7) as well as the rather minimal assumption that the sum of any two linear magnitudes is itself a linear magnitude, Cantor's first axiom implies that the collection of linear magnitudes has an ordered semigroup structure.[54] The second and more intriguing axiom, on the other hand, appears to implicitly assert that not only are ordinary infinite sums $x_1 + x_2 + x_3 + \ldots$ of positive linear magnitudes well defined, but the value of such a sum is the least upper bound of its partial sums $x_1, x_1 + x_2, x_1 + x_2 + x_3, \ldots$. Whether the least upper bound is supposed to be contained in the class of linear magnitudes or in some extension thereof Cantor does not say.[55] In any case, with the content of 2 apparently thus understood, Cantor presents his argument as follows.

Now, if $\zeta = \frac{1}{\omega}$ were a magnitude, ζ would have the characteristic that:

$$\zeta \cdot \omega = 1$$

i.e. if we set $\zeta_1, \zeta_2, \zeta_3, \ldots$ equal to ζ, i.e., if $\zeta_1 = \zeta_2 = \ldots \zeta_\nu = \ldots = \zeta$, it would be the case that:

(A) $\zeta_1 + \zeta_2 + \zeta_3 + \ldots$ in inf. $= 1$.

Now suppose s' is a magnitude between $\frac{1}{2}$ and 1, e.g., $s' = \frac{3}{4}$. Then following 2 there must be a finite whole number n such that:

(B) $\zeta_1 + \zeta_2 + \ldots + \zeta_n > \dfrac{3}{4}.$

However, because all the $\zeta_1, \zeta_2, \ldots$ are equal and are $= \zeta$, it follows from (B) that:

$$\zeta_{n+1} + \zeta_{n+2} + \ldots + \zeta_{2n} > \frac{3}{4}$$

and, consequently, that

1. Die Addition einer endlichen Anzahl jener Grössen ist stets möglich und genügt dem assoziativen Gesetze. Im besonderen ist also eine endliche Vervielfachung jeder Grösse $\zeta \neq 2^n \zeta.\nu$ möglich, worin ν ein endlicher ganzzahliger *Multiplicator* ist.
2. Auch eine einfach unendliche Reihe $\zeta_1, \zeta_2, \zeta_3, \ldots$ jener Grössen muß in der vorgeschriebenen Folge der Summanden eine bestimmte Summe s haben, wo s entweder dem alten oder dem erweiterten Gebiete angehört.
   Damit aber s die Summe jener unendlichen Reihe sei, muss, wenn s' *irgend* eine kleinere Grösse (unter den bekannten) als s bedeutet, eine endliche ganze Zahl n vohanden sein, so dass die Summe $\zeta_1 + \zeta_2 + \ldots + \zeta_n > s'$. Denn wäre für *jede* endliche ganze Zahl n: $\zeta_1 + \zeta_2 + \ldots + \zeta_n \leq s'$, so müsste auch $\zeta_1 + \zeta_2 + \ldots$ in inf. $\leq s'$ sein, was mit den Annahmen $\zeta_1 + \zeta_2 + \ldots +$ in inf. $= s$ und $s' < s$ nicht verträglich ist."
   [54] When addition is defined in $L_{(0,\omega_1)}$ in the manner we believe Cantor intended (see below), $L_{(0,\omega_1)}$ is indeed an ordered semigroup.
   [55] It appears to be both reasonable and nonproblematic to assume that for Cantor the sum of any two linear magnitudes is itself a linear magnitude. However, as we will later see, adopting the analogous assumption in the case of infinite sums of linear magnitudes is not so clearly free of difficulties.

(C) $$\zeta_1 + \zeta_2 + \ldots + \zeta_n + \zeta_{n+1} + \ldots + \zeta_{2n} > \frac{3}{4} + \frac{3}{4} = 1\frac{1}{2}.$$

However, (C) is incompatible with (A) because it has the absurd consequence that the first $2n$ terms on the left side of (A) have a sum which is larger than $1\frac{1}{2}$ while the totality of terms has the smaller sum 1. Therefore the assumption of magnitudes of the kind $\frac{1}{\omega}$ leads to contradiction.[56,57]

In contemporary parlance, Cantor's argument appears to be concerned with a lexicographically ordered semigroup, say Γ, whose universe extends $L_{(0,\omega_1)}$ and consists of all formal sums of the form $\alpha + x + n \cdot \frac{1}{\omega}$ where either $\alpha = 0$, $x = 0$, and n is a positive integer, or α is an ordinal $< \omega_1$, x is a real number such that $0 < x < 1$, and n

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[56] [Cantor February 4, 1887]: "Würde es nun eine Grösse $\zeta = \frac{1}{\omega}$ geben, so hätte ζ die Eigenschaft, daß:

$$\zeta \cdot \omega = 1$$

d. h. es würde, wenn wir unter $\zeta_1, \zeta_2, \zeta_3, \ldots$ überall ζ verstehen, d. h. $\zeta_1 = \zeta_2 = \ldots \zeta_\nu = \ldots = \zeta$ setzen, sein:

(A) $$\zeta_1 + \zeta_2 + \zeta_3 + \ldots \text{ in inf. } = 1.$$

Nehmen wir nun irgend eine Grösse s' zwischen $\frac{1}{2}$ und 1 an, z. B. $s' = \frac{3}{4}$, so muss nach 2. eine endliche ganze Zahl n vorhanden sein, so daß:

(B) $$\zeta_1 + \zeta_2 + \ldots + \zeta_n > \frac{3}{4}.$$

Weil aber alle $\zeta_1, \zeta_2, \ldots$ einander gleich und $= \zeta$ sind, so folgt aus (B), daß auch:

$$\zeta_{n+1} + \zeta_{n+2} + \ldots + \zeta_{2n} > \frac{3}{4}$$

mithin

(C) $$\zeta_1 + \zeta_2 + \ldots + \zeta_n + \zeta_{n+1} + \ldots + \zeta_{2n} > \frac{3}{4} + \frac{3}{4} = 1\frac{1}{2}.$$

Dieses Resultat (C) ist aber unvereinbar mit (A), weil es die absurde Folge hat, dass die $2n$ ersten Glieder links von (A) eine Summe haben, welche grösser ist als $1\frac{1}{2}$, während die Gesammtheit aller Glieder die kleinere Summe 1 hat. Also führt die Forderung von Grössen der Art $\frac{1}{\omega}$ auf Widerspruch."

[57] When the present paper was essentially complete we learned of the existence of [Laugwitz 2002]. Laugwitz's paper, which was published posthumously, covers some of the same material covered in the present paper and the aforementioned companion work, albeit in far less detail. With the exception of the editorial remarks contained in [Meschkowski and Nilson 1991], Laugwitz's paper is the only work we are aware of that discusses the contents of the above letter from Cantor to Kerry. Our assessment and analysis of the purported proof contained therein, however, differs very substantially from Laugwitz's. For Laugwitz's view and our critique thereof, see Appendix II.

is an integer ≥ 0. The argument is an interesting one; but contrary to Cantor's conten-
tion it does not prove that "numerical magnitudes of the kind $\frac{1}{\omega}$ are self-contradictory."
Indeed, it cannot–such numerical magnitudes are now known to be entirely coherent!
Rather, at best, the argument is a clever attempt to prove the impossibility of an ordered
semigroup extension of $L_{(0,\omega_1)}$ that both contains $\frac{1}{\omega}$ and satisfies Cantor's second axiom
for linear magnitudes. However, it rests upon a mistaken assumption–henceforth called
the Kerry-Cantor thesis– which asserts that: if $\frac{1}{\omega}$ exists then in general $\frac{1}{\omega}$ "produces the
sum 1 by adding itself to itself ω times." Indeed, it is Cantor's adoption of this thesis
in conjunction with (2), which implies that $\frac{1}{\omega} + \frac{1}{\omega} + \frac{1}{\omega} + \ldots$ (taken ω times)is equal
to the least upper bound in Γ of the partial sums $\frac{1}{\omega}, \frac{1}{\omega} + \frac{1}{\omega}, \frac{1}{\omega} + \frac{1}{\omega} + \frac{1}{\omega}, \ldots$, and it is
the latter together with the fact that the just-mentioned partial sums *have no least upper
bound in* Γ that underlies Cantor's derivation of a contradiction. Presumably, Cantor took
the Kerry-Cantor thesis to be entirely evident, being one of the "demands expressed by
the symbol $\frac{1}{\omega}$." However, to see that Cantor is mistaken about this as well as about the
supposed incoherence of the numerical magnitude $\frac{1}{\omega}$, we will now consider a structure
known to contemporary order-algebrists that invalidates both of these contentions.

Let $\mathbb{R}$ be the ordered field of real numbers, $\mathbb{Z}$ be the ordered Abelian group of integers
and $\mathbb{R}(\mathbb{Z})$ be the set of all formal series of the form

$$\sum_{\alpha < \beta} \omega^{y_\alpha} r_\alpha$$

where β is an ordinal, $\{y_\alpha : \alpha < \beta\}$ is a descending sequence of elements of $\mathbb{Z}$ and
$r_\alpha \in \mathbb{R} - \{0\}$ for each $\alpha < \beta$. The unique such series for which $\beta = 0$ (i.e., the empty
series) is the 0 of the non-Archimedean ordered field that arises by ordering the elements
of $\mathbb{R}(\mathbb{Z})$ lexicographically and defining addition and multiplication according to the rules

$$\sum_{y \in \mathbb{Z}} \omega^y a_y + \sum_{y \in \mathbb{Z}} \omega^y b_y = \sum_{y \in \mathbb{Z}} \omega^y (a_y + b_y)$$

$$\sum_{y \in \mathbb{Z}} \omega^y a_y \cdot \sum_{y \in \mathbb{Z}} \omega^y b_y = \sum_{y \in \mathbb{Z}} \omega^y \left[\sum_{\substack{(\mu,\nu) \in \mathbb{Z} \times \mathbb{Z} \\ \mu + \nu = y}} a_\mu \, b_\nu \right]$$

where terms with zeros for coefficients are inserted and deleted as needed.

$\mathbb{R}(\mathbb{Z})$, or, rather, an isomorphic copy thereof, was first introduced by Levi-Civita
[1898] in the second of his two great pioneering works on non-Archimedean geometry,
and alternative isomorphic copies thereof surfaced soon thereafter in related works of
Hilbert [1903; 1903a] and Hahn [1907: also see Ehrlich 1995]. It was Federigo Enriques,
however, in his *Sui numeri non archimedei e su alcune loro interpretazioni* (On non-
Archimedean Numbers and Some of their Interpretations) [1911b, pp. 90–96; also see
1912, pp. 472–478; 1924, pp. 367–372] who first realized that Levi-Civita's ordered field
or rather the just-described isomorphic copy thereof was a natural vehicle for incorpo-
rating an initial segment of Cantor's transfinite ordinals into a non-Archimedean ordered
field. Indeed, without mention of either normal forms or natural sums of ordinals, let
alone natural products of ordinals that had then yet to be introduced, Enriques essentially
showed that $\mathbb{R}(\mathbb{Z})$ is an extension of the lexicographically ordered semiring consisting
of all ordinals $\alpha < \omega^\omega$ (written in normal form) with sums and products defined natu-
rally. It is perhaps worth emphasizing that this now all but forgotten work of Enriques'

appeared almost four decades before Roman Sikorski's seminal paper *On an Ordered Algebraic Field* [1948], the work to which contemporary order-algebrists usually trace the insight that lexicographically ordered semirings of ordinals with sums and products defined naturally can be embedded in ordered fields. Ordered fields that are extensions of ordered semirings of ordinals so defined have received increasing attention in recent years since they naturally arise in the study of *surreal numbers* (cf. [Conway 1976; Ehrlich 2001; van den Dries and Ehrlich 2001]).

The reader will notice that in $\mathbb{R}(\mathbb{Z})$ $\frac{1}{\omega}$ (i.e., ω^{-1}) is the multiplicative inverse of $\omega = \omega^1$ and, hence, that $\frac{1}{\omega} \cdot \omega = 1$ in $\mathbb{R}(\mathbb{Z})$. On the other hand, in $\mathbb{R}(\mathbb{Z})$ the partial sums $\frac{1}{\omega}, \frac{1}{\omega} + \frac{1}{\omega}, \frac{1}{\omega} + \frac{1}{\omega} + \frac{1}{\omega}, \ldots$ do not have a least upper bound (as is evident from the fact that $\left\{ x \in \mathbb{R}(\mathbb{Z}) : \frac{n}{\omega} < x < 1/2^n = 1/2^n \omega^0 \right\} = \oslash$). Accordingly, in this system while it is indeed true that $\frac{1}{\omega} \cdot \omega = 1$, it is not true that $\frac{1}{\omega} + \frac{1}{\omega} + \frac{1}{\omega} + \ldots$ (taken ω times)=1, at least not if the infinite sum is defined in the usual manner as the limit of partial sums; indeed, if understood in the usual manner, $\frac{1}{\omega} + \frac{1}{\omega} + \frac{1}{\omega} + \ldots$ (taken ω times) is not even well defined.

This, however, should not be taken to imply that there can be no system in which $\frac{1}{\omega} \cdot \omega = 1$ and $\frac{1}{\omega} + \frac{1}{\omega} + \frac{1}{\omega} + \ldots$ (taken ω times)=1. Indeed, consider the lexicographically ordered subsemiring of $\mathbb{R}(\mathbb{Z})$ consisting of all formal series of the form

$$\sum_{\alpha < \beta} \omega^{y_\alpha} r_\alpha$$

where β is a positive integer and r_α is a positive integer for each $\alpha < \beta$. Since in this system 1 is the least upper bound of the partial sums $\frac{1}{\omega}, \frac{1}{\omega} + \frac{1}{\omega}, \frac{1}{\omega} + \frac{1}{\omega} + \frac{1}{\omega}, \ldots$, one can stipulate (without falling into logical difficulties) that within the system $\frac{1}{\omega} + \frac{1}{\omega} + \frac{1}{\omega} + \ldots$ (taken ω times)=1. Moreover, since the multiplication in the semiring is simply the multiplication in the field restricted to the just-cited members, we also have $\frac{1}{\omega} \cdot \omega = 1$.

7. Prelude to Cantor's "proof" of the impossibility of infinitesimals

As we mentioned above, Cantor's attempt to prove that "numerical magnitudes of the kind $\frac{1}{\omega}$ are self-contradictory" essentially reduces to an attempt to prove that such magnitudes are incompatible with his conception of a linear magnitude. This strategy has an obvious potential weakness, however, as several of his critics later observed; namely, it does not preclude the possibility that the existence of infinitesimals is entirely compatible with some alternative, yet entirely legitimate, conception of magnitude. Be this as it may, this was the strategy that Cantor continued to employ in his attempt to prove the impossibility of actual infinitesimal magnitudes more generally.

Roughly one month after having sent his argument to Kerry, Cantor apparently believed he had just such a general proof. In fact, by that time he apparently believed he could prove the even stronger contention that the linear numbers were restricted to the real numbers. In his letter to the Swedish mathematician and historian Gustav Eneström, dated March 6, 1887, he expressed the matter thus:

> Recently I have been successful in ascertaining an important point that long has engaged me. You will recall, that on page 8 of my "Grundlagen" I left it open whether there are actual infinitely small numbers and, more generally, if there are still other finite *linear*

numbers besides the rational and irrational numbers; by a *linear* number here is understood one which by comparison can be determined to be greater than, equal to or smaller than a real number. Despite all the *affirming* claims that have been written by J. Thomae, P. du Bois-Reymond, and O. Stolz, I was always of the view that the linear magnitudes are thoroughly completed with the familiar real numbers, and therefore that there are besides these no other linear numbers and in particular no *fixed infinitely small numbers.* Now I can prove this with the aid of transfinite numbers.[58]

A proof-sketch of the purported proof–assuming it was the same one–was first presented in letters to F. Goldscheider [Cantor May 13, 1887] and K. Weierstrass [Cantor May 16, 1887], and soon thereafter appeared in section VI of Cantor's *Mitteilungen zur Lehre vom Transfiniten* (Communications on the Theory of the Transfinite). Before examining it, however, we will first explore some further preparatory matters beginning with the contents of a passage from a letter from Cantor to Kerry written one week after the above letter to Eneström. The passage, while puzzling, is interesting because it provides us with further insight into Cantor's thinking about the concept of a linear magnitude.

Cantor begins the letter, dated March 18, 1887, by reminding Kerry "for the seventh time"

> . . . that to the nature of positive mathematical (not psychologistic) linear magnitudes not only belong comparability ($a \gtreqless b$) as you [Kerry] rightly emphasized, but also my two axioms [1 and 2 in Cantor February 4, 1887] in which is expressed unrestricted addition and subtraction (the latter of course in the sense that the smaller magnitude should be subtracted from the larger). Thus each smaller magnitude is considered part of the larger . . . .[59]

The chief insight that emerges from this passage is that Cantor took linear magnitudes to be closed under subtraction of smaller from larger elements. While not surprising in and of itself, this is closely related to the first of two puzzling aspects of this passage. Namely, though $L_{(0,\omega_1)}$ (and L_c, more generally) is an ordered semigroup that is closed

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[58] [Cantor March 6, 1887]: "In letzter Zeit ist es mir gelungen, einen wichtigen Punct festzustellen, der mich lange beschäftigt hat. Sie werden sich erinnern, dass ich in den "Grundlagen" pag. 8 unbestimmt gelassen habe, ob es eigentlich-unendlich kleine und allgemeiner ob es ausser den rationalen und irrationalen Zahlgrössen noch andere *lineare* endliche Zahlgrössen giebt; unter einer *linearen* Zahlgrösse wird hier eine solche verstanden, welche einen bestimmten Vergleich des Grösser, gleich und Kleinerseins mit den reellen Grössen zulässt. Trotz Allem, was darüber im *bejahenden* Sinne von I. Thomae, P. du Bois Reymond, O. Stolz geschrieben worden ist, war ich immer der Ansicht, dass das Gebiet der linearen Grössen mit den bekannten reellen Zahlgrössen durchaus abgeschlossen sei, dass es also ausser diesen keine anderen linearen Zahlgrössen und im Besonderen keine *bestimmt unendlich kleinen Zahlen* giebt. Jetzt kann ich dies mit Hülfe der transfiniten Zahlen beweisen."

[59] [Cantor March 18, 1887] ". . . dass zum Wesen der positiven mathematischen (nicht psychologistischen) linearen Grössen nicht nur die von Ihnen richtig hervorgehobene Vergleichbarkeit ($a \gtreqless b$) gehört, sondern auch die in meinen beiden Axiomen ausgesprochene unbeschränkte Additions- u. Subtractionsfähigkeit (letztere natürlich in dem Sinne, dass die kleinere Grösse von der grösseren abziehbar sein soll). Daher ist jede kleinere Grösse als *Theil* der grösseren zu betrachten. . . ."

under (one-sided) subtraction of smaller from larger elements, assuming the addition is defined in the manner we believe Cantor intended (see below), Cantor's remark about subtraction is misleading. The closure of subtraction of smaller from larger elements does not follow from his assumptions about addition. Consider, for example, the ordered sub-semigroup of positive real numbers consisting of all real numbers n and $n + \frac{1}{2}$ where n is a positive integer. While this system is closed under (ordinary finitary) addition, it is *not* closed under subtraction of smaller from larger elements; $\left(1\frac{1}{2} - 1\right) = \frac{1}{2}$, for example, is not a member of this semigroup. Moreover, allowing for the infinite sums expressed in Cantor's second axiom does not alter the conclusion; consider, for example, the subsemigroup of $L_{(0,\omega_1)}$ (with the addition either defined naturally or in the manner described below) consisting of all numbers $\alpha + x$ where α is a non-zero ordinal $< \omega_1$ and x is either 0 or $\frac{1}{2}$; again, $\left(1\frac{1}{2} - 1\right) = \frac{1}{2}$ is not a member of the semigroup.

The second aspect of Cantor's letter that is puzzling is more crucial, but here the puzzle only emerges when the letter is read in conjunction with his letter written to Eneström one week earlier. Namely, how can Cantor embrace his unrestricted addition for linear numbers while maintaining "the linear magnitudes are thoroughly completed with the known real numbers"? After all, $1 + 1 + 1 + \ldots$ (taken ω times) *is not a real number*! Of course, he could have avoided this difficulty in any of a number of ways; however, as we shall soon see, while Cantor continued to embrace his unrestricted addition for linear numbers (not to mention a substantial strengthening thereof), he subsequently sidestepped the above problem by tacitly denying that such sums of linear numbers are necessarily linear. Indeed, in his discussion containing his purportedly more general proof of the impossibility of infinitesimals he essentially remarks in passing that every linear number is bounded above by a real number. Before turning to the latter discussion, however, we will further explore some of the properties of $L_{(0,\omega_1)}$ and of Cantor's trans-finite line L_c more generally, for it is our suspicion that just as Cantor's thinking about $L_{(0,\omega_1)}$ helped shape his "proof" of the impossibility of numerical magnitudes of the kind $\frac{1}{\omega}$, it was his thinking about L_c that helped shape his "proof" of the impossibility of infinitesimals, more generally.

To begin with, as we alluded to earlier, $L_{(0,\omega_1)}$ and L_c are ordered semigroups closed under (one-sided) subtraction of smaller from larger elements, if the addition is defined in the manner we are confident Cantor intended. The latter is a straightforward extension of Cantor's addition for ordinals and may be defined for all $\alpha \oplus r$, $\beta \oplus s \in L_c$ as follows, where, again, $+_c$ is the familiar Cantorian sum of ordinals, and $r + s$ is the ordinary addition of real numbers:

$$(\alpha \oplus r) +_{L_c} (\beta \oplus s) = (\beta \oplus s), \text{ if } \alpha +_c \beta = \beta \, ;$$

$$(\alpha \oplus r) +_{L_c} (\beta \oplus s) = (\alpha +_c \beta) \oplus (r + s), \text{ if } \alpha +_c \beta > \beta \, .$$

The basic properties of subtraction in L_c and $L_{(0,\omega_1)}$ with addition thus defined par-allel the Cantorian ones for ordinals. In his *Grundlagen* Cantor described the latter as follows:
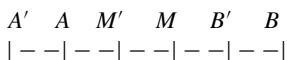
Subtraction can be considered from two points of view. If α and β are any two integers [i.e. finite or transfinite ordinals], $\alpha < \beta$, one easily persuades oneself that the equation $\alpha + \xi = \beta$ admits one and only one solution for ξ . . . . This number is to be set equal to $\beta - \alpha$.

If on the other hand one considers the following equation: $\xi + \alpha = \beta$ it turns out that this can often not be solved for ξ at all; this case, for example, occurs in the following equation: $\xi + \omega = \omega + 1$.

But also in those cases where the equation $\xi + \alpha = \beta$ is solvable for ξ it often happens that it is satisfied by infinitely many numerical values of ξ; but of these different solutions one will always be least. [Cantor 1883 in Cantor 1932, pp. 201–202; Cantor 1883 in Ewald 1996, p. 913; (Translation Ewald)][60]

The basis of our confidence that Cantor envisioned addition defined in $L_{(0,\omega_1)}$ (and in L_c, more generally) in the manner specified above is twofold. To begin with, throughout his career Cantor maintained that his definitions of sums and products of ordinals were grounded in the very concept of set and were not subject to modification; accordingly, the addition in $L_{(0,\omega_1)}$ when restricted to pairs of ordinals presumably would have to concur with Cantor's classical definition. What is more important, however, is that the above addition appears to be naturally suggested by a discussion of an initial segment of $L_{(0,\omega_1)}$ contained in Cantor's own *Mitteilungen zur Lehre vom Transfiniten* [1887]. The remarks in question originated in a letter from Cantor to Constantin Gutberlet dated January 24, 1886, that Cantor wrote in response to the theologian's attempt to prove the impossibility of an actual infinite line segment. According to Cantor, Gutberlet argued that a contradiction could be derived using the proposition: "If an infinite line existed, one could excise a finite stretch ... and then draw together and reconnect the two remaining stretches" [1887 in 1932, p. 397]. Precisely how Gutberlet's argument runs Cantor does not say; but he suggests it is defective insofar as it relies on the common misconception of attributing to the infinite properties that are characteristic of the finite. In particular, writes Cantor:

> If you displace a finite line AB in the direction such that its starting point A is moved about the stretch $AA' = 1$
>
> $$A' \quad A \quad M' \quad M \quad B' \quad B$$
> $$| - -| - -| - -| - -| - -|$$
>
> to A', it is also necessary for *each of its other points* [to be similarly shifted], e.g., M is shifted to M' about an equal stretch $MM' = 1$ and, *in particular, the end point B is shifted to B' about the stretch $BB' = 1$.*
>
> Of course, however, if instead of the finite line AB we imagine an actual infinite line AO in the same direction with the same starting point which has its endpoint O in the infinite, it is also the case that if A is moved to A' then each point M lying in the finite is moved about $MM' = 1$ to M', *but who informed you that this also applies to the infinitely distant endpoint O?*

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[60] Since $1 < \omega$ and $1 + \omega = \omega$, it follows from the convention specified in first of the three just-quoted paragraphs that $\omega - 1$ is an ordinal, and for similar reasons, so is $\omega^2 - \omega$. Throughout his career Cantor continued to set ξ equal to $\beta - \alpha$ when $\alpha < \beta$ and $\alpha + \xi = \beta$ (cf. [1897, p. 218; 1915, p. 155]). This tradition was continued by a number of twentieth-century set-theorists including Sierpinski [1965, p. 278] and Kuratowski and Mostowski [1968, p. 248], the latter two of whom emphasized that in accordance with the stated convention $\omega - 1 = \omega$ and $\omega^2 - \omega = \omega^2$. Other authors, however, including Fraenkel [1976, pp. 209–210] and Kamke [1950, p. 94], because of the non-commutative nature of the addition, preferred to set ξ equal to $-\alpha + \beta$. Given the latter convention, $-1 + \omega = \omega$, and $\omega - 1$ is undefined. Ordinals of the form $\tau - 1$ arise in accordance with the latter approach if and only if τ is an immediate successor of an ordinal.

Completely to the contrary, the acceptance of the latter leads to a contradiction, as you have yourself shown; this contradiction, however, does not authorize the denial of the possibility of an actual infinite line AO, as you assume; nothing contradictory follows from the characteristic of the actually infinite line AO that the infinitely distant point O alone remains fixed in its place while all other points M, A, B of the line AO are shifted to the left in equal stretches $MM' = AA' = BB' = 1 \ldots$.

Here the magnitude of the imagined actual infinite line AO corresponds to the *smallest* transfinite ordinal denoted by me with ω, so the just stated assertion involves the equation $1 + \omega = \omega$, which again is known not to contain the least contradiction, where $1 = A'A$ is on the left side meaning the *Augendus* [augend] and $\omega = AO$ is the *Addendus* [addend]. By comparison, though, is $\omega + 1 \ldots$ a transfinite number different from ω, specifically the whole transfinite following the smallest ordinal ω; however, the latter has no application in your example, where for you the *Augendus* is a finite magnitude $1 = A'A$ lying in the finite and the *Addendus* $AO = \omega$ is an actual infinite.[61]

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[61] [Cantor 1887 in 1932, pp. 397-398]: "In dieser Argumentation erkenne ich den Fehler, daß die Eigenschaften einer endlichen starren Linie ohne weiteres auf eine unendliche starre Linie übertragen werden, deren Eigenschaften von der Natur des Unendlichen abhängen.

Wenn Sie eine endliche Gerade AB in ihrer Richtung so verrücken, daß ihr Anfangspunkt A um das Stück $AA' = 1$

A'		A		M'		M		B'		B
| − − − − −	| − − − − −	| − − − − −	| − − − − −	| − − − − −	| − − − − −	|

nach A' geschoben wird, so ist dies nur so möglich, daß *jeder andre ihrer Punkte,* z. B. M nach M' um ein gleiches Stück $MM' = 1$ und *im besonderen auch der Endpunkt B* um das Stück $BB' = 1$ nach B' verrückt wird.

Denken wir uns aber statt der endlichen Linie AB in derselben Richtung und mit demselben Anfangspunkte eine aktual-unendliche Linie AO, die ihren Zielpunkt O im Unendlichen hat, so gilt zwar auch, daß jeder im Endlichen liegende Punkt M um $MM' = 1$ nach M' gerückt wird, falls A nach A' kommt, *wer sagt Ihnen aber, daß hier auch das gleiche gilt vom unendlich fernen Zielpunkt O?*

Ganz im Gegenteil führt letztere Annahme, wie Sie selbst gezeigt haben, zu einem Widerspruch; dieser Widerspruch berechtigt aber nicht, wie Sie annehmen, zur Leugnung der Möglichkeit der Existenz einer aktual-unendlichen Geraden AO, sondern er führt zu der nichts Widersprechendes involvierenden Eigenschaft der aktual-unendlichen Geraden AO, daß, während *alle anderen* Punkte M,A,B *der* Geraden AO um ein gleiches Stück $MM' = AA' = BB' = 1$ nach links gezogen werden ....

Da die gedachte aktual-unendliche Gerade AO ihrer Größe nach der von mir mit ω bezeichneten *kleinsten* transfiniten Ordnungszahl entspricht, so läßt sich das soeben Behauptete auch in der bekannten, nicht den geringsten Widerspruch involvierenden Gleichung $1 + \omega = \omega$ wiederfinden, wo auf der linken Seite $1 = A'A$ die Bedeutung des *Augendus,* $\omega = AO$ die des *Addendus* hat. Dagegen ist allerdings $\omega + 1$, wo ω als *Augendus*, l als *Addendus* figurieren, wie aus den Prinzipien meiner "Grundlagen" geschlossen wird, eine von ω *verschiedene* transfinite Zahl, nämlich die auf die kleinste ω *nächstfolgende ganze transfinite* Ordnungszahl; letzteres hat aber auf Ihr Exempel keine Anwendung, da bei Ihnen der *Augendus* eine endliche und im Endlichen liegende Größe $A'A = 1$, der *Addendus* $AO = \omega$ eine aktual-unendliche ist."

The above remarks, of course, do not inform us how Cantor thought the sum of two segments having lengths $\alpha \oplus r$, $\beta \oplus s \in L_c$ would be defined in the general case. On the other hand, given that he is considering lengths of continuous line segments for which $1 + \oplus \omega = \omega$ and $\omega \oplus 1 > \omega$, it seems only natural for him to have further supposed that $\frac{1}{2} + \oplus \omega = \omega$ and $\omega \oplus \frac{1}{2} > \omega$. It is these and related intuitions that suggest the definition of addition in L_c specified above. We now turn to the matter of transfinite sums in L_c with ordinary finitary addition thus understood, and to Cantor's conception of transfinite sums more generally.

The definition of an ordinary infinite sum of positive magnitudes that is implicit in Cantor's second axiom for linear magnitudes is a special case of a more general definition of transfinite sums of positive magnitudes which by 1887 was an unpublished principle of Cantor's thinking, and later came to play an important role in his most mature investigations of ordinals [1897, pp. 218–220; 1915, pp. 156–158]. In a letter to Mittag-Leffler dated February 10, 1883, Cantor formulated the definition as follows:

> The definition of the sum of a series of positive numbers, which is given as a well-ordered set is provided by the *least* hyperfinite number which is greater than or equal to the sum of arbitrarily many numbers of the set taken in their *given succession*; that such a *minimum* exists is easily seen.[62]

Also in the letter, Cantor offered the following illustrations that are intended to show that the resulting sums depend on the sequence of the summands:

$$1 + 2 + 3 + ...v... = \omega$$

$$2 + 3 + ... + v + ... + 1 = \omega + 1$$

$$1 + 3 + 5 + ... + (2v + 1) + ... + 2 + 4 + 6 + ...(2v) + ... = 2\omega.^{63,64}$$

Moreover, in a letter to Mittag-Leffler dated March 3, 1883 Cantor went on to note that in accordance with his definition

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[62] [Cantor February 10, 1883]: "Die Definition der Summe einer Reihe aus positiven Zahlen, welche als wohlgeordnete Menge gegeben ist wird nämlich geliefert durch die *kleinste* überendliche Zahl, welche grösser oder gleich ist der Summe von beliebig vielen in der *gegebenen Succession* genommenen und summierten Zahlen der Menge; dass ein solches *Minimum* immer vorhanden ist sieht man leicht."

[63] The expression "2ω" is not a typographical error. In 1883, Cantor had not yet adopted the now familiar notation according to which the ordinal in question would be written "$\omega \cdot 2$".

[64] The same year that Cantor's *Grundlagen* appeared in print Mittag-Leffler published a French translation of an edited portion of it in his *Acta Mathematica* [Cantor 1883a]. During its preparation for publication Mittag-Leffler wrote to Cantor to seek clarification on a number of points regarding Cantor's theory. The first two of the three questions he raised read as follows:

> 1. Among your new numbers are there any that might fit between the rational and irrational numbers? For example, between zero and one are there still other numbers besides the rational and irrational numbers that are greater than zero and smaller than one? Can you give me an example?

$$1 + \frac{1}{2} + \frac{1}{3} + ... = \omega.$$

He also took the opportunity to restate and amplify his definition thus: "According to me," says Cantor,

> the definition of the sum of positive magnitudes of an absolutely infinite number series which is given in the form of a *well-ordered* set a_α, where α runs through all numbers which are smaller than, *or* smaller than and equal to a bound [ordinal] , is as follows:
> $\sum\limits_{\alpha=1}^{A} a_\alpha$ is equal to the *upper* bound of all the numbers $\sum\limits_{\alpha=1}^{A'} a_\alpha$ where $A' < A$ (respectively $A' \leq A$).
>
> For the case where $A = \omega$ and the series $\sum a_\alpha$ converges, the sum which follows from *this* definition agrees entirely with that which is obtained by the known definitions, as you will certainly see. [65]

> 2. Let $\sum \alpha_v$ be a divergent series with only positive terms [that are real numbers]. Are such series given any meaning by your investigations? For example, can you say of two such series $\sum \alpha_v$ and $\sum \beta_v$ which is the larger?
>
> "On your 1st question," [responded Cantor] "allow me to answer you later; today I am not yet certain I would be able to express myself beyond what I said in § 4 of my work."

In response to the second question, however, Cantor wrote the remarks quoted above in the main body of the text.

The original texts of Mittag-Leffler's questions and Cantor's just quoted response read as follows:

> "1. Giebt es unter Ihren neuen Zahlen solche die zwischen die rationalen und irrationalen Zahlen eingepasst werden können? Giebt es zum Beispiel zwischen Null und Eins ausser den rationalen und irrationalen Zahlen die grösser als Null und kleiner als Eins sind noch andere Zahlen? Können Sie mir ein Beispiel geben?
> 2. Ich nehme an dass $\sum \alpha_v$ eine Reihe mit nur positiven Gliedern sei, welche divergiert. Bekommt eine solche Reihe irgend eine Bedeutung bei Ihren Untersuchungen? Kann man zum Beispiel von zwei solchen Reihen $\sum \alpha_v$ und $\sum \beta_v$ aussprechen, welche die grössere ist?" [Mittag-Leffler February 7, 1883, p. 115]
> [Cantor February 10, 1883, p. 115]: "Auf Ihre 1te Frage erlauben Sie mir Ihnen später zu antworten; heute würde ich mich noch nicht bestimmter darüber ausdrücken können als in §4 meiner Arbeit."

[65] [Cantor March 3, 1883, p. 117]: "Die Definition der Summe positiver Größen, die in der Form einer *wohlgeordneten* Menge a_α gegeben sind, wo α alle Zahlen der absolut unendlichen Zahlenreihe zu durchlaufen hat, die kleiner, *oder* kleiner und gleich sind einer Grenze A, ist bei mir folgende:

$\sum\limits_{\alpha=1}^{A} a_\alpha$ ist gleich der *oberen* Grenze aller Zahlen: $\sum\limits_{\alpha=1}^{A'} a_\alpha$ wo $A' < A$ resp. $A' \leq A$.

Für den Fall, dass $A = \omega$ und dass die Reihe $\sum a_\alpha$ convergirt stimmt die nach *dieser* Definition sich ergebende Summe mit derjenigen durchaus überein, welche man nach den *bekannten* Definitionen erhält, wie Sie gewiss sehen werden."

As far as we know, the only such transfinite sums of *numbers* that Cantor treated in his published work are transfinite sums of ordinals (and cardinals) and selected ordinary infinite sums of real numbers. Cantor's definition, however, is plainly applicable to transfinite sums of members of L_c as well. Not only are such ordinary infinite sums of members of $L_{(0,\omega_1)}$ well-defined in $L_{(0,\omega_1)}$ when finitary addition is defined in the manner specified above, but the more general transfinite sums of members of L_c are well-defined in L_c as well. This is of some significance since just as the existence of such well-defined ordinary infinite sums of positive magnitudes in $L_{(0,\omega_1)}$ makes it possible to define $\zeta \cdot \omega$ in $L_{(0,\omega_1)}$ for each $\zeta \in L_{(0,\omega_1)}$, the existence of the more general transfinite sums in L_c makes it possible to define $\zeta \cdot \eta$ in L_c for each $\zeta \in L_c$ and each ordinal η. Indeed, if $\zeta \in L_c$ and $\zeta \cdot \eta$ is defined (according to Cantor's definition) as the transfinite sum $\sum_{\alpha=1}^{\eta} \zeta_\alpha$ where $\zeta_\alpha = \zeta$ for all α, then $\zeta \cdot \eta$ is well-defined for all

ordinals η and is equal to the least upper bound in L_c of all the partial sums $\sum_{\alpha=1}^{\beta} \zeta_\alpha$ where $0 < \beta < \eta$ if η is an infinite limit ordinal and $0 < \beta \le \eta$ if η is a successor ordinal; furthermore, if ζ is itself an ordinal, then $\zeta \cdot \eta = \zeta \cdot_c \eta$ where $\cdot_c$ is the familiar Cantorian product of ordinals.

It is our suspicion that Cantor was fully cognizant of the above, and it is this suspicion that motivated our earlier observation that much as Cantor's thinking about $L_{(0,\omega_1)}$ helped shape his "proof" of the impossibility of numerical magnitudes of the kind $\frac{1}{\omega}$, it may very well have been his thinking about L_c that helped shape his "proof" of the impossibility of infinitesimals, more generally. Indeed, for as we shall now see, just as Cantor's unpublished "proof" relies on the assumption that if $\frac{1}{\omega}$ is a linear magnitude then $\frac{1}{\omega} \cdot \omega$ is well-defined, his published "proof-sketch" employs the tacit assumption that if ζ is a linear magnitude then $\zeta \cdot \eta$ is well-defined for all ordinals η.

8. Cantor's "proof" of the impossibility of infinitesimals

As we have already mentioned, a sketch of Cantor's purported general proof of the impossibility of infinitesimals was first presented in letters to F. Goldscheider [Cantor May 13, 1887] and K. Weierstrass [Cantor May 16, 1887], and soon thereafter appeared in [Cantor 1887]. The discussion in the latter, which essentially follows the text of the letter to Goldscheider, reads as follows.

> You mention in your letter the question of actual *infinitely small* magnitudes. At several places of my works you will find expressed the opinion that this is *impossible*, i.e., *they are self-contradictory* in thought, and I already implied in my work "Foundations of a General Theory of Manifolds", p. 8, §4, even though still with a certain reserve, that a rigorous proof of this position could be derived from the theory of transfinite numbers. During this winter, the time was first found to express my ideas on this subject in the form of a formal proof. It concerns the theorem:
> *Non-zero linear numbers ζ (i.e., numbers which may be regarded as bounded, continuous lengths of straight lines) which would be smaller than each arbitrarily small finite number do not exist, i.e., they contradict the concept of linear numbers.*

The thought process of my proof is simply as follows: I proceed from the *assumption* of a linear magnitude ζ which is so small that its *n*-fold product

$$\zeta \cdot \eta$$

is less than unity *for each whole number*, and prove from the concept of linear magnitude with the help of certain propositions of *transfinite* number theory, that even when ν is an arbitrarily large transfinite ordinal (i.e., the order type of a well-ordered set)

$$\zeta \cdot \upsilon$$

is smaller than any finite magnitude that is as small as you please. This means that ζ *cannot be made finite through any actual infinite multiplication*, and is therefore certainly not an *element* of finite magnitude. Thus, the assumption we made contradicts the concept of a linear magnitude, which is of the sort that, according to it each linear magnitude must be thought of as an integral part of another, in particular of finite linear magnitude. So nothing is left but to let go of the assumption that there is a magnitude ζ which for any finite whole number *n* would be smaller than $\frac{1}{n}$, and with this our proposition has been proven.

It seems to me that this is an important application of the theory of transfinite numbers, which is capable of pushing aside widespread prejudices.

The fact of actual infinite numbers is thus so little ground for the existence of actual infinitely small magnitudes that, on the contrary, the impossibility of the latter can be proven with the former.

I also don't believe that this result can be reached fully and strictly in any other way.

The *need* of our theorem is especially clear for the purpose of opposing the newer attempts of O. Stolz and P. Dubois-Reymond to derive the legitimacy of actual infinitely small magnitudes from the so-called "Archimedean axiom" (cf. O. Stolz, "Zur Geometrie der Alten, insbesondere über ein Axiom des Archimedes" 1881-82, 1883; "Die unendlich kleinen Grössen" 1884; "Vorlesungen über allgemeine Arithmetik", Part 1, Leipzig 1885, p. 205).

Archimedes appears to be the first to remark that, the assertion used in Euclid's Elements, where upon from any arbitrarily small line segment can be produced through sufficiently large multiplication an arbitrarily large line segment, requires proof, and for that reason he believed that this assertion should be called an "Assumption."

(Cf. Euclid's *Elements,* Book V, Definition 4: Magnitudes are said to have a ratio to one another which are capable, when multiplied, of exceeding one another; also, especially Elements, Book X, Proposition 1, Archimedes' The Sphere and Cylinder I, Postulate 5 and the Introduction to his work: The Quadrature of the Parabola).

Now it is the reasoning of those authors (O. Stolz loc. cit.), that if one deletes this supposed "axiom," the permissibility of actual infinitely small magnitudes, which are there called "moments," would emerge. But if the above theorem of mine is applied to the continuous straight line, *the necessity of the Euclidean assumption* immediately follows. Therefore the so-called "Archimedean Axiom" *is not an axiom at all, but a theorem that follows with logical necessity from the concept of linear magnitude.*[66]

In the years following the publication of these remarks questions and controversy about them rapidly emerged. What was clear of course was that Cantor believed he had

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[66] For the original German text, see Appendix IV.

provided a proof-sketch that infinitesimals contradict the concept of a linear magnitude and thereby a proof-sketch that infinitesimals "*are self-contradictory* in thought" and hence that "*the existence of actual infinitely small magnitudes...* [constitutes an] *...impossibility*." It was also clear that Cantor believed that by applying his "theorem" on the nonexistence infinitesimals to the continuous straight line he was able to prove the version of the Archimedean axiom found in Euclid's *Elements* and thereby obviate its need as an independent axiom.[67] Also apparent to at least some of Cantor's readers was that Cantor believed that his "proof" applied not solely to geometrical magnitudes but stood in direct opposition to "the newer attempts of O. Stolz and P. Dubois-Reymond to derive the legitimacy of actual-infinitely small magnitudes from the so-called 'Archimedean axiom'." On the other hand, what was not so evident to Cantor's readers was the precise content and import of Cantor's "proof-sketch" or how Cantor thought the argument should be fleshed out. This of course is not terribly surprising since Cantor's purported proof-sketch provides the reader with neither the definitions of the purported proof's central concepts nor a list of the propositions of transfinite number theory upon which it is supposedly based. Nor, unfortunately, did Cantor ever attempt to fill these gaps, at least not in his subsequent publications nor in any subsequent letters of which we are aware. Nor, of course, did Cantor's readers have general access to his earlier unpublished letters where he did attempt to shed at least some light on these issues.

Perhaps the chief source of puzzlement, as we have already noted, concerned Cantor's use of the term "linear number." What precisely did he mean by such numbers? Since, according to Cantor, "Non-zero linear numbers ... [are] ... *numbers which may be regarded as bounded, continuous lengths of straight lines*," it was probably safe for his readers to assume at least:

> (**L0***) The system $\langle L, +, < \rangle$ of non-zero linear numbers is a positively ordered semigroup, having neither a least element nor any idempotent elements,[68] that contains the strictly positive cone $\langle \mathbb{R}^+, +_{\mathbb{R}^+}, <_{\mathbb{R}^+} \rangle$ of real numbers as a substructure.

Also plausible given Cantor's contention and perhaps more natural from the standpoint of late nineteenth-century mathematics (see Note 69) was the somewhat stronger assumption:

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[67] Apparently, Cantor also believed that by proving the Archimedean axiom he had fulfilled a need that had long ago been recognized by Archimedes. However, precisely how Archimedes viewed the so-called Axiom of Archimedes has long been the source of controversy, and Cantor's interpretation is only one of a number of possible interpretations. For insightful and provocative discussions of Archimedes' own views regarding the Archimedean axiom, see [Hjelmslev 1950; Dijksterhuis 1987, pp. 148–150; Knorr 1987, pp. 431–433; and Knorr 1978, pp. 205–213 along with the references cited therein]. Knorr's [1978] contains arguments that lend support to Cantor's view.

[68] An element μ is *idempotent* if $\mu + \mu = \mu$. For further discussion of such elements including an example of a positively ordered semigroup containing a non-zero idempotent infinitesimal, see Appendix II.

(L0) The system $\langle L, +, < \rangle$ of non-zero linear numbers is a *strictly* positively ordered semi-group, without a least element, that contains the strictly positive cone $\langle \mathbb{R}^+, +_{\mathbb{R}^+}, <_{\mathbb{R}^+} \rangle$ of real numbers as a substructure.[69]

Still another plausible assumption for Cantor's readers to have made, and an assertion Cantor almost certainly did assume, is right-solvability, i.e.,

(L1*) For all linear numbers ζ and ζ' where $\zeta < \zeta'$ there is a linear number γ such that $\zeta + \gamma = \zeta'$.[70]

Moreover, since Cantor remarked that "each [non-zero] linear magnitude must be thought of as an integral part of another, in particular of finite linear magnitude" presumably it was also safe for his readers to assume:

(L1) For each linear number ζ, there is a finite linear number r such that $r \geq \zeta$.

Before turning to an analysis of Cantor's "proof-sketch" *per se*, it will be instructive to briefly ponder the potential import for linear numbers and for Cantor's hopes therefore that accrue from the adoption of particular subsets of these four assumptions beginning with L0* and L0.

At first sight, one might think it would have been safe for Cantor to assume for the sake and purpose of his proof that the system of non-zero linear numbers satisfies strict positivity ($a + b > a, b$ for all $a, b \in L$) rather than mere positivity ($a + b \geq a, b$ for all $a, b \in L$) and the absence of idempotent elements ($a + a \neq a$ for all $a \in L$). However, while Cantor does not appear to have ever explicitly commented on any of these conditions, let alone L0* and L0, we believe there are at least two reasons that might have pushed him in the direction of positivity and the absence of idempotent elements, if pressed. First, as we have already observed, in an earlier section of the paper containing the above "proof-sketch," Cantor discussed a transfinite straight line the measures of whose line segments constitute an initial segment of his "continuous set of numbers" L_c; moreover, whereas these measures satisfy positivity and the absence of idempotent

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[69] The basis of our contention that from the standpoint of late nineteenth-century mathematics L0 would have been a more natural assumption than L0* is simply that idempotent elements do not appear to have played any role in discussions of ordered semigroups until well into the twentieth century. Whereas idempotent elements of groups (without total orders) had been discussed by Frobenius in 1895 and by E. H. Moore in 1905, idempotent elements of semigroups, more generally, appear to have been first investigated by Ward, Suschkewitsch, Poole, Rees and Climescu in the 1930s and 1940s [cf. Clifford and Preston 1961, p. 20]. Moreover, discussions of non-zero idempotent elements of (additively written) ordered semigroups appear to have emerged even later with A. H. Clifford's influential investigations of positively ordered (as opposed to merely strictly positively ordered) semigroups [cf. Clifford 1954b; 1958].
Unlike in (additively written) ordered semigroups, non-zero idempotent elements cannot occur in (additively written) ordered groups.

[70] Indeed, as we learned from Cantor's letter to Kerry dated March 18, 1887, Cantor assumed that linear magnitudes have the property of "unrestricted ... subtraction (... in the sense that the smaller magnitude should be subtracted from the larger). Thus each smaller magnitude is considered part of the larger ...."

elements they do not satisfy strict positivity (since, for example, $1 + \omega = \omega$). Thus, for Cantor, writing in 1887, it is not evident that the phrase "*numbers which may be regarded as bounded, continuous lengths of straight lines*" carries with it the implication of strict positivity. Second, as we believe Cantor himself makes clear, among the principal targets of his "proof" are Stolz's systems of moments, systems that likewise satisfy positivity and the absence of idempotent elements but not strict positivity. Accordingly, if Cantor's "proof" was to have any hope of undermining Stolz's moments, he could not have assumed strict positivity and, hence, L0 from the outset. This of course is not intended to suggest that Cantor would not have deemed the linear numbers to be strictly positive; since he held that the non-zero linear numbers could be shown to coincide with the strictly positive real numbers he undoubtedly would, but given his purposes at hand this would be something he would have to prove.[71] On the other hand, as best as we can tell, neither Cantor nor his critics ever expressed any recognition of this, and we suspect that at least for his critics it was L0 rather than L0* that they tacitly assumed.[72]

Despite having informed Eneström only one month earlier that he was finally able to prove with the aid of transfinite numbers that there are no non-zero linear numbers except for the strictly positive real numbers Cantor made no overt attempt to incorporate such a proof along with the above "proof-sketch." Whether he believed that L0 (or, even, L0*) together with L1 and a proof of the nonexistence of non-zero infinitesimal linear numbers was sufficient for that purpose we are in no position to say–though if he did believe it, he would have been mistaken as the nontrivial Archimedean extensions of $\langle \mathbb{R}^+, +_{\mathbb{R}^+}, <_{\mathbb{R}^+} \rangle$ that emerged in the work of Bettazzi [1890] would soon make clear.[73] On the other hand,

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[71] Everything just said about strict positivity applies to the cancellation laws of addition (see Notes 7 and 33), neither of which is satisfied by Stolz's systems of moments or by Cantor's transfinite straight line. It is perhaps also worth noting that while Stolz's systems of moments are solvable, i.e., they are both right-solvable and left-solvable (see Note 7), Cantor's transfinite straight line is merely right-solvable. The failure of left-solvability is evident from the fact that while $\omega > 1$ there is no ordinal γ such that $\gamma + 1 = \omega$. Thus, for Cantor writing in 1887, it is by no means evident that the phrase "*numbers which may be regarded as bounded, continuous lengths of straight lines*" carries with it either the implications of strict positivity, cancellativity or left-solvabiltiy.

See Parenthetical Observation I of Appendix III for further observations related to these matters.

[72] In fact, one of Cantor's more sympathetic critics interpreted Cantor's contention that "each linear magnitude must be thought of as an integral part of another" as asserting that "the juxtaposition of two segments allows the formation of another segment, composed of the two though different from each"[Vivanti 1891b, p. 254], an assertion whose analog for linear numbers is given by:

(**L0****) For all non-zero linear numbers ζ and ζ', $\zeta' + \zeta \neq \zeta', \zeta$.

But, as is easy to see, the conjunction of L0* and L0** is equivalent to L0.

[73] In modern parlance, Bettazzi isolated Archimedean, strictly positively ordered semigroups, without least elements, that contain the strictly positive cone $\langle \mathbb{R}^+, +_{\mathbb{R}^+}, <_{\mathbb{R}^+} \rangle$ of real numbers as a substructure as well as *anomalous pairs*. Following Alimov [1950], two elements a and b of a positively ordered semigroup are said form an anomalous pair if for all integers $n > 0$, $na < nb < (n + 1)a$, or $na > nb > (n + 1)a$. Intuitively speaking, in an Archimedean positively ordered semigroup two elements form an anomalous pair if they differ by an infinitesimal amount. Since

of course, he might have believed quite correctly that L0 (or, even, L0*) together with L1, L1* and a proof of the nonexistence of non-zero infinitesimal linear numbers was sufficient to show that $\langle L, +, < \rangle$ coincides with $\langle \mathbb{R}^+, +_{\mathbb{R}^+}, <_{\mathbb{R}^+} \rangle$–though even if he did, it is doubtful he would have been in a position to prove it.[74] In connection with the above one also cannot help wondering what Cantor took to be the justification for L1? Could it be that L1, which in effect rules out infinitely large linear numbers, was introduced by Cantor as a quick fix to the problem of unlimited addition of linear magnitudes we alluded to in our discussion of Cantor's letter to Kerry of March 18, 1887, or did Cantor believe he had some more theoretical justification for maintaining that linear magnitudes are parts of finite magnitudes? Again, we just don't know. In any case, since L0* together with L1 do preclude the possibility of linear numbers that are infinitely large relative to the real numbers, to establish the Archimedean nature of $\langle L, +, < \rangle$ it only remained for Cantor to show that there are no non-zero infinitesimal linear numbers.

As several of Cantor's critics observed, in his attempt to establish the impossibility of non-zero infinitesimal linear numbers, Cantor implicitly relied upon the following two assumptions that transcend the familiar late nineteenth-century assumptions about magnitudes, the second of which he apparently took to be a manifestation of his contention that "each [non-zero] linear magnitude must be thought of as an integral part of another, in particular of finite linear magnitude," presumably, it being understood that the ordinals constitute "the extended sequence of integers" [Cantor 1883 in Ewald 1996, p. 887].

> **(L2)** If ζ is a linear number, then $\zeta \cdot \eta$ is well-defined for all ordinals η.[75]
> **(L3)** If ζ and r are non-zero linear numbers for which $\zeta < r$ and r is finite, then $\zeta \cdot \eta \geq r$ for some ordinal η (where $\zeta \cdot \eta$ is itself a finite linear number).[76]

In particular, relying on L2 he believed he could prove his "theorem" by establishing the incompatibility of the existence of an infinitesimal linear number and L3, and hence

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a positively ordered semigroup without an identity is isomorphic to an ordered subsemigroup of $\langle \mathbb{R}^+, +_{\mathbb{R}^+}, <_{\mathbb{R}^+} \rangle$ if and only if it is Archimedean and contains no anomalous pairs [Fuchs 1961; 1963, p. 169: Theorem 5], it readily follows that: if $\langle L, +, < \rangle$ satisfies L0* together with L1 and the Archimedean Axiom, then $\langle L, +, < \rangle$ coincides with $\langle \mathbb{R}^+, +_{\mathbb{R}^+}, <_{\mathbb{R}^+} \rangle$ if and only if L contains no anomalous pairs.

See Parenthetical Observation II of Appendix III for an example of a continuous (i.e., dense and Dedekind complete) structure that satisfies L0 together with the Archimedean axiom but does not coincide with $\langle \mathbb{R}^+, +_{\mathbb{R}^+}, <_{\mathbb{R}^+} \rangle$.

[74] For a proof-sketch that L0*, L1, L1* and the absence of non-zero infinitesimal members of L collectively suffice to show that $\langle L, +, < \rangle$ coincides with $\langle \mathbb{R}^+, +_{\mathbb{R}^+}, <_{\mathbb{R}^+} \rangle$, see Parenthetical Observation III of Appendix III.

[75] Strictly speaking, Cantor's "proof-sketch" only makes use of the following weaker version of L2: If ζ is an infinitesimal linear number, then $\zeta \cdot \eta$ is well defined for all ordinals η. On the other hand, as we shall soon see, in his letter to Veronese dated September 7, 1890, Cantor expresses his support for L2.

[76] While Cantor's critics usually identify L3 as the assumption tacitly employed by Cantor in conjunction with L2, there is nothing in Cantor's remarks that would point to L3 rather than one of the following two assertions as the assumption Cantor actually had in mind:

the incompatibility of the existence of an infinitesimal linear number and the concept of linear number, more generally, by proving what we shall call

> **Cantor's Lemma**. *Let r and ζ be non-zero linear numbers where r is finite. If ζ is an infinitesimal, then $\zeta \cdot \eta < r$ for all ordinals η.*

Unlike Cantor's unpublished "proof" which indicates how $\frac{1}{\omega} \cdot \omega$ is to be defined, the published "proof-sketch" contains no clear statement how $\zeta \cdot \eta$ is to be interpreted when ζ is an infinitesimal and η is a transfinite ordinal. Despite this, most of the early commentator's on Cantor's purported proof including Stolz [1888, p. 603], Vivanti [1891b, pp. 253–254], Bettazzi [1891, p. 178; 1892, pp. 39–40], Frege [1892 in 1984, p. 180], Veronese [1891, pp. 105–106] and Zermelo [Cantor 1932, p. 439] tacitly supposed that Cantor intended these products to be defined in a manner which is entirely concordant with Cantor's unpublished definition of transfinite sum contained in his letters to Mittag-Leffler discussed above, that is, by treating ζ as a unit element and defining $\zeta \cdot \eta$ in much the same manner as Cantor defines $1 \cdot \eta$. For example, when referring to the multiplication in question, Frege observed that: "The word 'multiplied' would have to be understood here in Cantor's sense" [1892 in 1984, p. 180; 1892 in 1967, p. 165]. We suspect that these authors believed that if Cantor had intended the product to be defined in some other way he would have felt obliged to say as much. Moreover, at least one of his commentators, Veronese [1891, p. 105: Note 1], took Cantor's previously discussed treatment of actual infinite line segments of lengths ω and $\omega + 1$ to provide implicit support for this particular interpretation of how Cantor understood $\zeta \cdot \eta$ is to be defined when ζ is a bounded segment of a straight line and η is a transfinite ordinal. In addition, with "$\zeta \cdot \eta$" thus understood Cantor's "integral part" interpretation of L3 not only takes on a natural geometrical interpretation in terms of segment addition but one that is in complete harmony with Cantor's just-mentioned discussion of line segments having transfinite lengths. In any case, as we suggested above, given the contents of the aforementioned letters to Kerry and the temporal proximity of these letters to those of Eneström, Goldscheider and Weierstrass, this construal of Cantor's intention appears to have the ring of truth. This being the case, henceforth we will assume that Cantor did indeed construe L2 in this manner and we will write **L2$_c$** to designate L2 thus understood.

As Cantor's remarks in his above-cited letters to Mittag-Leffler make clear, Cantor would have been well-aware that L2$_c$ carries with it the presupposition that the system of numbers in which the products $\zeta \cdot \eta$ are defined–presumably L_c supplemented, for the sake of the proof, with supposed infinitesimals and any requisite elements that differ from

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(L3*) If ζ and r are linear numbers for which $\zeta < r$ and r is finite, then $\zeta \cdot \eta > r$ for some ordinal η (where $\zeta \cdot \eta$ is itself a finite linear number).

(L3**) If ζ is a linear number, then $\zeta \cdot \eta = r$ for some finite linear number r and some ordinal η.

Each of the three assertions could serve the same end in Cantor's argument and each may be naturally interpreted as expressing a manifestation of his contention that "each [non-zero] linear magnitude must be thought of as an integral part of another, in particular of finite linear magnitude." In fact, as is demonstrated in Parenthetical Observation IV of Appendix III, if L2 is interpreted in the manner we believe Cantor intended–see L2$_c$ below–the three assertions are equivalent.

one another by infinitesimal amounts–satisfies some sort of least upper bound property; in particular, $\zeta \cdot \eta$ would be the least upper bound in the system of the set of products $\zeta \cdot \alpha$ ($\alpha < \eta$) whenever η is an infinite limit ordinal. Moreover, as a consequence of this, one would have the following generalization of the second part of Cantor's second axiom of linear magnitudes from his letter to Kerry:

(C1) If $s = \zeta \cdot \eta$ where ζ is a linear number and η is an infinite limit ordinal, and $0 < s' < s$, then there is an ordinal $\alpha < \eta$ such that $\zeta \cdot \alpha > s'$.

Assuming Cantor to have indeed embraced L2$_c$ and, hence, C1, along with L0*, one could also imagine him proving his Lemma (and thereby his Theorem) by *reductio ad absurdum* as follows. Suppose the Lemma is false. Then ζ is a non-zero infinitesimal linear number, r is a non-zero finite linear number (i.e., a member of L which, in virtue of L0*, lies between two elements of $\mathbb{R}^+$) and there is an ordinal η such that $\zeta \cdot \eta \geq r$. But since every subclass of ordinals has a least member, it follows that:

(Ω) There is a least non-zero ordinal, say, ν, such that $\zeta \cdot \nu \geq s$ for some non-zero finite linear number s (i.e., $\zeta \cdot \nu \geq s$ for some non-zero finite linear number s and if $\zeta \cdot \nu' \leq t'$ for some ordinal ν' and some non-zero finite linear number t', then $0 < \nu \leq \nu'$).

Now, either μ is a limit ordinal or μ is a successor ordinal. If ν is a limit ordinal and s' is any of the infinitely many non-zero finite linear numbers smaller than s, then by C1 $\zeta \cdot \mu \geq s\prime$ for some $\mu < \nu$, which contradicts Ω. Moreover, if ν is a successor ordinal, then $\zeta \cdot (\nu - 1) \geq s''$ for some non-zero finite linear number s'', which likewise violates Ω; thereby proving the Lemma.

This argument is in fact valid; thus, assuming it or an argument very much like it to have been the argument Cantor had in mind, Cantor might very well have been on safe logical ground. On the other hand, of course, it does not prove the impossibility of infinitesimals. At best, it proves the impossibility of an ordered semigroup extension of the reals that contains infinitesimals and satisfies L0*, L2$_c$ and L3.[77] While stated perhaps

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[77] In his biography of Cantor, Joseph Dauben made a related though somewhat misleading observation. According to Dauben:

Numbers were *linear* if a finite or infinite number of them could be added together to produce yet another linear magnitude .... Having assumed that all numbers *must* be linear, this was equivalent to the Archimedean property, and thus it was no wonder that Cantor could "prove" the axiom. The infinitesimals were excluded by his original assumptions, and his proof of their impossibility was consequently flawed by its own circularity. [Dauben 1979, pp. 130–131]

To begin with, the characterization of linear numbers specified by Dauben neither implies nor is implied by the Archimedean axiom. Consider, for example, L_c with finitary addition defined in the manner we specified in the above discussion of Cantor's letter to Kerry dated March 18, 1887, and transfinite addition defined *à la* Cantor. With finitary and transfinite addition thus defined, the sum of any finite or transfinite number of members of L_c is itself a member of L_c, but L_c is not Archimedean–since, for example, $n1 < \omega$ for all positive integers n. Conversely, while $\langle \mathbb{R}^+, +_{\mathbb{R}^+}, <_{\mathbb{R}^+} \rangle$ is Archimedean, $1 + 1 + 1 + ...$ (taken ω times) is not a member of $\mathbb{R}^+$. Moreover, even if Dauben's mathematical contention were correct, I do not see why it would be appropriate to describe

with less precision, observations of this sort are not entirely new. The first person to draw attention to such a limitation appears to have been Otto Stolz who directed his attention to Cantor's reliance on L2 and, apparently, L2$_c$ in particular. Other authors such as Bettazzi called into question L3. Before moving beyond Cantor's "proof-sketch" proper, however, we will first say a few more words about the contention contained therein that linear numbers are "*numbers which may be regarded as bounded, continuous* [stetiger] *lengths of straight lines*," a contention which, at first sight, appears to play no important role in Cantor's purported proof other than perhaps that of motivating such underlying ordered semigroup assumptions as L0*, L0 and L1*.

As the reader might suspect, Cantor's contention helped contribute to the controversy and confusion that soon befell and continues to befall his purported proof of the impossibility of infinitesimals. In particular, some authors such as Frege [1892 in 1984, p. 180] and Meschkowski [1967, pp. 120–121] have apparently taken the just-quoted remark to indicate that Cantor's attempt to prove the impossibility of infinitesimals was an attempt to show that the now standard geometrical linear continuum is devoid of infinitesimals. However, while admitting that it would not be entirely unnatural to construe Cantor's remark in this fashion, especially when considered in isolation, we find this interpretation to be implausible for several reasons that go beyond the simple observation that both in his private letters and published works Cantor includes the works and remarks of du Bois-Reymond and Stolz on infinitesimals among the targets of his "proof." To begin with, already in 1872 Cantor had published his seminal paper in which he introduced his familiar construction of the real numbers [1872 in 1932, pp. 92–96], pointed out that each point of a Euclidean line could be shown "by means of purely logical argumentation" to be coordinated by a pair of such "number-magnitudes" [1872 in 1932, p. 96], and introduced his own formulation of the so-called *Cantor-Dedekind axiom* which postulates that "to every [real] number there corresponds a definite point of the [Euclidean] line, whose coordinate is equal to that number" [1872 in 1932, p. 97]. Thus, by 1872, for Cantor, as for Dedekind, establishing that the geometrical linear continuum is devoid of infinitesimals was reducible to showing that the system of real numbers is devoid of infinitesimals–*something Cantor never perceived the need to prove*! Indeed, in his *Grundlagen*, when Cantor first proposed the idea of proving the impossibility of infinitesimals using his theory of transfinite ordinals he characterized "infinitely small numbers" (and numbers that differ from a real number by an infinitesimal amount, more generally) as

> numbers which do not coincide with the rational and irrational numbers ... but which might be inserted into supposed gaps amidst the real numbers, just as the irrational numbers are inserted into the chain of the rational numbers, or the transcendental numbers into the structure of the algebraic numbers. [Cantor 1883 in Cantor 1932, p. 171; Cantor 1883 in Ewald 1996, p. 887; (Translation Ewald)]

Cantor's argument as circular since Cantor's conception of a linear number is not a mere restatement of either the Archimedean axiom or the absence of infinitesimals. On the other hand, as we have already mentioned, Cantor's purported proof would not be successful since it would merely preclude the existence of infinitesimal linear numbers.

Moreover, as Veronese [1891, p. 105: Note 1] later pointed out, if Cantor was merely intending to establish that there are no infinitesimal segments in the ordinary continuum, there was no need for him appeal to "propositions of *transfinite* number theory" to achieve that end. In fact, we suspect Cantor was well aware that Stolz, in his paper on the Axiom of Archimedes [1883, p. 509; Note*], had already provided an elementary argument showing that any absolute (or relative) system of magnitudes in Stolz's sense is Archimedean if it is a linear continuum in the sense of Cantor (see Note 25). In addition, there seems to be no way to square this interpretation with the thrust of the earlier "proof" contained in his letter to Kerry or with his already quoted remarks contained in his letter to Gustav Eneström to the effect that:

> I was always of the view that the linear magnitudes are thoroughly completed with the familiar real numbers, and therefore that there are besides these no other linear numbers and in particular no *fixed infinitely small numbers.* Now I can prove this with the aid of transfinite numbers. [Cantor March 6, 1887]

However, if Cantor was not referring specifically to the Cantor-Dedekind geometrical linear continuum, what did he have in mind and what possible role did it play in his purported proof? While we are not aware of any published or unpublished writing of Cantor's that directly addresses these questions we suspect that an exchange of letters between Cantor and Dedekind written in May and June of 1877 may help to shed important light on them. The subject of the letters is, appropriately enough, "the essence of continuity."

In his *Stetigkeit und irrationale Zahlen* (Continuity and Irrational Numbers) Dedekind contends that "property IV"–what is now called the *Dedekind cut axiom*– expresses "the essence of continuity" [1872 in Ewald 1996, pp. 767, 771]. While embracing the substance of Dedekind's implicit definition of a continuous ordered set, Cantor, in a card dated May 10, 1877 and again in a letter dated May 17, 1877, nevertheless demurs that:

> the stress which at various points in your paper you *expressly* lay on property IV as being the *essence* of continuity must lead to misunderstanding ... [since] ... this property also holds of the system of all integers, which can, however, be regarded as a prototype of discontinuity. [Cantor May 17, 1877 in Ewald 1996, pp. 851–852; (Translation Ewald); also see Noether and Cavaillès 1937, p. 22]

Perceiving that their "opinions diverge at most about expediencies, not about necessities," Dedekind retorts:

> You concede ... that my definition in fact overlooks nothing; e.g. when I say 'Domains which possess properties I [the ordering relation is transitive] and II [the ordering relation is dense] are called continuous if they also possess property IV .... But it seems that you would prefer ... 'Domains, whose elements ... [are] ... of the type defined by I are called continuous if they also possess properties II *and* IV'.... [I]f the revised definition pleases somebody better, I have nothing to say against its legitimacy–that is, not if it should be of advantage for certain investigations. But my original formulation pleases me much better, and I think it is more expedient in treating the essence of continuity to lay the emphasis solely on IV and to discuss property II earlier, before continuity or discontinuity is at issue."[Dedekind May 18, 1877 in Ewald 1996, p. 852; (Translation Ewald); also see Noether and Cavaillès 1937, pp. 22–23]

Conceding the apparent force of these remarks, Cantor thanked Dedekind for his letter and added that:

> I completely agree with its contents; and I acknowledge that the difference in our points of view was merely external [Cantor June 20, 1877 in Ewald 1996, p. 853; (Translation Ewald); also see Noether and Cavaillès 1937, p. 25].

As is evident from the above exchange, in 1877 Cantor, like Dedekind, was of the opinion that a totally ordered set is continuous if and only if it is dense and Dedekind complete.[78] Indeed, there we find unpublished anticipations of the now standard view that grew out of the early twentieth-century work of Huntington [1905–1906, p. 15; 1917/1955, p. 44].[79] Accordingly, if we assume that Cantor's opinion did not change in this regard, this would explain why he claimed in §4 of his *Grundlagen* that L_c "is a continuous set of numbers" [Cantor 1883 in Ewald 1996, p. 887] despite the fact that L_c is not connected and, therefore, not a linear continuum in the classic sense he introduced in §10 [Cantor 1883 in Ewald 1996, p. 903] of the very same paper. Moreover, if we likewise assume that this is the sense of "continuous" Cantor had in mind when he contended that linear numbers are "*numbers which may be regarded as bounded, continuous lengths of straight lines*," this would also shed light on the basis of his explicit and implicit use of least upper bound principles in his discussions of linear magnitudes since a densely ordered set satisfies the Dedekind cut axiom if and only if every bounded subset has a least upper bound. Indeed, while admitting that Cantor may well have had a different sense of "continuous" in mind, we are not aware of any other sense that brings harmony and clarity to his disparate and sometimes cryptic remarks regarding linear magnitudes or that squares as neatly with his views on related matters.[80]

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[78] In modern parlance, the Dedekind completeness property for an ordered set P containing more than one element may be stated thus: If P is the union of two nonempty subsets P_1 and P_2 where every member of P_1 precedes every member of P_2, then either P_1 contains a greatest member or P_2 contains a least member. In Dedekind's original formulation it is not clear whether Dedekind intended the sentence connective "or" to be understood *inclusively* or *exclusively*. Some authors, including Russell [1903, pp. 279–280], interpreted it exclusively and others, including Veblen [1905, p. 165] and Huntington [1917, p. 44], treated it inclusively, although it is not clear they thought this is what Dedekind had in mind (cf. [Huntington 1904–1905, p. 164; 1917, 1955, p. 19]). On the other hand, since in his exchange with Dedekind Cantor claimed that "this property also holds of the system of all integers," presumably he did understand it in the inclusive sense. Of course, in the case of a densely ordered set, which is the case Dedekind was concerned with, the assertions that result from adopting the two different senses of "or" are equivalent.

[79] Although Huntington took his axioms for "continuous series" from Veblen [1905], Veblen called such series "continuous" if and only if they are isomorphic to the standard linear continuum. Moreover, while Stolz's [1883] also essentially contained the general definition, unlike Huntington [1905–1906, pp. 16, 24; 1917/1955, pp. 45, 56], Stolz did not provide any example of an open continuously ordered set that is not isomorphic to the standard linear continuum.

[80] Recently, M. Moore [2002, p. 306] published an interesting paper containing an alternative translation (attributed to W. D. Hart) of Cantor's purported proof of the impossibility of infinitesimals. Unfortunately, the translation omits the passages that refer to Cantor's earlier work and that directs the thrust of Cantor's attack to the works of Stolz and du Bois-Reymond. From a histor-

9. Stolz's response to Cantor

Although doubts about the precise content and import of Cantor's purported proof soon emerged, there was never a doubt in Stolz's mind that his works were among its principal targets. Stolz accordingly published a formal response the following year in *Mathematische Annalen* in a paper entitled *Ueber zwei Arten von unendlich kleinen und von unendlich grossen Grössen* (Two Kinds of Infinitely Small and Infinitely Large Magnitudes). Stolz begins his defense by asserting that:

> Mr. G. Cantor has shown, that if a magnitude ζ is assumed to be smaller than each positive real number and each of the multiples $\zeta \cdot n$, where n is a natural number, as well as each of the products $\zeta \cdot \nu$, where ν is an arbitrary transfinite ordinal number, are defined, then *each of the latter products must also be smaller than an arbitrarily small positive real number*. Accordingly, it is not possible to assume infinitely small elements of the linear numbers.
>
> This proposition, however, by no means stands in contradiction with the two kinds of theories of infinitely small magnitudes I have already set up.[81]

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ical standpoint, these are serious omissions because it is these passages that help place Cantor's "proof" into historical context and draw attention to its intended targets. Apparently unaware of the history, Moore suggests that "Cantor's argument is not just about arithmetic and set theory, but also and crucially about analytic geometry, about the interplay between geometry and arithmetic, and the ways in which that interplay is constrained by the properties of space." Moreover, Moore fleshes these ideas out in the course of his analysis using such ideas as Archimedean and non-Archimedean ordered field as well as Cartesian space over Archimedean and non-Archimedean, Pythagorean ordered fields. The fact that these path-breaking conceptions were first introduced by Veronese, Levi-Civita and Hilbert in the decade following the publication of Cantor's "proof" did not deter Moore from offering this interpretation. We believe, therefore, Moore was wise when he cautioned readers that: "I am no Cantor scholar. I am interested in the argument mainly as a possible source of insight into philosophical problems arising from such recent theories of infinitesimals as Abraham Robinson's nonstandard analysis. I am thus not so much concerned with exactly what Cantor meant to say as I am with what we can say, along Cantor's lines, about the infinitely small. I will be accordingly be unapologetically anachronistic in giving my reconstruction ...." On the other hand, as our analysis would suggest, we are not inclined to embrace Moore's further contention that "my [i.e., his] reconstruction is a plausible first guess as to Cantor's general drift" [2002, pp. 305–306].

[81] [Stolz 1888, p. 601]: "Herr G. Cantor hat gezeigt, dass wenn man von einer Grösse ζ annimmt, sie sei kleiner als jede absolute reelle Zahl und es seien sowohl die Vielfachen derselben $\zeta \cdot n$, wo n jede natürliche Zahl, als auch die Producte $\zeta \cdot \nu$, wo ν jede transfinite Ordnungszahl sein darf, erklärt, dann auch *jedes der letzteren Producte kleiner als jede noch so kleine absolute reelle Zahl sein muss*. Demnach geht es nicht an, unendlich kleine Elemente der linearen Zahlgrössen anzunehmen.

Mit diesem Satze steht die Theorie der [b]isher aufgestellten zwei Arten von unendlich kleinen Grössen keineswegs in Widerspruch."

Indeed, with regard to his systems of moments of functions he notes:

> The n-fold multiple of $\mathfrak{u}(f)$ is $\mathfrak{u}(nf)$. Generally, $nf(x)$ has no limit as the natural number n grows without bound, so the product $\mathfrak{u}(f) \cdot \omega$ is undefined, wherein ω signifies the first number of the second number class.[82, 83]

And with regard to his earlier system based on the ideas of du Bois-Reymond he adds:

> Since by the multiple $\mathfrak{u}(f) \cdot n$ the magnitude $\mathfrak{u}(f^n)$ is understood, it is already obvious that in this case the product $\mathfrak{u}(f) \cdot \omega$ also can not be defined.[84]

Thus, Stolz does not appear to raise questions about the cogency of Cantor's purported proof, but rather about the applicability of the supposed theorem. Indeed, according to Stolz, even if Cantor's "Theorem" is in fact a theorem, it does not preclude the existence of the two systems of infinitesimals he has discussed. After all, argues Stolz, Cantor's "Theorem" is only applicable to systems of linear magnitudes in Cantor's sense, i.e., systems of magnitudes whose properties include a well-defined product by transfinite ordinals, and the systems he has considered are not systems of linear magnitudes in this sense!

However, while Stolz stands firm in maintaining the logical cogency of his systems of infinitesimals, he does go on to add that:

> I am far from attaching any value other than a formal one to the just-described systems of magnitudes.[85]

In fact, says Stolz:

> Since for the moments of functions there is no product $\mathfrak{u}(f) \cdot \nu$ [for transfinite ν], one cannot naturally regard them, like the elements of the absolute real numbers, as representing line segments.[86]

Thus, Stolz seems to be suggesting that if Cantor's argument is merely intended to rule out the possibility of non-Archimedean number systems that can be employed to represent line segments, then given the absence of the aforementioned products by transfinite ordinals, Cantor need not view the existence of Stolz's systems as posing a challenge to his view. Unfortunately, why he believes that such a product must be well-defined for

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[82] [Stolz 1888, p. 603]: "Das n-fache von $\mathfrak{u}(f)$ ist $\mathfrak{u}(nf)$. Da $nf(x)$ bei unendlichem Wachsen der natürlichen Zahl n im Allgemeinen keinen endlichen Grenzwerth hat, so lässt sich nicht einmal das Product $\mathfrak{u}(f) \cdot \omega$ definiren, worin ω die erste Zahl der zweiten Zahlenclasse bedeutet."

[83] It is Stolz's contention that the product $\mathfrak{u}(f) \cdot \omega$ is undefined because $nf(x)$ has no limit as the natural number n grows without bound that suggests to us that Stolz construed L2 as L2$_c$.

[84] [Stolz 1888, p. 603]: "Unter den Vielfachen $\mathfrak{u}(f) \cdot n$ wird nunmehr die Grösse $\mathfrak{u}(f^n)$ verstanden, woraus ersichtlich ist, dass auch in diesem Falle schon des Product $\mathfrak{u}(f) \cdot \omega$ nicht erklärt werden kann."

[85] [Stolz 1888, p. 603]: "Ich bin weit davon entfernt, dem soeben geschilderten Grössensysteme einen andern als formalen Werth beizulegen."

[86] [Stolz 1888, p. 603]: "Für die Momente der Functionen giebt es also keine Producte $\mathfrak{u}(f) \cdot \nu$, so dass man natürlich auch sie nicht als Elemente der absoluten reellen Zalhen, bezw. der diese darstellenden Strecken betrachten kann."

numbers that can represent line segments Stolz never says. On the other hand, Stolz's apparent rejection of infinitesimal line segments in 1888 seems to be entirely consistent with the view implicit in his paper of 1883 where *straight line* for Stolz appears to be equated with the continuous straight line of Dedekind. And this view emerges even more clearly in the concluding paragraph of his response to Cantor where we are told:

> For neither of the two described systems of magnitudes is the proposition that I call, for brevity, the Axiom of Archimedes, unconditionally valid. That it is valid for each system of *continuous* magnitudes, and therefore for the straight lines, I have shown taking as a basis the Dedekind definition of continuity.[87]

Although Cantor never published a formal response to Stolz's defense of 1888, apparently he was not impressed with the strength of Stolz's argument. Indeed, two years later when Giuseppe Veronese reiterated Stolz's defense in a letter to Cantor, Cantor responded to Veronese as follows:

> You say "Mr. Stolz has demonstrated that your [i.e., Cantor's] theorem has no bearing on either his infinitely small magnitudes or those of du Bois Reymond, because one can not define their multiplication by the number omega."
>
> Mr. Stolz could not have done greater damage to his theory when he emphasized that his alleged "*infinitely small magnitudes*" could not be multiplied by ω. An absolute linear magnitude that is supposed to be real [i.e., have reality] but which nevertheless cannot be multiplied without limit (and, hence, also with transfinite multipliers) is not a magnitude at all. And even if he calls these (self contradictory and *completely useless*) *abominations* magnitudes for his and du Bois Reymond's private use, *science* will *never* follow. For private purposes, he can just as well concern himself with square circles or hyperbolic ellipses, as with actual infinitely small magnitudes; as long as they do not leave the confines of his study, nobody will oppose these flights of fancy. As far as I'm concerned, such activities are *Signisticismus* (playing with symbols).[88]

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[87] [Stolz 1888, p. 604]: "Für keines der beiden im Vorstehenden betrachteten Grössensysteme gilt derjenige Satz, den ich kurz als das Axiom des Archimedes bezeichne, unbedingt. Dass derselbe für jedes System von *stetigen* Grössen, also auch für die geradlinigen Strecken von selbst erfüllt ist, habe ich unter Zugrundelegung der Dedekind'schen Definition der Stetigkeit dargethan."

[88] [Cantor September 7, 1890]: "Sie sagen "M. Stolz a démontré que votre théorema ne vaut pas pour ses grandeurs un fini ment [presumably, un fini ment = infiniment] petites ni pour celles de duBoisReymond, car on ne peut pas définir leur multiple selon le nombre omega."

Herr Stolz konnte seinen angeblichen "*unendlich kleinen* Größen" *keinen* stärkeren Stoß in's Herz versetzen, als indem er hervorhob, daß sie sich nicht durch ω vervielfachen lassen. Eine absolut lineare Größe, die Realität haben soll und trotzdem sich nicht unbegrenzt, (also auch mit transfiniten Multiplikatoren) vervielfachen läßt, ist *gar keine Größe* und wenn er diese (sich selbst widersprechenden und ganz *ohne Anwendung und Nutzen* dastehenden) *Undinge* für seinen resp. duBois Reymonds Privatgebrauch Größen *nennt*, so wird ihm *die Wissenschaft nie* darin folgen können. Für private Zwecke kann man sich ebensogut mit viereckigen Kreisen oder hyperbolischen Ellipsen, wie mit actual unendlich kleinen Größen beschäftigen; solange man damit aus seiner Studirstube nicht heraustritt, wird Niemand diesen Passionen ein Hinderniss entgegensetzen. Derartige Beschäftigungen nenne ich *Signisticismus* (Zeichenspielerei)."

10.   The emergence of non-Archimedean systems of magnitudes IV: The work of Thomae

Although Stolz first employed du Bois-Reymond's system of orders of infinity to establish the existence of a non-Archimedean system of absolute magnitudes, he could have established the same result employing an alternative type of system, an instance of which had been implicit in the literature since 1870 when components of it were discussed by Johannes Thomae [1870, pp. 40–41; 1872, pp. 124–126; 1873, pp. 9–10; 1880, pp. 113–114]–the man who, according to Georg Cantor, "can claim a questionable fame for having infected mathematics with the cholera bacillus of infinitesimals ..." [Cantor December 13, 1893 in Meschkowski, 1965, p. 505]. Indeed, as we already noted, this is precisely what Stolz did in his *Theoretische Arithmetik* [1902, p. 280], and he seems to have already hinted at this possibility in a pair of footnotes contained in his paper of 1883 [p. 505: Note*; pp. 506–507: Note**].

Although Thomae did not explicitly mention the non-Archimedean natures of the various systems he considered over the years, already in 1870 he asserted that his system of "measures of ... the orders of vanishing of functions" constitutes a "number domain" that is "infinitely more dense than ... the ordinary real numbers" and he exhibited a member of the domain denoted "lg" which he describes as "a number which while not zero is smaller than each specifiable number, i.e., a number in the ordinary number domain" [1870, p. 40: Note*]. And two years later–the year that Cantor's, Dedekind's, Heine's and Weierstrass's theories of real numbers first appeared in print–he developed these ideas further when he wrote:

> ... these measures constitute a one-dimensional, continuous manifold [in the sense of Riemann] for the determination of which all our ordinary rational and irrational numbers do not suffice. Indeed, a rigorous theory of irrational numbers ... soon to be published by Mr. E. Heine ... has need of the following hypothesis: "Any magnitude that is different than zero by less than each number no matter how small is itself zero." However, in the continuity of values of which we speak, and which designates the order in which a function A_n vanishes as n approaches infinity there are such values that are essentially different than zero but smaller than each arbitrarily small number .... I have already spoken of this subject on page 40 of my book *Abriss einer Theorie der complexen Functionen und der θ-functionen einer Veränderlichen* [1870]. There exists also in this range of numbers (if it be permitted to generalize this name to serve for points of the continuity of the values that designate orders of vanishing) numbers greater than all ordinary numbers ....[89]

Following an illustration of the latter point, Thomae goes on to conclude:

> It may be that the numbers we have proposed will never play an important role in applied mathematics; nevertheless it seems to me that it is not superfluous to bring these forms to the attention of mathematicians, because they shed light on the nature of ordinary num-

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[89] [Thomae 1872, p. 125]: "... ces mesures constituent une continuité d'une dimension, pour la détermination de laquelle tous nos nombres communs rationnels et irrationnels ne suffisent pas. En effet, une théorie rigoureuse des nombres irrationnels ... publiée, ce que Mr. E. Heine fera bientôt ... a besoin de l'hypothèse suivante: "Toute grandeur qui est différente de zéro de moins que tout nombre d'une petitesse quelconque, est zéro elle même." Dans la continuité des valeurs

bers. For it clearly follows that it is not justified to claim that a point would immediately follow another in the continuity of [ordinary = real] numbers, which appears intuitively certain from the continuity of points of a line of the space. In addition, it follows that it is a true [i.e., genuine] hypothesis to suppose that a number that is not negative but lesser than all arbitrarily small positive ordinary numbers is itself zero, because one can abandon this hypothesis without renouncing the laws of addition and multiplication, as I have shown in the place quoted. Mathematicians, I believe, cannot remove infinitely small quantities that are different than zero from the sphere of analytic discussion, even though the theory of ordinary numbers and calculus based thereon are essentially based on the assumption that such numbers do not exist. This is not the case except for the commonly used numbers. One is readily disposed to extend this property to all mensurable continuous manifolds, because it links [the ordinary] numbers to all the points of continuity; however, it is this that has not always seemed just to me, since sometimes the [ordinary] numbers do not suffice for all points.[90]

Like du Bois-Reymond's "orders of infinity," Thomae's "measures" emerged from the study of the rate of change of functions. However, as the preceding remarks suggest, in his early work Thomae was primarily concerned with the *orders of vanishing* of functions or, the *orders of infinite smallness* of functions, as Bettazzi [1890], Borel [1899; 1902], Vivanti [1899; 1908] and Enriques [1911b; 1912] preferred to call them. Whereas the orders of infinity of functions emerged primarily from the study of positive, continuous, monotonic, increasing, real-valued functions, the orders of vanishing of functions emerged largely from the intimately related investigation of positive, continuous, monotonic, decreasing, real-valued functions. The order of vanishing of such a

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dont nous parlons, et qui assignent l'ordre dans lequel une fonction A_n s'annule, n croissant à l'infini, il y a de telles valeurs qui sont plus petites que chaque nombre aussi petit qu'on veut, et qui cependant diffèrent essentiellement de zéro .... J'ai déjà parlé de ce sujet dans mon livre "*Abriss einer Theorie der complexen Functionen und der θ-functionen einer Veränderlichen*" page 40. Il existe aussi dans cette étendue des nombres (s'il est permis de généraliser ce nom et s'en servir pour les points de la continuité des valeurs qui marquent les ordres de décroissement) des nombres plus grands que tout nombre commun ...."

[90] [Thomae 1872, pp. 125–126]: "Peut-être que les nombres que nous avons proposés ne joueront jamais un rôle important dans les mathématiques appliquées; néanmoins il me semble qu'il ne soit pas superflu de rendre attentifs les mathématiciens à ces formes, parcequ'elles peuvent mettre dans son jour la nature des nombres communs. Car il en provient clairement, qu'il n'est pas juste de prétendre qu'un point suive immédiatement un autre dans la continuité des nombres, ce qui parait intuitivement sûr dans la continuité des points d'une ligne de l'espace. Il en résulte de plus, qu'il est une vraie hypothèse de supposer qu'un nombre qui n'est pas négatif, mais moindre que tout nombre commun positif, quelque petit qu'il soit, soit zéro luimême, parceque l'on peut abandonner cette hypothèse sans renoncer aux lois de l'addition et de la multiplication, ce que j'ai montré au lieu cité. Les mathématiciens, je crois, ne pourront éloigner de la sphère des discussions analytiques les quantités infiniment petites, différentes néanmoins de zéro, bien que la théorie des nombres communs et les calculs y fondés soient appuyés essentiellement sur cela, que de telles nombres n'existent pas. Mais cela ne vaut que pour l'étendue des nombres usités. On est aisément disposé à étendre cette propriété sur toutes les continuités mensurables, parcequ'on rapporte les nombres aux points de toute continuité; c'est ce que ne me semble pas être toujours juste, puisque les nombres quelque fois ne suffisent pas pour tous les points."

function $f(x)$ is the rate at which $f(x)$ tends to 0 as the variable x approaches a given value a.[91] In 1870, in particular, Thomae was concerned with the orders of vanishing of functions of the form

$$x^a \cdot \frac{1}{(\log x)^\beta} \frac{1}{(\log^2 x)^\gamma} \cdots \frac{1}{(\log^m x)^\mu} \quad \text{(as } x \text{ approaches +0)}$$

where $\alpha, \beta, \nu, \dots, \mu$ are real numbers, m is a positive integer, and $\log^2 x = \log \log x$, $\log^3 x = \log \log \log x$, etc. However, unlike du Bois-Reymond and Stolz, Thomae represented the rates of growth of functions with the members of an infinite-dimensional version of Herman Grassmann's systems of *hypercomplex numbers* or simply, *complex numbers*, as most late nineteenth- and early twentieth-century mathematicians called them.

Grassmann's systems of complex numbers were developed in his monograph on the calculus of extension [1862]. They are divisible Abelian groups consisting of all elements of the form

$$a_1 e_1 + \dots + a_n e_n$$

where n is a fixed integer $> 1, e_1, \dots, e_n$, are distinct *units*, and $a_1, \dots, a_n \in \mathbb{R}$. Addition for these complex numbers is defined by the rule

$$(a_1 e_1 + \dots + a_n e_n) + (b_1 e_1 + \dots + b_n e_n) = (a_1 + b_1)e_1 + \dots + (a_n + b_n)e_n$$

where $(a_1 + b_1), \dots, (a_n + b_n)$ are the respective sums in $\mathbb{R}$. $0e_1 + \dots + 0e_n$ is the zero of the group; and the divisibility of the group is evident since for each element $a_1 e_1 + \dots + a_n e_n$ of the group and for each positive integer m, $(a_1/m)e_1 + \dots + (a_n/m)e_n$ is a member of the group for which

$$m[(a_1/m)e_1 + \dots + (a_n/m)e_n] = a_1 e_1 + \dots + a_n e_n.$$

---

[91] As we have already mentioned (see Note 19), du Bois-Reymond had also considered such functions in his [1870–71]. In 1875 he went further when he observed that

> ... the most complete symmetry exists between functions becoming zero and becoming infinite, in such a manner that everywhere the positive numbers correspond in the most striking manner to becoming infinite, the negative numbers to becoming zero, zero to remaining finite.

> [du Bois-Reymond 1875, p. 363]: "... dass zwischen Null- und Unendlichwerden die vollständigste Symmetrie herrscht, dergestalt, dass überall dem Unendlichwerden der Functionen die positiven Zahlen, dem Nullwerden die negativen Zahlen, dem Endlichbleiben die Null in schlagendster Weise entsprechen."

Indeed, in the succeeding decades it became apparent that building on the just-described symmetry one could construct a system of orders of infinite smallness along the lines taken by Stolz [1883]. For the details of the construction, see [Vivanti 1899, pp. 54–66; 1908, p. 194]. Also see [Bettazzi 1890, § 66; 1893, p. 55] and [Enriques 1911b, pp. 99–100; 1912, pp. 481–482].

In fact, each of these groups may be naturally regarded as a n-dimensional vector space over $\mathbb{R}$, where the units $e_1, \ldots, e_n$ constitute a basis for the space and for each $r \in \mathbb{R}$ and each element $a_1 e_1 + \ldots + a_n e_n$

$$r(a_1 e_1 + \ldots + a_n e_n) = r(a_1 e_1) + \ldots + r(a_n e_n).$$

Thomae essentially extended Grassmann's construction by considering a notational variant of a system of complex numbers of the form $a_1 e_1 + \ldots + a_n e_n + \ldots$ where n ranges over all positive integers, the $a_n s$ are members of $\mathbb{R}$, $e_1 = 1$, and at most a finite number of the coefficients are nonzero.[92,93] More significant, however, was Thomae's realization that such a system can be ordered in accordance with the condition

$$a_1 e_1 + \ldots + a_n e_n + \ldots < b_1 e_1 + \ldots + b_n e_n + \ldots , \text{ if at the first } i$$

$$\text{for which } b_i - a_i \neq 0, a_i < b_i.$$

Indeed, in accordance with this *lexicograghical ordering*–as Hausdorff [1914] later called it –Thomae's and Grassmann's systems of complex numbers are non-Archimedean ordered Abelian groups. This being the case, the set of strictly positive elements of each such system is a non-Archimedean (totally) ordered absolute system of magnitudes in the sense of Stolz.

Thomae constructs his measures and assigns them to the orders of vanishing of the aforementioned functions in two stages: first he assigns to the "orders" of the functions $\frac{1}{\log x}, \frac{1}{\log^2 x}, \frac{1}{\log^3 x}, \ldots$ measures denoted by $\lg, \lg^2, \lg^3$, and so on; and next to each function of the form

$$x^\alpha \cdot \frac{1}{(\log x)^\beta} \cdot \frac{1}{(\log^2 x)^\gamma} \cdots \frac{1}{(\log^m x)^\mu}$$

where $\alpha, \beta, \nu, \ldots, \mu$ are real numbers and m is a positive integer, he assigns the measure

$$\alpha + \beta \lg + \gamma \lg^2 + \cdots + \mu \lg^m .$$

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[92] Three years earlier, Hankel [1867: Section 31] introduced a similar extension of Grassmann's construction.

[93] Strictly speaking, Thomae never discusses addition for his "complex numbers" in his [1870; 1873 and 1880]. On the other hand, as the above quoted passage from his [1872] makes clear, he believed that already in his [1870] he had demonstrated that his numbers can be added in a manner that preserves the classical properties for addition. Perhaps the most natural interpretation of this is that having identified his numbers as "complex numbers," he took it to be understood that they could be added in the then already standard termwise fashion. This would also help to explain why he always specified the rule by which he intended his complex numbers to be multiplied [1870, p. 41; 1873, p. 9 and 1880, p. 113]. Indeed, unlike the rule he specified (see above) that satisfies the classical properties of multiplication and that is, as he emphasized, compatible with a total ordering of his numbers, there were well-known non-commutative multiplications for complex (i.e., hypercomplex) numbers in the literature due to Hamilton and Grassmann that were known not to enjoy these properties.

By now letting the elements $1, \lg, \lg^2, \ldots$ serve as the units, and by defining addition termwise and ordering the "measures" lexicograghically, Thomae obtains a divisible ordered Abelian group (i.e., a totally ordered relative system of magnitudes in the sense of Stolz (see Note 13)).

The reader will notice that in accordance with Thomae's lexicograghical ordering, $0 < n \lg < 1$ for all n, $0 < n \lg^2 < \lg$, for all n, and so on, thereby establishing the non-Archimedean nature of the system. Moreover, since $0 < n \lg < 1$ for all n, it follows that $0 < \lg < 1/n$ for all n, which justifies Thomae's contention that lg is "a number which while not zero is smaller than each number in the ordinary number domain."

Moreover, as Thomae alluded to in his already quoted remark of 1872, the just-described system of measures is closed under multiplication and preserves the familiar multiplicative laws when multiplication for measures is defined by:

$$(\alpha + \beta \lg + \gamma \lg^2 + \cdots)(\alpha' + \beta' \lg + \gamma' \lg^2 \cdots) =$$

$$\alpha\alpha' + (\alpha\beta' + \beta\alpha') \lg + (\alpha\gamma' + \beta\beta' + \alpha'\gamma) \lg^2 + \cdots$$

Indeed, Thomae's system has the full structure of an ordered commutative algebra with identity when multiplication is defined as above. In fact, while Veronese [1891], Levi-Civita [1892–93; 1898], Hilbert [1899] and Hahn [1907] made no reference to Thomae's definition, it was one or another generalization of the basic idea underlying it that was later used in their constructions of non-Archimedean ordered fields.

In the first edition of his *Elementare Theorie der analytischen Functionen einer complexen Veränderlichen* [1880], Thomae constructed a similar ordered algebra by considering the orders of infinity of functions of the form

$$x^a \cdot (\log x)^\beta \cdot (\log^2 x)^\gamma \cdot \ldots \cdot (\log^n x)^\nu,$$

as x approaches $+\infty$, where $\alpha, \beta, \gamma, \ldots, \nu$ are real numbers, n is a positive integer and $\log^2 x$, $\log^3 x$, etc., are defined as above. The measure of the order of the function $x^a \cdot (\log x)^\beta \cdot (\log^2 x)^\gamma \cdot \ldots \cdot (\log^n x)^\nu$ as x approaches $+\infty$ is defined by Thomae as $\alpha + \beta l_1 + \gamma l_2 + \ldots + \nu l_n$ where l_1 denotes the order of $\log x$, l_2 denotes the order of $\log^2 x$, and so on. Thomae informs us that:

> The just formed number forms may be regarded as complex numbers with infinitely many units [i.e., numbers of the form $\alpha + \beta l_1 + \gamma l_2 + \ldots$ where zeros for coefficients have been added as needed], to which backwards units $l_{-1}, l_{-2}, \ldots$ could also be added corresponding to the orders of $e^x, e^{e^x}, \ldots$."[94]

He also makes explicit in his discussion of multiplication what was already implicit in his earlier discussions of 1870 and 1873, namely that $I_\mu \cdot I_\nu = I_{\mu+\nu}$ for any two units I_μ and I_ν (it being understood that $I_0 = 1$). What is genuinely new here, however, is his assertion that the system so constructed can be extended to a system in which each mea-

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[94] [Thomae 1880, p. 113]: "Die eben gebildeten Zahlenformen können als complexe Zahlen mit unendlich vielen Einheiten angesehen werden, wobei auch noch rückwärts Einheiten etwa $l_{-1}, l_{-2}, \ldots$ zugefügt werden könnten, welche den Ordnungen von $e^x, e^{e^x}, \ldots$ entsprechen ...."

sure has a multiplicative inverse. According to Thomae, if $\alpha = 0$, the units I_{-1}, I_{-2}, ...
are required to express the inverse; and on the other hand, says Thomae:

> If α is not 0, then
>
> $$\frac{1}{\alpha + \beta l_1 + \gamma l_2 + \cdots} = \frac{1}{\alpha}\left(1 + \frac{\beta}{\alpha}l_1 + \frac{\gamma}{\alpha}l_2 \cdots\right)^{-1}$$
>
> is to be introduced by the binomial theorem.[95]

Finally, to draw this section of his discussion of "order measure-numbers" to a close,
Thomae goes on to add:

> That these numbers have heretofore not been introduced into analysis as a means of cal-
> culation is because a field for their application to problems of applied mathematics has
> not been found.[96]

It is perhaps worth noting that Thomae would be in algebraic trouble here if (as Fisher
[1981, p. 124] has suggested) his just-cited contention about the existence of multiplica-
tive inverses was intended to suggest that the infinities of the infinitary calculus or some
subset thereof, with sums and product defined *à la* Thomae, form a field. On the other
hand, if, as we suspect, Thomae was merely claiming that his abstract system of measures
could be extended to a field, his remark would have been somewhat prophetic. Indeed,
not only can his system be extended to an ordered field, as the aforementioned works of
Levi-Civita, Hilbert and Hahn implicitly showed, but by having drawn attention to the
role that can be played by the binomial theorem in determining multiplicative inverses in
fields of "formal power series" he would have anticipated to some extent the technique
employed by some of the early twentieth-century geometers and order-algebrists to do
just that (cf. [Vahlen 1905, p. 32; Forder 1927, p. 33; and Gleyzal 1937, p. 583: Theorem
1]). This technique was later given an elegant theoretical basis by B. H. Neumann in
his seminal work on ordered division rings [1949], and it together with the inductive
technique developed by Levi-Civita [1892–93], Hilbert [1899] and Hahn [1907] are the
standard techniques employed in the literature today.

   In connection with the above, it should be noted that even Thomae's definition of
multiplication may be problematic if it intended to represent the product of orders of
infinity of the aforementioned functions in a perspicuous fashion. This was first pointed
out by Salvatore Pincherle in his *Ordini D'infinito Delle Funzioni* [1884]. Pincherle
[1884, p. 746] argued that if a system of arithmetic for the orders of infinity of the afore-
mentioned functions is to be useful it must satisfy the following three principles that are,
however, collectively incompatible with the body of familiar laws of arithmetic:

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[95] [Thomae1880, p.113]: "Ist α nicht 0, so ist

$$\frac{1}{\alpha + \beta l_1 + \gamma l_2 + \cdots} = \frac{1}{\alpha}\left(1 + \frac{\beta}{\alpha}l_1 + \frac{\gamma}{\alpha}l_2 \cdots\right)^{-1}$$

nach dem binomischen Satze auszuführen."

[96] [Thomae1880, p.113]: "Dass diese Zahlen in der Analysis bisher nicht als Rechnungsel-
emente eingeführt sind, hat seinen Grund darin, dass ein Feld für Anwendung derselben auf
Probleme der angewandten Mathematik für sie noch nicht gefunden ist.

(A)   if , $\mathfrak{U}(f) \geq \mathfrak{U}(g)$, then $\mathfrak{U}(f+g) = \mathfrak{U}(f)$;

(B) $\mathfrak{U}(f) + \mathfrak{U}(g) = \mathfrak{U}(fg)$;

(C) $\mathfrak{U}(f) \times \mathfrak{U}(g) = \mathfrak{U}\{f(g)\}$.

Although Pincherle did not identify a specific set of the classical multiplicative laws that are incompatible with (A) – (C), he illustrated the incompatibility of Thomae's definition by considering the function $\log(x \log x)$. Guided, in part, by Hardy's discussion of the matter [1910, p. 26; 1924, p. 26], Pincherle's argument-sketch [1884, pp. 746–747] may be fleshed out as follows. Following Thomae, let $\mathfrak{U}(x) = 1$ and $\mathfrak{U}(\log x) = l_1$. Then, by (C), $\mathfrak{U}(\log \log x) = l_1^2$, and hence, by (B), (C) and the fact that $l_1^2 = l_2$, we have

$$\mathfrak{U}(\log(x \log x)) = l_1(1 + l_1) = l_1 + l_1^2 = l_1 + l_2.$$

On the other hand, by (A) and the fact that $\log(x \log x) = \log x + \log \log x$, we have

$$\mathfrak{U}(\log(x \log x)) = \mathfrak{U}(\log x + \log \log x) = l_1$$

which is impossible since $l_1 \neq l_1 + l_2$. Without reference to Thomae's work, Borel [1902, pp. 38–40; 1910, pp. 21–22] later explained the source of incompatibility more clearly by showing that the multiplication specified in (C), while associative and right-distributive over the (commutative and associative) addition specified in (B), is neither commutative nor left-distributive over the addition.[97,98]

11. The emergence of non-Archimedean systems of magnitudes V: The work of Bettazzi

While it was Thomae who first realized that the orders of infinity associated with the members of a particular class of functions can be represented by members of a lexicographically ordered system of complex numbers, the full significance of such structures for non-Archimedean systems of magnitudes only became apparent in 1907 with the appearance of Hans Hahn's great pioneering work *Über die nichtarchimedischen Grössensysteme* [On non-Archimedean Systems of Magnitudes]. Indeed, it was in this work that Hahn showed that every ordered Abelian group may be embedded in an appropriate lexicographically ordered system of complex numbers. However, as

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[97] To say that the multiplication is merely right-distributive over the addition is to say that while $(\mathfrak{U}(f) + \mathfrak{U}(g))\mathfrak{U}(h) = \mathfrak{U}(f)\mathfrak{U}(h) + \mathfrak{U}(f)\mathfrak{U}(g)$, it is not in general the case that $\mathfrak{U}(h)(\mathfrak{U}(f) + \mathfrak{U}(g)) = \mathfrak{U}(h)\mathfrak{U}(f) + \mathfrak{U}(h)\mathfrak{U}(g)$.

[98] It is perhaps worth noting that whereas the ordered additive structures of systems of orders of infinitude and infinitesimalitude find a natural interpretation in contemporary work on *Hardy fields* (cf. [Rosenlicht 1983, p. 303]), interest in the multiplicative structures of such systems appears to have gone by the wayside.

Hahn himself acknowledged [1907, p. 602; also see Ehrlich 1996], his own embedding theorem may be regarded as a generalization of a more modest embedding theorem established by Rodolfo Bettazzi in his monograph *Teoria Delle Grandezze* [Theory of Magnitudes] [1890; 1893]. It is to this and related contributions of Bettazzi that we now turn.

Bettazzi's monograph–which was awarded a prize in 1888 by the *Accademia dei Lincei*–provided what was by far the most penetrating investigation of ordered algebraic systems written until that time. Indeed, as Vivanti commented in his article-length review of the work for Darboux's *Bulletin des Sciences Mathématiques* [1891]:

> I am pleased to note that here is a work that is distinguished from the thus far analogous others by the generality of its fundamental point of view, by the rigor of its reasoning, the extent of its development, and the novelty and importance of some of its results.[99]
>
> ... the beautiful dissertation of M. Bettazzi ... deserves to be known and studied by all who are interested in the fundamental questions of the science of numbers.[100]

Bettazzi's monograph was in fact both read and referred to by some of the foremost order-algebrists of his time including Veronese [1891, pp. XXVI, 623–624], Stolz [1891, p. 108], Hölder [1901, p. 4] and, ultimately, Hahn. Like Stolz before him, Bettazzi drew inspiration for his investigation of systems of magnitudes from the remarks of Grassmann. However, besides providing a deeper analysis than Stolz, Bettazzi concerned himself with a wider range of types of systems of magnitudes or *classes of magnitudes*, as he called them. Principal among these is what Bettazzi calls *classes of magnitudes (of one dimension) with respect to S*, where $S(A, B)$ denotes the sum of two elements A and B in a given class [1890, § 31; 1893, p. 15]. These classes either consist solely of positive elements, or contain both positive and negative elements as well as a zero element, and are dubbed *one-directional* and *two-directional*, respectively [1890, § 28; 1893, p. 23]. Bettazzi's one-directional and two-directional classes of magnitudes (of one dimension) with respect to S are natural generalizations of Stolz's systems of absolute magnitudes and systems of relative magnitudes, respectively, insofar as the former satisfy all the conditions of the latter systems except that they need not be divisible (or even dense, for that matter), and they need not be right-solvable (see Note 7). Bettazzi calls a class *proper* or *improper* depending upon whether or not it satisfies the latter condition [1890, § 31; 1893, pp. 24–25], and he says a class is *limited* or *unlimited* [1890, § 30; 1893, p. 24] depending upon whether or not it contains a least positive element. In contemporary parlance limited proper classes are said to be *discrete*; a proper class in Bettazzi's sense is limited if and only if it is not dense.

Following Stolz, Bettazzi distinguished between classes of magnitudes that satisfy the Archimedean axiom and those that do not; he refers to them as *classes of the 1st*

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[99] [Vivanti 1891, p. 53]: "Nous sommes heureux de signaler ici un Ouvrage qui se distingue de tant d'autres analogues par la généralité du point de vue fondamental, par la rigueur des raisonnements, par l'ampleur des développements et par la nouveauté et l'importance de quelques-uns des résultats."

[100] [Vivanti 1891, p. 68]: "... du beau Mémoire de M. Bettazzi ... qui mérite d'être connu et étudié par tous ceux qui s'intéressent aux questions concernant les fondements de la science des nombres."

kind and *classes of the 2nd kind*, respectively [1890, § 54; 1893, p. 44]. Unlike Stolz, however, Bettazzi observed (albeit without proof) that classes of the 2nd kind may be (uniquely) decomposed into a totally ordered system of mutually exclusive and collectively exhaustive classes of the 1st kind, the latter of which he calls the *principal subclasses* of the class [1890, § 61; 1893, p. 51]. In modern parlance, these subclasses, which were introduced independently by Veronese [1891], are called *Archimedean classes*.[101] Bettazzi also offered a rudimentary taxonomy of the various kinds of systems of principal subclasses classes of the 2nd kind could have which included the recognition that there are classes having finitely many principal subclasses as well as those having infinitely many such subclasses [1890, § 61; 1893, pp. 50–51]. He further observed that a principal subclass of a proper class is not itself a proper class unless it is the first principal subclass of a class. However, the remarkable insight that for each Cantorian order type τ there is a system of magnitudes having a system of principal subclasses of order type τ only emerged with the work of Hahn [1907; also see Ehrlich 1995].

As illustrations of classes having infinitely many principal subclasses Bettazzi offers three purported examples, only two of which proved to be non-problematic. Of the latter, one is Thomae's system of 1870 [1890, § 66, § 150; 1893, pp. 55, 156–157], and the second is a system with $+$ and $<$ defined as in [Stolz, 1883] whose universe consists of orders of infinite smallness of the functions

$$x^n, z_1^{n_1} = (a^{-\frac{1}{x}})^{n_1}, z_2^{n_2} = (a^{-\frac{1}{z_1}})^{n_2}, z_3^{n_3} = (a^{-\frac{1}{z_2}})^{n_3}, \ldots z_p^{n_p} = (a^{-\frac{1}{z_{p-1}}})^{n_p}, \ldots$$

(where $z_1 = a^{-\frac{1}{x}}$, $z_2 = a^{-\frac{1}{z_1}}$, ... etc., with $a > 1$) and those of the products $x^m z_1^{m_1} z_2^{m_2} \ldots z_{p-1}^{m_{p-1}} z_p^{m_p}$ where successively $p = 1, 2, 3, \ldots, n, n_1, n_2, n_3, \ldots$, take on all the positive integer values, and $m, m_1, m_2, m_3, \ldots$ take on all the positive and negative integer values.[102]

In these two cases, as Bettazzi remarks, the ordered set of principal subclasses may be indexed over the positive integers, the members of succeeding principal subclasses being increasingly smaller. Indeed, says Bettazzi, in Thomae's system we find

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[101] The term "Archimedean class" appears to be due to Neumann [1949]. It was Hahn [1907], however, who first proved the decomposition theorem for Archimedean classes referred to above (as an elementary part of his proof of his celebrated embedding theorem for ordered Abelian groups). It is noteworthy that the various proofs of the decomposition theorem found in the literature make use of the Axiom of Choice (or some equivalent thereof), an axiom Bettazzi rejected [Bettazzi 1892a; 1896; also see Moore 1982]. For proofs of *Hahn's Embedding Theorem*, the reader may consult [Clifford 1954a], [Fuchs 1963, pp. 56–60] and the wealth of other related references referred to in [Ehrlich 1995].

[102] [Bettazzi 1890, § 66; 1893, p. 55]: "ordini di infinitesimo delle funzioni

$$x^n, z_1^{n_1} = (a^{-\frac{1}{x}})^{n_1}, z_2^{n_2} = (a^{-\frac{1}{z_1}})^{n_2}, z_3^{n_3} = (a^{-\frac{1}{z_2}})^{n_3}, \ldots z_p^{n_p} = (a^{-\frac{1}{z_{p-1}}})^{n_p}, \ldots$$

(essendo $z_1 = a^{-\frac{1}{x}}$, $z_2 = a^{-\frac{1}{z_1}}$, ... ecc., con $a > 1$) e da quelli dei prodotti $x^m z_1^{m_1} z_2^{m_2} \ldots z_{p-1}^{m_{p-1}} z_p^{m_p}$, facendo successivamente $p = 1, 2, 3, \ldots$, dove $n, n_1, n_2, n_3, \ldots$ prendono tutti i valori interi e positivi, ed $m, m_1, m_2, m_3, \ldots$ tutti i valori interi, positivi e negativi."

... in class I ... the orders of x^n; in II those of $(\frac{1}{\log x})^{n_1}$; in III those of $(\frac{1}{\log^2 x})^{n_2}$, etc.[103]

Moreover, in the second system, says Bettazzi,

> [t]he principal subclasses are those of the orders of infinite smallness of x^n, of $x^m (a^{-\frac{1}{x}})^{n_1} = x^m z_1^{n_1}$, of $x^m z_1^{n_1} z_2^{n_2}$, etc."[104]

Bettazzi's third alleged example is du Bois-Reymond's supposed system consisting of

> the orders of infinite smallness of *all possible* functions ....[105]

However, as we have already noted, this conception, which served as a source of confusion and controversy, was later generally recognized not to be a one-dimensional, proper class of magnitudes in Bettazzi's sense.

However, while Bettazzi devoted a fair amount of attention to classes of magnitudes having infinitely many principal subclasses, his aforementioned embedding theorem for non-Archimedean classes is solely concerned with systems having a finite number of such subclasses. Moreover, as Bettazzi emphasized, his embedding theorem for non-Archimedean classes is a straightforward generalization of his corresponding theorem for Archimedean classes; namely

> *Bettazzi's First Embedding Theorem*: If A is an Archimedean, one-dimensional, proper class (of magnitudes) and a is a positive member of A, then there is an embedding of A into the one-dimensional, proper class of real numbers that sends a to 1.

Bettazzi, of course, does not employ the twentieth-century term "embedding," but speaks instead of the "*metrical correspondence*" (corrispondenza metrica) [1890, § 92; 1893, p. 81] that exists between A and a one-dimensional, proper class of real numbers. The real number that is associated with a given member of A in accordance with the metrical correspondence that sends a to 1 is said to be the "*measure* of the magnitude with respect to the chosen unit" [1890, § 102; 1893, p. 89].

Bettazzi formulates his embedding theorem for non-Archimedean structures in terms of the notational variants of Grassmann's aforementioned vector spaces over the reals that arise by expressing the members in the form

$$\alpha^{(0)} 1_0 + \alpha^{(1)} 1_1 + \ldots + \alpha^{(n)} 1_n$$

where $\alpha^{(0)}, \alpha^{(1)}, \ldots, \alpha^{(n)}$ are real numbers and $1_0, 1_1, \ldots, 1_n$ are the units associated with the various Archimedean classes. More specifically, if for each positive integer n we let $B(n)$ be the set of all "numbers" of the form $\alpha^{(0)} 1_0 + \alpha^{(1)} 1_1 + \cdots + \alpha^{(n)} 1_n$

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[103] [Bettazzi 1890, § 150; 1893, pp. 156–157]: "... nella classe I ... gli ordini di x^n...; nella II quelli di $(\frac{1}{\log x})^{n_1}$; nella III quelli di $(\frac{1}{\log^2 x})^{n_2}$, ecc."

[104] [Bettazzi 1890, § 66; 1893, p. 55]: "Le sottoclassi principali sono quelle degli ordini d'infinitesimo di x^n, di $x^m (a^{-\frac{1}{x}})^{n_1} = x^m z_1^{n_1}$, di $x^m z_1^{n_1} z_2^{n_2}$, ecc."

[105] [Bettazzi 1890, § 66; 1893, p. 55]: "dagli ordini d'infinitesimo di *tutte* le funzioni *possibili* ...."

with addition defined termwise and order defined lexicographically, then the following expresses the content of

> *Bettazzi's Second Embedding Theorem*: If A is a one-dimensional, proper class (of magnitudes) having $n + 1$ principal subclasses $A_0, \dots, A_n$ for some positive integer n, and $a_0, \dots, a_n$ are positive members of $A_0, \dots, A_n$, respectively, then there is an embedding of A into $B(n)$ that sends a_i to 1_i for each i such that $0 \leq i \leq n$.

Having established his second embedding theorem Bettazzi briefly turned to the question of whether an analog could be established for one-dimensional, proper classes of magnitudes having infinitely many principal subclasses. He was quite pessimistic about this possibility, however, remarking:

> it looks to me as if the concept of measure is not *in general* applicable to classes of this kind.[106]

It was not until the publication of Hahn's aforementioned embedding theorem [1907; also see Ehrlich 1995] that Bettazzi's contention was shown to be wrong.[107]

12. The emergence of non-Archimedean systems of magnitudes VI: Veronese's non-Archimedean continuum

It is important to emphasize that unlike Bertrand Russell, Cantor's other supporters in his attack on infinitesimals did not necessarily share his rejection of infinitesimals *per se*.[108] Vivanti [1891a; 1891b], Peano [1892] and Pringsheim [1898–1904], in particular, all proclaimed the logical cogency of the infinitesimals contained in Stolz's non-Archimedean systems or in systems much like them. Like Cantor, however, they expressed doubts about the use of infinitesimal line segments in geometry–in particular, according to Cantor and Peano, Veronese's use of them in his pioneering work on non-Archimedean geometry [1891; 1894].

Intimations of Veronese's introduction of infinitesimal line segments into geometry first emerged in 1889 in his paper entitled *Il continuo rettilineo e l'assioma V d'Archimede* (The Rectilinear Continuum and Axiom V of Archimedes). It was also in this early contribution to the theory of magnitudes that Veronese first proposed the math-

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[106] [Bettazzi 1890, § 149; 1893, p. 156]: "mi sembra che il concetto di misura non sia applicabile *in generale* alle classi di questo genere."

[107] Although Bettazzi made no attempt to link his pessimism to considerations involving the Axiom of Choice, it is perhaps worth reiterating in connection with this our earlier observation that the various extant proofs of *Hahn's Embedding Theorem* make use of the Axiom of Choice (or some equivalent thereof), an axiom that Bettazzi opposed. In fact, the role played by the axiom in the extant proofs of the embedding theorem goes far beyond the role played by it in the proof of the decomposition theorem referred to above in Note 101. Moreover, Hahn went so far as to suggest that the theorem could not be proved without the axiom (or some equivalent thereof) [Hahn 1907, pp. 602–603; Ehrlich 1995, p. 178].

[108] Actually, as we have already mentioned in Note 4, even Russell accepted the existence of some rather trivial examples of infinitesimals.

ematico-philosophical thesis that the concept of a rectilinear continuum is independent of the Archimedean axiom. This thesis, which presents a direct challenge to the Cantor-Dedekind philosophy of the continuum was developed in great detail by Veronese in his *Fondamenti di Geometria* [1891; 1894] and further promoted by him in a number of supplementary works [1893–94; 1897; 1898, 1898a; 1902; 1909].

Veronese begins his investigation by remarking:

> In my study on the foundations of geometry of arbitrarily many dimensions which will be published very soon, I occupy myself with the continuum. I note that Mr. Stolz has revealed the importance of Axiom V of Archimedes' famous work "The Sphere and Cylinder." Given two segments A and B, $A < B$, then according to this axiom ... there always is a finite whole number n such that $A.n > B$. Mr. Stolz believed that from the principle of the continuum it was possible to deduce this property; but especially after conceiving the new infinite and actual infinitesimal which can satisfy my Principles I-V of the continuum I persuaded myself it is not possible to deduce the above axiom from a principle of continuity that does not contain in any way the principle in question. The definition of the continuum employed by Stolz supposes implicitly the Axiom of Archimedes and, accordingly, his demonstration of this property is superfluous.
>
> The aim of this note is therefore to clarify the position occupied by the Archimedean Axiom in the principles of the rectilinear continuum and to deduce some important properties which are usually assumed as axioms without admitting any new ones.[109]

Already in Veronese's paper of 1889, the reader finds the unfortunate marriage of attributes that became characteristic of Veronese's contributions to non-Archimedean mathematics more generally: on the one hand, one finds ideas of a highly imaginative and profoundly original mathematical thinker, and on the other, one finds ideas that run the gamut from obscure to poorly stated to outright suspicious. In truth, evidence of this montage of virtues and foibles is already present in the paper's opening paragraph. Indeed, even if one grants Veronese his important and prophetic contention that the concept of rectilinear continuum is independent of the Archimedean condition it is certainly misleading to suggest as Veronese does that Stolz's attempt to demonstrate this property using (a version of) Dedekind's continuity condition is superfluous since Dedekind's continuity condition contains the Archimedean condition. The same point was made twelve years later by Otto Hölder in his influential pioneering work "*Die*

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[109] [Veronese 1889, p. 603]: "Nei miei studi sui fondamenti della geometria a quante si vogliano dimensioni, che saranno presto pubblicati, ho dovuto occuparmi anche del continuo. È noto che il sig. O. Stolz ha rilevato l'importanza dell'assioma V della celebre opera di Archimede "De sphaera et cylindro". Dati due segmenti rettilinei A e B, $A < B$, secondo questo assioma ... vi è sempre un numero intero finito n tale che $A.n > B$. Il sig. Stolz ha creduto che dal principio del'continuo si potesse dedurre questa proprietà; ma specialmente dopo la consizione di nuovi infiniti e infinitesimi attuali che pur soddisfano ai miei principî I-V sul continuo mi sono persuaso che non si può dedurre l'assioma suddetto dal principio della continuità se in qualche modo non è contenuto in questo principio stesso. La definizione del continuo del sig. Stolz suppone implicitamente l'assioma d'Archimede, e la sua dimostrazione di questa proprietà è quindi inutile.

Lo scopo della presente Nota è dunque di far risaltare il posto che occupa l'assioma d'Archimede tra i principî del continuo rettilineo, e di dedurre alcune proprietà importanti che sono assunte comunemente come assiomi, senza ammetterne di nuove."

Axiome der Quantität und die Lehre vom Mass," (The Axioms of Quantity and the Theory of Measurement). As Hölder put it:

> Veronese ... has claimed that the concept of a continuum must be stated differently from the way it is given by Dedekind, that Dedekind's axiom (our axiom VII) contains the Archimedean axiom, and, furthermore ... that Stolz's definition of continuity (in his Vorlesungen über Arithmetik, p. 82) assumes the Archimedean axiom and that, consequently, Stolz's proof of this axiom is superfluous.
>
> The comment that the Archimedean axiom is "contained" in Dedekind's axiom of continuity could lead to misunderstandings. I emphasize that the Archimedean axiom can be deduced from axiom VII aided by [our] axioms I to VI, but only via the proof given in the text or something similar, which is why such a proof is by no means superfluous. [Hölder 1901, p. 10 in Hölder 1996, p. 248; (Translation Michell and Ernst)]

However, while Hölder certainly found reasons to criticize aspects of Veronese's paper, he also found much there which was valuable including the basis of his own system of axioms for magnitudes. More specifically, axioms I to VI of Hölder's system collectively constitute a straightforward variation of Veronese's Principles I-III; indeed, it is essentially the most straightforward variation that is required to limit the range of models of Veronese's three principles to totally ordered systems. Hölder essentially acknowledged as much when he wrote:

> For convenience, it has been assumed that no equal magnitudes exist which are discernible, that is, non-identical. Consequently, the axioms [stated by Veronese] that two magnitudes are equal when they are equal to a third magnitude and that equal added to equal results in equal are unnecessary. Of course, these facts must be considered in applications. ... Axioms I–VI, used here, correspond to Veronese's principles I–III .... [Hölder 1901, pp. 4–5 in Hölder 1996, p. 247; (Translation Michell and Ernst)]

Yet, despite Hölder's remark, Veronese's contribution has been all but forgotten. This is especially apparent in the writings of contemporary measurement theorists where the publication of Hölder's system of axioms is treated as a watershed event (cf. [Krantz, Luce, Suppes and Tversky, 1971, p.71]).

Veronese's Principles I–III, which collectively consist of ten assertions, are stated as follows.

> **Principle I**. (1) If A and B are arbitrary objects (*magnitudes*) of a system Σ, one and only one of the following relations is valid: $A = B$ (A is *equal* to B), $A > B$ (A is *greater than B*), $A < B$ (A is *less than B*).
> (2) If $A = B$ and $B = C$, then $A = C$.
> (3) If $A = B$ and $B > C$, then $A > C$.[110]

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[110] [Veronese 1889, p. 604]: "**Princ. I**. (1) Se A e B sono oggetti qualunque (*grandezze*) di un sistema dato Σ, si ha una ed una sola delle relazioni: $A = B$(A *uquale* B), $A > B$ (A *maggiore* di B), $A < B$ (A *minorie* di B).
(2) se $A = C$, $A = B$ è $B = C$.
(3) se $A = B$, $B > C$ è $A > C$."

Principle II. (1) If A and B are arbitrary objects (magnitudes) of a system Σ, the sign $A + B$ indicates one and only one object of the system, and we have:
(2) $A + (B + C) = (A + B) + C$
(3) $A + B > A$, $A + B > B$.
If $A < B$, there are objects X and X' in Σ such that
(4) $A + X = B$
(5) $X' + A = B$.
(6) If $A = A'$ and $B = B'$, we have $A + B = A' + B = A + B'$ and hence $A' + B'$.[111]

Principle III. In the system there is no minimal interval (magnitude) if the zero is excluded.[112]

Veronese's Principles I and II collectively insure that the equivalence classes of the members of Σ with order and addition suitably defined is the strictly positive cone of an ordered group; and Principle III, given the satisfaction of Principles I and II, is equivalent to assuming (in modern parlance) the *density* of Σ, that is, for all A and B in Σ where $A < B$, there is a C in Σ such that $A < C < B$.

It became evident from the aforementioned work of Hölder that Veronese's three principles together with Dedekind's continuity condition implies both divisibility and commutativity of addition. Also evident from Hölder's work is that Principles I–III together with Dedekind's continuity condition implies the Archimedean condition, and that Principles I–III together with the Archimedean condition are sufficient to obtain commutativity of addition. It is of interest to note that these corollaries of Hölder's results for totally ordered systems are all that can be salvaged from an earlier more general attempt of Veronese. Indeed, in his paper of 1889 Veronese attempted to show that both divisibility and commutativity of addition are consequences of Principles I–III together with his generalization of Dedekind's continuity condition. However, by 1901 Hölder had already provided a nondivisible, non-Archimedean model of Veronese's Principles I–III together with Veronese's generalization of the Dedekind's continuity condition; and more recently, Banaschewski [1957] has shown how to construct ordered groups whose strictly positive cones are examples of non-Archimedean systems that satisfy Principles I–III together with Veronese's continuity condition but which fail to satisfy commutativity.

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[111] [Veronese 1889, p. 604]:"**Princ. II.** (1) Se A e B sono oggetti qualunque del sistema Σ, il segno $A + B$ indica uno ed un solo oggetto del sistema, e si ha:
(2) $A + (B + C) = (A + B) + C$
(3) $A + B > A$, $A + B > B$.
Se $A < B$, vi sono in Σ oggetti X e X' tali che
(4) $A + X = B$
(5) $X' + A = B$.
(6) Se $A = A'$, $B = B'$ si ha: $A + B = A' + B = A + B'$, e perciò $= A' + B'$."
[112] [Veronese 1889, p. 610]: "**Princ. III.** Nel sistema Σ non vi è un intervallo (grandezza) minimo se si esclude lo zero."

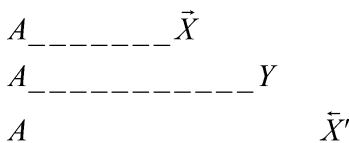$$A _____\vec{X}$$
$$A _____Y$$
$$A _____\bar{X}'$$

Figure 2

Nevertheless, while Veronese's attempt to establish divisibility and commutativity of addition using his continuity condition was ill-fated, his continuity condition was not, though he has not always been given the credit for it he deserves. Indeed, as we have recently emphasized [Ehrlich 1994a, pp. xx–xxi; 1997, pp. 224–225], during the decades bracketing the turn of the twentieth-century Veronese's *absolute continuity condition* (as Veronese called it) was widely discussed by numerous authors including Levi-Civita [1892/93; 1898; also see Laugwitz 1975], Hölder [1901, pp. 10–11; 1924, p. 89], Schoenflies [1898, p.105; 1906, p. 27; 1908, pp. 58–64], Brouwer [1907 in 1975, pp. 49–50], Vahlen [1907], Vitali [1912, pp. 133–134], Enriques [1907, pp. 37–38; 1911, pp. 38–39], and Hahn [1907, p. 603]. However, when it reentered the theory of ordered algebraic systems through the work of Baer [1929] and Cohen and Goffman [1949; 1950] its connection with Veronese was overlooked. Since that time it has been rediscovered by several authors [Aguiló Fuster 1963; Scott 1969; Massaza 1970–71], and is well known to contemporary logicians and order-algebrists alike, albeit under a variety of other names (cf. [Baer 1970; Hauschild 1966; Priess-Crampe 1983; and Ehrlich 1997]).[113]

Following a long string of definitions and subsequent elaborations [pp. 610–612], Veronese states his absolute continuity principle as follows where, contrary to now standard usage, "(XX')" is understood to denote a closed interval whose extremities X and X' are distinct.

> **Principle IV**. If an interval (XX') whose extremities always vary in opposite directions becomes indefinitely small, it always contains an element Y of Σ distinct from X and X'.[114]

Veronese understands his principle to apply to a variable interval (XX') that is a subinterval of an arbitrary closed interval (A, B) in Σ; in particular, it is concerned with two subintervals (A, X) and (A, X') of (A, B) where $(A, X) < (A, X')$ and $(XX') = (A, X') - (A, X)$. While keeping A fixed and preserving the condition that $(A, X) < (A, X')$, X is envisioned to increase in a strict monotonic fashion as X' simultaneously decreases in a strict monotonic fashion. (See Fig. 2)

Against this backdrop Principle IV asserts: if X may increase in a strict monotonic fashion as X' simultaneously decreases in a strict monotonic fashion so that for each interval Z in Σ X and X' take on distinct values for which $(XX') < Z$ (i.e., "(XX') ... becomes indefinitely small"), then there is an interval (A, Y) in Σ such that $(A, X) < (A, Y) < (A, X')$ for all such values of X and all such values of X'.

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[113] Two authors who do attribute the principle to Veronese are Neumann [1949, p. 215] and Laugwitz [1975, p. 308].

[114] [Veronese 1889, p. 612]: "**Princ. IV**. Se l'intervallo (XX') i cui estremi sono sempre variabili in verso opposto diventa indefinitamente piccolo, esso contiene sempre un elemento Y di Σ distinto da X e X'."

The reader will notice that insofar as Σ is assumed to contain no least member and X and X', which are distinct, are assumed to vary simultaneously, X cannot assume a greatest value and X' cannot assume a smallest value. Accordingly, if one simply replaces the references to the variables X and X' with references to the collections of values the variables assume, then on the basis of Veronese's definitions we arrive at the following crisp formulation of the condition that was originally made popular by Hölder [1901, pp. 10–11; 1996, p. 248] and that is frequently employed in the literature today:

> If X and X' are nonempty subsets of Σ where X has no greatest member, X' has no smallest member, and every member of X precedes every member of X', then if for each δ in Σ there are elements a of X and b of X' for which $b - a < \delta$, there is a z in Σ lying strictly between the members of X and those of X'.[115]

Moreover, since the element z referred to in Hölder's formulation is unique, as Veronese himself notes [1889, p. 612], the condition can also be stated in the following form employed by Schoenflies [1906, p. 29] and Brouwer [1907/1975, pp. 49–50] that more clearly highlights its relation to the Dedekind continuity condition.

> If X, X' is a partition of Σ into nonempty subsets for which every member of X precedes every member of X', and if for each element δ of Σ there are elements a of X and b of X' such that $b - a < \delta$, then either X has a greatest member or X' has a least member, but not both.[116]

Veronese, of course, was well aware of the intimate relation that exists between the two continuity conditions but he believed his was preferable to Dedekind's. In a footnote to his continuity axiom Veronese writes:

> It seems to me that this [continuity] principle is easier to intuitively justify than the others, even the one given by Mr. Dedekind, which is important to take into account in the foundations of geometry, whose axioms must be derived from spatial intuition without regard to all the possible abstract hypotheses that do not contradict those axioms. In my opinion, it is not the division of the elements of an interval (A, B) into the two groups (X) and (X') [of values X and X' respectively] such that we always have $(A, X) < (A, X')$ that leads to the postulate of the continuum [presumably, the assertion that an element

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[115] Hölder originally stated the condition thus:

There are two classes of magnitudes, the magnitudes x and the magnitudes x'; no magnitude can belong to both classes at the same time, which does not necessitate that both classes together comprise the totality of all magnitudes or all magnitudes within an interval. Each magnitude, x, should be less than any magnitude, x'; there should be no greatest magnitude in x and no least magnitude in x'; and for each magnitude, δ, in the totality of all magnitudes, an x and an x' can be found such that $x' - x < \delta$. Veronese's postulate implies that under these assumptions there exists a magnitude which lies between the two classes and which differs from x and x'. [Hölder 1901, pp. 10–11 in Hölder 1996, p. 248; (Translation Michell and Ernst)]

[116] The reader will notice that if Σ is assumed to be a group rather than the strictly positive cone thereof, δ would have to be assumed to be a strictly positive element of Σ. The same applies to Hölder's formulation of Veronese's continuity condition.

determines the cut], but rather it is that (X, X') becomes indefinitely small. In an absolute continuum ... [to be defined below] ... there [may] exist these divisions without (X, X') becoming indefinitely small, and thus without there being elements Y which determine them ....[117]

On the other hand, as Veronese goes on to add:

> As we shall see, the postulate of Dedekind ... [which guarantees the absence of divisions where (X, X') cannot become indefinitely small] ... is proven with axiom V of Archimedes ....[118]

Indeed, it is a simple matter to show that in the Archimedean case, and only in the Archimedean case, Veronese's metrical condition on cuts is invariably satisfied. Thus, for Veronese, unlike Dedekind, continuous systems of magnitudes need not be completely devoid of Dedekind gaps, although they must be devoid of those Dedekind gaps which satisfy the metrical condition satisfied in the classical case.

As we alluded to above, Hölder introduced a non-Archimedean model of Veronese's Principles I–IV that fails to satisfy divisibility. Given the instructive nature of his construction and the light his accompanying discussion sheds on our just-stated remarks, we reproduce the relevant portion of Hölder's discussion in its entirety.[119] According to Hölder:

> A system of objects which satisfies Veronese's axiom of continuity, but not the Archimedean axiom nor axiom VII [Dedekind's continuity axiom], is obtained in the following way: consider all functions of y of the form $ay + by^2$, where a is a positive integer or zero and b any real (finite) numerical value, but where, when $a = 0$, b must be positive and non-zero. If one stipulates that of the two functions, $a_1 y + b_1 y^2$ and $a_2 y + b_2 y^2$, the first is called the greater when $(a_1 y + b_1 y^2) - (a_2 y + b_2 y^2)$ is positive for small positive values of y, and when the addition of functions is defined in the usual [termwise] way, then one can see that [our] axioms I to VI are fulfilled.
>
> If one has two classes of functions satisfying the conditions of the modified Veronese axiom, functions $\alpha y + \beta y^2$ in the first class and functions $\alpha' y + \beta' y^2$ in the second class, then one should be able to find the two functions, $\alpha_0 y + \beta_0 y^2$ and $\alpha'_0 y + \beta'_0 y^2$, from the first and second classes, such that $(\alpha'_0 y + \beta'_0 y^2) - (\alpha_0 y + \beta_0 y^2) < y^2$. From this inequality it follows that $\alpha'_0 = \alpha_0$. If one considers all the functions of the first class which are $> \alpha_0 y + \beta_0 y^2$ and all functions of the second class which are $< \alpha'_0 y + \beta'_0 y^2$, then all

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[117] [Veronese 1889, p. 612]: "A me pare che questo principio si giustifichi intuitivamente meglio degli altri, anche di quello dato dal sig. Dedekind, del che bisogna tener gran conto nei fondamenti della geometria, i cui assiomi devono derivare dall'intuizione spaziale senza per questo trascurare tutte le ipotesi astratte possibili che non contraddicono a questi assiomi. Secondo me non è la divisione degli elementi di un intervallo (A, B) in due gruppi (X) e (X') tali che si abbia sempre $(A, X) < (A, X')$ che ci conduce al postulato del continuo, ma bensì il fatto che (X, X') diventa indefinitamente piccolo. Nel continuo assoluto ... vi sono di queste divisioni senza che (X, X') diventi indefinitamente piccolo e quindi senza che vi siano elementi Y che le determinano...."

[118] [Veronese 1889, p. 612]: "Coll'assioma V d'Archimede si dimostra, come vedremo, il postulato di Dedekind ...."

[119] To see that Hölder's model is not divisible one merely has to notice that while the function $1 \cdot y$ is a member of his model the function $1 \cdot y/2$ is not.

these functions will be of the form, $\alpha_0 y + by^2$, and differ only in the values of b. In this way two classes of values of b are obtained which again satisfy the conditions in question, and, since we are now dealing with ordinary, real numerical values, one can conclude that there exists a value, b_0, falling between these two classes. The function, $\alpha_0 y + b_0 y^2$, is the one whose existence is stated by Veronese's postulate. This postulate is therefore fulfilled in its modified, i.e., more general, form.

Since no multiple of y^2 is greater than y, the Archimedean axiom is not satisfied here. From this alone, it follows that Dedekind's axiom of continuity is not valid here; this becomes immediately apparent when one allocates the functions for which $a = 0$ to a first class and those where $a > 0$ to a second.

Therefore, Veronese's postulate is not equivalent to Dedekind's axiom (VII), although axioms I to VI have been assumed right from the beginning. On the other hand, it can be easily seen that under the assumptions made this last axiom is equivalent to the fact that an infinite number of magnitudes, which are less than a certain defined magnitude which does not belong to them, always have a so-called "upper bound." [Hölder 1901, p. 12 in Hölder 1996, p. 249; (Translation Michell and Ernst)]

While Hölder's just-described system satisfies Veronese's continuity condition, it is not an *absolute continuum* in Veronese's sense. According to Veronese [1889, p. 613], for a model of Principles I–IV to be so designated it must also satisfy his

Principle V. If α and β are two intervals of the system and $\alpha < \beta$ there is a determined symbol of multiplication (number) η such that $\eta\alpha > \beta$.[120]

Principle V is, of course, a straightforward generalization of the Archimedean axiom. Indeed, as Veronese notes:

If the number η is always in the class of natural numbers 1 2 3 ... n ... then principle V is called *axiom V of Archimedes*.[121]

"In this case," says Veronese,

the system Σ is called [an] *ordinary continuum*.[122]

Veronese's emphasis on "multiplication" in the statement of Principle V is significant since with the exception of the Archimedean case the operation required by Principle V is not reducible to repeated addition. This foreshadows the fact that the non-Archimedean models of Veronese's principles offered by Veronese [1891], Levi-Civita [1892–93; 1898], and others are strictly positive cones of non-Archimedean ordered fields and non-Archimedean ordered rings. It is the members of a given such system that are infinitely large and infinitely small, relative to the multiplicative identity (i.e., the unit element) of the system, which are respectively the "new infinite and actual infinitesimal" entities that Veronese mentioned in the opening paragraph of his paper. Veronese says very little

[120] [Veronese 1889, p. 613]: "**Princ. V**. Se α e β sono due intervalli del sistema ed è $\alpha < \beta$ vi è sempre un simbolo di molteplicità (numero) η determinato tale che $\eta\alpha > \beta$."
[121] [Veronese 1889, p. 613]: "Se il numero η è sempre della classe naturale 1 2 3... n ... il principio V si chiama *assioma V d'Archimede*."
[122] [Veronese 1889, p. 613]: "In tal caso il sistema Σ si chiama continuo ordinario."

about these entities in his paper of 1889. However, with regard to the numbers η referred to in Principle V he does indicate that

> There is no place here for Cantor's transfinite numbers because they have neither the second part of property II_3 nor property II_5....[123]

On the other hand, as he goes on to add:

> Our infinite whole [i.e., natural] numbers satisfy this condition, but it is not our present aim to introduce the properties of these numbers that enlarge the field of abstract continua. In our book, which is now being printed, these numbers will be discussed at length with the application of absolute continua to geometry itself.[124]

Besides characterizing a one-dimensional absolute continuum as above, Veronese considers a number of variations of his system including those containing a zero element with or without directed elements as well as totally ordered versions thereof. However, since these variations, which are motivated by the straight line of geometry, do not add anything significantly new to the present discussion, we will not consider them further at this time.

13. Stolz's rethinking of continuity and of infinitesimal line segments

As is evident from its opening remarks, Veronese's paper of 1889 appears to have been written in part as a response to Stolz's early acceptance of the Cantor-Dedekind philosophy of the continuum. In fact, as Veronese explains in a note affixed to the paper's title, the paper itself is an extract of a manuscript addressing these matters that he sent to Stolz "last June" [1889, p. 603: Note 1]. Veronese appears to have also used that opportunity to draw Stolz's attention to defects in the "proof" contained in Stolz's *Allgemeine Arithmetik* that Dedekind continuity implies the Archimedean axiom. Stolz's response to all of this, which was announced in the just-cited footnote of Veronese's paper, appeared under the title *Ueber das Axiom des Archimedes* [1891]. Veronese was undoubtedly quite pleased with Stolz's response for not only did Stolz revise the just-cited proof in light of Veronese's criticisms but he openly proclaimed that

> Veronese ... has recognized that the Axiom of Archimedes is not a consequence of continuity....[125]

As in his earlier discussions of continuity, Stolz's [1891] is concerned with a system having all the properties of an absolute system of magnitudes except that the more

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[123] [Veronese 1889, p. 613]: "Ciò non ha luogo pei numeri transfiniti di G. Cantor perchè per essi non ha sempre luogo la seconda delle proprietà II_3 et la proprietà II_5...."

[124] [Veronese 1889, p. 613]: "I nostri numeri interi infiniti soddisfano a questa condizione, ma non è nostro scopo di far conoscere qui le proprietà di questi numeri che allargano il campo del continuo astratto. Nel nostro libro, che sta ora sotto stampa, questi numeri saranno trattati ampiamente per l'applicazione del continuo assoluto alla geometria stressa."

[125] [Stolz 1891, p. 107]: "Veronese ... erkannte ... dass das Axiom des Archimedes nicht Folge der Stetigkeit sei ...."

general conditions of density and absence of a least element are assumed in place of divisibility. And, as in his earlier discussions, central to his analysis is the now familiar notion of a *gap*.[126] What is new to Stolz's later treatment, however, is the following result that was established independently by Bettazzi [1890] and which, as we shall later see, plays an implicit role in Bettazzi's own treatment of continuity:

> If a system Π has the properties specified above, then Π is Archimedean if and only if for each gap (P_1, P_2) of Π and each magnitude D in Π there are magnitudes p_1 in P_1 and p_2 in P_2 such that $p_2 - p_1 < D$. [1891, p. 108][127]

In his earlier works, Stolz attempted to show that a system Π having the properties specified above is Archimedean, if Π is continuous, by showing: i) if Π is continuous, then Π has the least upper bound property, and ii) if Π has the least upper bound property, then Π is Archimedean. However, in virtue of the above result it was evident that if Π is non-Archimedean, then it contains a gap, say, (P_1, P_2), which in turn implies the failure of the least upper bound principle since P_1 has no such bound. Accordingly, having embraced Veronese's contention that a continuous system of magnitudes need not be Archimedean, Stolz was forced to renounce the above line of reasoning by rejecting (i). In the final sentence of his paper Stolz did so when he wrote:

> Consequently, it is not permissible, as I formerly maintained, to regard this axiom [i.e., the Archimedean axiom] for line segments as a consequence of continuity or to prove it by means of the concept of bound.[128]

Moreover, having accepted the logical possibility of a non-Archimedean geometrical continuum, Stolz would also have to revise his earlier view by accepting the consistency

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[126] Stolz uses the term "gap" in its contemporary sense. That is, by a gap of Π Stolz means a partition of Π into two groups P_1 and P_2 (that are tacitly assumed to be nonempty) having the following properties: (i) each magnitude of Π is contained in one and only one of the groups; ii) whenever p_1 is a magnitude in P_1, every magnitude of Π smaller than p_1 is in P_1, and whenever p_2 is a magnitude in P_2, every magnitude of Π greater than p_2 is in P_2; (iii) P_1 contains no greatest magnitude and P_2 contains no smallest magnitude [1883, p. 508; 1885, p. 81; 1891, pp. 107–108].

[127] Appended to Stolz's statement of the theorem is the following footnote:

> After finishing this note I found that these comments have already been made by Mr. Bettazzi in his Accademia dei Linci prize-winning 'Teoria della grandezze' (Pisa 1890). [1891, p. 108]

[128] [Stolz 1891, p. 112]: "Es ist mithin nicht zulässig, wie ich früher behauptete, dieses Axiom für die Strecken als eine Folge der Stetigkeit anzusehen oder mittelst des Begriffes der Grenze zu beweisen."

of line segments that are infinitesimal relative to other segments. He essentially did as much the same year when in his *Grössen und Zahlen* (Magnitude and Number) he wrote:

> we have been successful in our day in setting up consistent infinitely large and infinitely small magnitudes ....[129]

and included among his examples

> the new actual infinities and infinitesimals announced by ... Veronese [in his [1889]].[130, 131]

14. Does the actual infinitesimal exist? The Bettazzi-Vivanti debate I: Vivanti's opening arguments

The same year that Stolz made the above announcements, Giuseppe Peano began the publication of *Rivista di matematica*, a journal devoted to issues in the foundations of mathematics, and mathematical philosophy more generally. To help inaugurate his journal Peano invited Bettazzi, Vivanti, and Veronese to engage in a debate on the question of the existence of infinitesimals. Vivanti and Bettazzi accepted the invitation, but Veronese declined because he was "busy with the publication" of his *Fondamenti di Geometria*, a book which in any case "already dealt with the subject in great depth" [Veronese 1891, p. 622; Veronese 1894, p. 702]. As it turned out, the exchange extended over the first two volumes of the *Rivista* and consisted of four installments: an opening article by Vivanti [1891a], a response by Bettazzi [1891], a response to Bettazzi's response by Vivanti [1891b], and a final response by Bettazzi [1892]. And while Veronese did not formally participate in the exchange, when his *Fondamenti di Geometria* appeared in 1891 he used the opportunity to respond to remarks made in [Vivanti 1891a] about his earlier work and the purported proof of Cantor [Veronese 1891, pp. 622–625].

The exchange between Bettazzi and Vivanti is interesting not only because it provides us with detailed insight into the thinking of two of the participants in the general late nineteenth-century debate but because it offers us a glimpse of the state of the debate immediately preceding the advent of non-Archimedean geometry. Moreover, and what is perhaps most important, it brings to the fore in an unmistakable fashion the fact that the issues at stake in the debate were not always restricted to what would today be regarded as straightforward mathematical questions, but also embraced considerations arising from competing philosophies of geometry, competing philosophies of the infinite, com-

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[129] [Stolz 1891a, p. 16]: "ist es in unseren Tagen auch gelungen, widerspruchsfreie actuale unendlich grosse und unendlich kleine Grössen aufzustellen."

[130] [Stolz 1891a, p. 16]: "Veronese angekündigten neuen infiniti und infinitesimi attuali ...."

[131] While Stolz never changed his mind about the consistency of infinitesimal line segments, his embrace of Veronese's philosophy of the continuum appears to have been short-lived. In his (and Gmeiner's) *Theoretische Arithmetik* [1902, pp. 113–117], Stolz not only returned to his original embrace of the Cantor-Dedekind philosophy but made no mention of Veronese's theory. He did, however, remind the reader that "due to criticisms of the proof raised by G. Veronese," the proof that continuity implies the Archimedean axiom contained in his *Allgemeine Arithmetik* was withdrawn [1902, p. 114].

peting philosophies of the continuum and competing philosophies of mathematics more generally.

Vivanti opens the exchange with a number of historical remarks including the observation that infinitesimals have been interpreted in the literature in a variety of fashions, which he lists as follows:

a) The infinitesimal is a null quantity (Euler);
b) It is a finite variable quantity that tends to zero (Carnot, Cauchy);
c) It is an entity of a nature different than that of ordinary quantities, an intensive magnitude, devoid of extension, that acts as a generating moment of the quantities (Newton, Kant);
d) It is an ordinary quantity that is so small that when repeated any finite number of times doesn't form any assigned finite quantity (Poisson, Du Bois-Reymond).[132]

After identifying the latter interpretation–which he erroneously attributes to Poisson and du Bois-Reymond–with "*the actual infinitesimal*,"[133] he observes:

While the actual infinitesimal has been banished from the calculus, it has reappeared in other parts of mathematics. To examine its nature and the role that it has in this science, it is necessary first of all that I define it exactly.[134]

"What is the meaning of the term *infinitesimal*?," asks Vivanti, and he answers the question, albeit somewhat clumsily, as follows:

Let us divide a unit into n [equal] parts; if m is an arbitrary number smaller than n, m of those parts (m n-esimals) will not be sufficient to form the unit. We now let n increase beyond any limit until it becomes infinite, any part of the unit can now be called an *infinitesimal*, and, if m is an arbitrary number smaller than n, m of those parts (m n-esimals) will not be enough to form the unit. Accordingly, we have been able to characterize the infinitesimal by means of this property, that is, *when repeated any finite number of times, it*

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[132] [Vivanti 1891a, p. 136]:
"*a*) L'infinitesimo è una quantità nulla (Eulero);
 b) Esso è una quantità finita, variabile e tendente a zero (Carnot, Cauchy);
 c) Esso è un ente di natura diversa dalle quantità ordinarie, una grandezza intensiva, priva d'estensione, che funge quale momento generatore delle quantità (Newton, Kant);
 d) Esso è una quantità ordinaria tanto piccola, che ripetuta un numero finito qualsiasi di volte non forma mai una quantità finita assegnata (Poisson, Du Bois-Reymond)."

[133] While Poisson and du Bois-Reymond certainly championed "*the actual infinitesimal*," it is misleading for Vivanti to attribute characterization d–which closely resembles one of the modern conceptions of an actual infinitesimal–to them. After all, according to Poisson "[a]n infinitely small magnitude is a magnitude less than any magnitude of the same nature" [Poisson, 1883, p. 16]; and, as Veronese correctly observed, "[t]his proposition evidently contains a contradiction in terms" [1891, p. 622]. Moreover, as Veronese repeatedly emphasized, du Bois-Reymond appears to have never actually defined precisely what he meant by an infinitesimal [1894, p. 700; 1909, p. 198; 1994, p. 169].

[134] [Vivanti 1891a, p. 137]: "Ma l'infinitesimo attuale, bandito dal Calcolo, ricompare in altri campi della Matematica. Per esaminare la sua natura e la parte che esso ha in questa scienza, ci è d'uopo anzitutto definirlo esattamente."

never constitutes the unit (or any finite determined quantity). The infinitesimal so defined appears like a constant quantity of the same nature as $1/2, 1/3, \ldots, 1/n, \ldots$.[135]

"[I]t is this sort of *actual infinitesimal* of which we want to speak" [1891a, p.137], says Vivanti, and with this understood he poses the question:

Does the actual infinitesimal exist?[136]

Answering this question is the focus of the remainder of the essay, beginning with a brief section entitled *Cantor's Demonstration of the non-Existence of Infinitesimals.* Vivanti opens the section by remarking:

First of all, we should orient ourselves to see in which domain we should seek this entity, the existence of which we have put in discussion.
All the segments that can be considered upon an unspecified straight line are of one of two kinds:
Either we have a segment bounded by the two points A, B of the line (segment AB);
Or we have a segment consisting of the portion of a straight line, located to the right or to the left of a point O of the line (segment $O\infty$ or ∞O)....
The segments of the first kind are called *finite*, and those of the second *infinite*.[137]

Having thus placed himself squarely in the classical tradition regarding the distinction between finite and infinite line segments, Vivanti informs us that if we take a finite segment as a unit of measure the question of the existence of an infinitesimal segment reduces to the question whether or not the following is the case:

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[135] [Vivanti 1891a, p. 137]: "Sia divisa l'unità in *n* parti; se *m* è un numero qualunque minore di *n*, *m* di quelle parti (*m n*-esimi) non basteranno a formare l'unità. Facciamo ora crescere *n* oltre ogni limite, sino a che divenga infinito; ciascuna parte dell'unità potrà dirsi un *infinitesimo*, e, se *m* è un numero finito qualunque, *m* di quelle parti non potranno mai formare l'unità. Si può adunque caratterizzare l'infinitesimo mediante questa proprietà, che *esso*, *ripetuto un numero finito qualsiasi di volte*, *non forma giammai l'unità* (oppure una quantità finita determinata qualunque). L'infinitesimo così definito ci appare come una quantità costante, della stessa natura di $1/2, 1/3, \ldots, 1/n, \ldots$"

[136] [Vivanti 1891a, p. 137]: "Esiste l'infinitesimo attuale?"

[137] [Vivanti 1891a, p. 137]: "Ci occorre anzitutto orientarci e vedere in quale campo dobbiamo cercare questo ente la cui esistenza abbiamo posto in discussione.
Tutti i segmenti che possono prendersi sopra una retta indefinita sono dell'una o dell'altra di queste due specie:
O si ha un segmento limitato da due punti A, B della retta (segmento AB);
O si ha un segmento costituito dalla porzione della retta posta a destra o a sinistra d'un punto O di essa (segmento $O\infty \circ \infty O$) ....
I segmenti della prima specie si dicono *finiti*, quelli della seconda *infiniti*."

There is a segment such that when repeated a finite number of times, however large that number might be, that would never exhaust an assigned finite segment.[138, 139]

Moreover, says Vivanti:

G. Cantor has answered this question negatively. We are given [however] a brief and incomplete outline how he reached this conclusion ....[140]

Still, adds Vivanti:

It seems that the idea of the proof of Cantor is this. Asserting that ζ is a segment is equivalent to admitting that if we successively arrange a sufficiently large series of segments, all equal to ζ, upon a straight line, we shall of necessity eventually cover the assigned finite segment in its entirety; next Cantor states (and here there is a gap in his explanation) that if this is not possible by means of a finite series of segments, it is impossible by means of an infinite series as well, however extended the series might be.[141]

Having found Cantor's argument to be at best incomplete, Vivanti goes on to construct his own argument against the existence of infinitesimal line segments–an argument that is not only dubious by contemporary standards but that struck his readers as dubious as well.

Vivanti begins laying the groundwork by noting that:

The question of the existence of the infinitesimal could be considered from a little different point of view.[142]

In particular, after stating the "Postulate of Archimedes" Vivanti asserts:

One could now ask: What is the nature of this assertion, and from what does it draw its force?[143]

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[138] [Vivanti 1891a, p. 138]: "Esiste un segmento tale, che, ripetuto un numero finito comunque grande di volte, non esaurisca mai un segmento finito assegnato?"

[139] The reader will notice that in accordance with the classical distinction between finite and infinite segments, every bounded infinitesimal segment is finite. In the companion work to the present paper referred to in the Introduction, we will see that this idea was called into question by Veronese [1891, p. 623; 1894, pp. 703–704].

[140] [Vivanti 1891a, p. 138]: "G. Cantor ha risolto tale questione in senso negativo. Del come egli sia giunto a questa conclusione si trova un accenno breve e incompleto ...."

[141] [Vivanti 1891a, p. 138]: "Il concetto della dimostrazione di Cantor sembra essere questo. Dire che ζ è un segmento equivale ad ammettere che, disponendo successivamente sopra una retta una serie abbastanza grande di segmenti tutti eguali a ζ, si debba di necessità arrivare a coprire per intero un segmento finito assegnato; ora Cantor stabilisce (e qui v'ha una lacuna nella sua esposizione) che, se ciò non è possibile mediante una serie finita di segmenti, non lo è neppure mediante una serie infinita, comunque estesa essa sia."

[142] [Vivanti 1891a, p. 139]: "La questione dell'esistenza dell'infinitesimo può essere considerata sotto un punto di vista un po' diverso."

[143] [Vivanti 1891a, p. 139]: "Ora può chiedersi: Di quale natura è questa asserzione, e da che attinge essa la sua forza?"

"To answer such a question," says Vivanti, "it is appropriate to begin with some general considerations" [Vivanti 1891a, p. 139] about the nature of mathematical objects, which he spells out as follows.

> The objects, whose study forms the subject matter of mathematics, are fictitious entities, perfectly arbitrary creations of our thought, which may have any properties provided that they do not contradict one another. However, since this science, like each of the others, is born from the observation of nature, to a large extent mathematical entities may be said to ensue from the *idealization* of really existing objects; as, for example, the concepts of straight line and plane probably emerged by freeing themselves from the waters of thin taut threads and surfaces.
>
> Consequently, a mathematical entity E somehow constitutes an ideal image of a real object E_1. Above all, the entity E is rigorously defined; that is from *among the most evident* properties of E_1, suitably *idealized*, are chosen properties that are sufficient to distinguish our entity from any other. The choice of these properties can generally be made in more than one way; however, once E's definition is fixed, all of its other properties can be demonstrated from this base. Such properties are likewise *axioms or theorems*, depending on whether they are evident from the definition of E or they are deduced from it by means of a longer chain of ratiocinations.[144]

Before continuing with his preparatory remarks, Vivanti explains why in the preceding observation he merely claimed that "to a large extent mathematical entities may be said to ensue from the *idealization* of really existing objects."

> Today's mathematics, [says Vivanti,] with its tendency to generalize, has also turned attention to entities whose concepts are not derived directly from the observation of existing objects, and whose properties are selected *completely arbitrarily* without the guide of considerations relative to the real world .... [S]uch entities may be said to be *conventional*....[145]

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[144] [Vivanti 1891a, p. 139]: "Gli oggetti, il cui studio forma l'argomento della Matematica, sono enti fittizi, creati dal nostro pensiero in modo perfettamente arbitrario, ed aventi quelle proprietà qualunque (purchè tra loro non contraddittorie) che ci piace d'attribuire ad essi. Però, siccome quella scienza è nata, al pari d'ogni altra, dalla osservazione della natura, gli enti matematici derivano in gran parte, ci sia permessa la frase, dalla *idealizzazione* di oggetti realmente esistenti; così, per esempio, un filo sottile fortemente teso e la superficie libera delle acque hanno dato probabilmente origine ai concetti di retta e di piano.

Sia pertanto E un ente matematico, che costituisca in qualche modo l'imagine ideale d'un oggetto reale E_1. Si tratta anzitutto di definire rigorosamente l'ente E; cioè di scegliere alcune *fra le più evidenti* proprietà di E_1, convenientemente *idealizzate*, e tali che bastino a distinguere il nostro ente da qualunque altro. La scelta di queste proprietà può generalmente farsi in più modi; stabilita però in un dato modo la definizione di E, tutte le altre sue proprietà devono potersi dimostrare in base a questa. Tali proprietà costituiscono altrettanti *assiomi* o *teoremi*, secondochè esse risultano in modo evidente dalla definizione di E o devono dedursi da essa mediante una più o meno lunga catena di raziocini."

[145] [Vivanti 1891a, p. 140]: "La Matematica odierna, per la sua tendenza a generalizzare, ha rivolto l'attenzione anche ad enti il cui concetto non è derivato direttamente dall'osservazione di oggetti esistenti, le cui proprietà sono scelte in modo *completamente arbitrario* e senza la guida di considerazioni relative al mondo reale .... [T]ali enti ... potrebbero dirsi *convenzionali* ...."

As we shall later see, it is presumably this distinction between conventional and nonconventional entities that Vivanti believes permits him to embrace the systems of infinitesimals developed by du Bois-Reymond, Thomae, and Stolz while denying the existence of infinitesimal line segments.

Having concluded his digression, the discussion turns to a sketch of the logically equivalent definitions of the classical linear continuum due to Veronese and Bettazzi using Bettazzi's terminology (see Note 147 for definitions) as a common thread. As Vivanti puts it:

Bettazzi chooses the following to constitute the definition of the continuum:

a) C is a [one-dimensional] *proper class*;
b) C is *connected*;
c) C is *closed*.

From this definition of the continuum the Postulate of Archimedes can be proved.
Veronese instead defines the [ordinary] continuum in terms of the following properties:

a) C is a [one-dimensional] *proper class*;
b) C is *unlimited* [i.e. has no least strictly positive member];
c) C is *closed*;
d) C possesses the property expressed by the Postulate of Archimedes.[146, 147]

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[146] [Vivanti 1891a, p. 140]: "Fra queste Bettazzi sceglie le seguenti a costituire la definizione del continuo:
a) L'insieme considerato è una *classe propria C*;
b) C è *connessa.*
e) C è *chiusa.*

Definito così il continuo, si può dimostrare che per esso ha luogo il postulato d'Archimede.
Veronese definisce invece il continuo colle proprietà seguenti:
a) Esso è una *classe propria C*;
b) C è *illimitata*;
c) C è *chiusa*;
d) C gode della proprietà enunciata nel postulato d'Archimede."

[147] As Vivanti notes, according to Bettazzi [1890, § 48; 1893, p. 41], a *continuous class of one dimension* is a one-dimensional proper class that is both connected and closed. The definitions of connected and closed emerge, for Betttazzi, as the culmination of a series of definitions that unfold as follows. Let C be a proper class in the sense of Bettazzi, and let (X, Y) be a partition of C such that $x < y$ for all $x \in X$ and all $y \in Y$. If X has a maximal member and Y has a minimal member, then (X, Y) is said to be a *succession*; and if X has no maximal member and Y has no minimal member, then (X, Y) is said to be a *section* or a *jump*, respectively, depending upon whether or not the following condition holds: for each $\delta \in C$ there are $x \in X$ and $y \in Y$ such that $y - x < \delta$. [In contemporary parlance, a *section* of C is a Dedekind gap (X, Y) of C such that for each $\delta \in C$ there are $x \in X$ and $y \in Y$ such that $y - x < \delta$; and a *jump* is a Dedekind gap (X, Y) of C that is not a section.] If C exhibits neither successions nor jumps it is said to be *connected* (*connessa*), and if C admits no sections and every section of an arbitrary subclass of C is filled by at least one magnitude of C, it is said to be *closed* (*chiusa*) [Bettazzi 1890, § 48; 1893, p. 40].

As Bettazzi mentions in his *Teoria Delle Grandezze* [1890 § 49; 1893, p. 41], his definition of a "continuous class of one dimension" is intimately related to Cantor's conception of a "continuous

With the preliminaries now in place, Vivanti asks the reader to "consider the collection I of all finite segments on a given straight line having a common extremity on the left, say, *p*" and, with reference to this, attempts to prove the nonexistence of infinitesimal line segments as follows:

> Clearly, if we define addition of segments in the ordinary way our collection constitutes a proper class of magnitudes of one dimension [in the sense of Bettazzi]. To completely characterize this collection, however, we must consider other of its properties and select among them the most evident. Now what among the properties of the considered collection are there which are evident from the intuitive concept we have of the straight line?
>
> In our opinion, it could be maintained as intuitively evident that the collection I is *connected* and *closed*; this being the case it has all the characteristics attributed by Bettazzi to the *continuum*. This implies that the collection I satisfies the postulate of Archimedes and, hence, also that there does not exist an actual infinitesimal rectilinear segment.[148]

Thus, the basis of Vivanti's argument essentially reduces to the contention that the Archimedean axiom is a logical consequence of connectivity and closure, the latter being intuitively evident properties of the straight line that result from suitably idealizing the straight line of experience. Why Vivanti believed it could be maintained as intuitively evident that the straight line thus construed is connected and closed he does not say. Following the presentation of his argument, however, he does go on to add that:

> the definition of [the continuum given by] Veronese ... doesn't appear to be suitable to adopt, because property *d* [i.e., the Archimedean condition] which distinguishes it can not be regarded as evident, as is evidenced by the fact that the opposite hypothesis–the existence of the actual infinitesimal–was and still is admitted by mathematicians and by philosophers.[149]

Presumably, therefore, Vivanti believed no one would ever countenance the possibility of the denial of either connectivity or closure. But if he did, he would have been wrong! A number of mathematicians beginning with Bettazzi and Veronese would soon challenge the claim that connectivity–which implies the Archimedean axiom–is a necessary

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point-set," that is, a point-set that is both perfect and connected in the sense of Cantor (see Note 25). Indeed, as Vivanti [1891a, pp. 57–58] observed in his review of Bettazzi's work, the two definitions are equivalent when applied to the kind of systems of magnitudes with which Bettazzi is concerned–one dimensional proper classes in the sense of Bettazzi.

[148] [Vivanti 1891a, p. 141]: "Definita nel modo ordinario l'addizione dei segmenti, è chiaro che il nostro insieme costituisce una classe propria di grandezze ad una dimensione. A caratterizzare completamente questo insieme dobbiamo prendere altre proprietà di esso, e dobbiamo sceglierle fra le più evidenti. Ora quali tra le proprietà dell'insieme considerato ci risultano come evidenti dal concetto intuitivo che abbiamo della linea retta?

A nostro avviso può ritenersi come evidente, che l'insieme I è *connesso* e *chiuso;* per modo che esso ha tutti i caratteri attribuiti da Bettazzi al *continuo.* Ne risulta che nell'insieme I ha luogo il postulato d'Archimede; quindi anche per questa via siamo giunti alla conclusione, che non esiste un segmento rettilineo attualmente infinitesimo."

[149] [Vivanti 1891a, p. 141]: "alla definizione di Veronese ... non ci sembra da adottarsi, perchè la proprietà *d* che ne fa parte non può considerarsi come evidente,–e ne è prova il fatto, che l'ipotesi opposta–l'esistenza dell'infinitesimo attuale–fu ed è tuttora ammessa da matematici e da filosofi."

property of the straight line. Moreover, challenges to the claim that closure–which implies the absence of Dedekind gaps–is similarly necessay were already implicit in the writings of Cantor [cf. 1882 in 1932, pp. 156–157] and Dedekind. Dedekind expressed the latter matter rather poignantly when, in the preface to his *Was sind und was sollen die Zahlen?*, he wrote:

> in works on geometry continuity is only casually mentioned by name, but is never clearly defined, and therefore cannot be employed in demonstrations. To explain this matter more clearly I note the following example: If we select three non-collinear points *A*, *B*, and *C* at pleasure, with the single limitation that the ratios of the distances *AB*, *AC*, *BC* are algebraic numbers, and regard as existing in space only those points *M*, for which the ratios of *AM*, *BM*, *CM* to *AB* are likewise algebraic numbers, then it is easy to see that the space made up of the points *M* is everywhere discontinuous. But in spite of this discontinuity, and despite the existence of gaps in this space, all constructions that occur in Euclid's *Elements*, can, so far as I can see, be just as accurately effected here as in a perfectly continuous space; the discontinuity of this space would thus not be noticed in Euclid's science, would not be felt at all. If anyone should say that we cannot conceive of space as anything else than continuous, I should venture to doubt it and call attention to the fact that a far advanced, refined scientific training is demanded in order to perceive clearly the essence of continuity and to understand that besides rational quantitative relations, also irrational, and besides algebraic, also transcendental quantitative relations are conceivable. [Dedekind 1888 in Ewald 1996, p. 793; (Translation Ewald)]

Having concluded his argument against the existence of infinitesimal line segments, Vivanti considers three arguments that purport to establish the contrary. The first is based on the geometrical theory of probability, the second has its roots in the history of philosophy, and the third is an attempt to show that the existence of Cantor's infinite ordinals implies the existence of the infinitesimal. However, since only the second was championed by any of the principal late nineteenth-century proponents of infinitesimal line segments, and therefore might have had a nonnegligible impact on the general late nineteenth-century debate, we will limit our attention to it.[150]

The argument in question, as Vivanti emphasizes, has its roots in the philosophy of the continuum and is concerned with the age-old Zenonean question of whether a line may be regarded as being composed of points. Du Bois-Reymond [1882], along with Veronese [1891] and the philosopher-logician, Charles Sanders Peirce, [1898; 1900; 1976] all appealed to arguments of this kind to help establish the need for theories of infinitesimals. It was the versions of the argument developed by du Bois-Reymond in his *Die allgemeine Functionentheorie*, however, that principally exercised Vivanti.

Vivanti broached the matter thus.

> It is assumed by many, that the existence of the infinite harbors the same consequence for that of the infinitesimal. If you could divide the unit into an actually infinite number of parts, each of these, it is said, will be actually infinitesimal. But what will be the nature these parts? More precisely, if we imagine that a finite segment is divided into an infinite

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[150] It is the author's intention that the present paper as well as the companion paper referred to in the Introduction will be incorporated into a more comprehensive work. At that time we will also treat the other two arguments, both of which are rejected by Vivanti and Bettazzi.

number of parts, will each of these be a point or a segment? Here all the arguments come to mind which have been accumulated in philosophy throughout time against the possibility that the continuum is constituted of points. Du Bois-Reymond reproduced some of them in somewhat different forms that necessarily arrive at the conclusion that the actual infinitesimal exists.[151]

Although du Bois-Reymond discussed this issue in a number of places, his central argument on the matter appears in the section of his monograph entitled *The Infinitely Small and its Main Characteristics*. There he writes:

> *The statement that the quantity of division points on the unit segment is infinitely large generates with logical necessity a belief in the infinitely small.*
>
> For if we consider what we presented above as the correct concept of quantity, that points on a length do not follow one another without distance, so cannot adjoin one another, but are always separated by segments, so that points alone can never form a segment, then also the infinitely many points are separated by infinitely many segments, and so finite, that is, finite in number of these segments can be contained in the unit segment, because by the arbitrariness of the unit length, any segment, however small, must have the same character as the unit segment, so that infinitely many division points must again be present on it.
>
> It therefore results *that the unit segment decomposes into infinitely many subsegments of which none is finite*. Thus the infinitely small actually exists. [du Bois-Reymond 1882, pp. 71–72: (Translation Fisher 1981, p. 114)]

In response to this type of argument (and, we suspect, the above argument in particular)[152] Vivanti writes:

> We do not attribute great significance to such arguments, which in substance reduce to the impossibility of conceiving the straight line as composed of points. Here it seems that the same impossibility is even more valid for the infinitesimal segment in that such a segment, being in turn decomposable into infinitesimally smaller segments, does not have the character of an element, and therefore his overture does not budge the problem of the constitution of the continuum but leaves it unresolved.[153]

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[151] [Vivanti 1891a, p. 144]: "Si ammette da molti, che l'esistenza dell'infinito porti come conseguenza quella dell'infinitesimo. Se si può dividere l'unità in un numero attualmente infinito di parti, ciascuna di queste, si dice, sarà attualmente infinitesima. Ma di qual natura saranno queste parti? Più precisamente, se imaginiamo diviso un segmento finito in un numero infinito di parti, ciascuna di queste sarà un punto od un segmento? Qui tornano al pensiero tutti gli argomenti accumulati dalla filosofia d'ogni tempo contro la possibilità che il continuo sia costituito di punti. Du Bois-Reymond, riproducendone alcuni sotto forma un po' diversa, arriva necessariamente alla conclusione che l'infinitesimo attuale esiste."

[152] Vivanti directs the reader to § 21 of du Bois-Reymond's book for the statement of the argument he is concerned with. However, since no such argument is contained in § 21, we suspect his reference should have been to § 24 where the above passage is found.

[153] [Vivanti 1891a, p. 144]: "Noi non sappiamo attribuire gran valore a tali argomenti, i quali si riducono in sostanza alla impossibilità nostra di concepire la retta come formata di punti. Ci sembra anzi che la stessa impossibilità sussista pel segmento infinitesimo, con questo di più, che essendo tale segmento a sua volta decomponibile in segmenti infinitesimi più piccoli, esso non ha

Having rejected du Bois-Reymond's argument along with the two other aforementioned "proofs" of the existence of infinitesimal line segments, Vivanti draws the main body of the paper to a close by summarizing his central conclusions. In addition to reiterating his rejection of the purportedly positive proofs, Vivanti observes that:

1. The infinitesimal calculus does not need anything besides the finite quantities;
2. The field of existence of a real positive variable consists exclusively of finite quantities, with the exception of only two, 0 and ∞; and the actual infinitesimal as an extended magnitude, that is one that can be represented by means of a segment, does not exist.[154]

In his review of Bettazzi's monograph referred to above, Vivanti identifies the systems of moments of functions and of infinities of functions studied by Stolz and du Bois-Reymond as non-Archimedean systems of magnitudes and appears to lend his support to them [Vivanti 1891, p. 59]. Nevertheless, on the basis of the main body of the present work it would not have been unreasonable for a reader to wonder if Vivanti had had a change of heart and came to deny the existence of actual infinitesimals *per se*. However, as we alluded to above, this was not the case. Indeed, immediately following his just-quoted conclusions he opens the *Appendix* to the work with the remark:

> However, even if the actual infinitesimal does not exist in the realm of real quantities, as we have already established, this does not preclude that in other mathematical fields it is possible to define entities possessing analogous properties to such infinitesimals. Although such entities have no bearing on our argument, I hope it will not seem entirely inappropriate to say a few words about them.[155]

The systems Vivanti is referring to are primarily those of du Bois-Reymond, Thomae and Stolz, and he devotes the bulk of the Appendix to providing summaries of them. As we suggested above, it is presumably the presence in mathematics of what Vivanti calls "conventional entities" that he believes permits him to embrace the existence of such systems of infinitesimals while denying the existence of infinitesimal line segments. Moreover, he does eventually clarify this point in response to some prodding by Bettazzi, as we shall later see.

From a historical point of view, however, what is far more interesting about Vivanti's Appendix than his embrace and overview of the aforementioned systems is the following passage he uses to introduce them. In it we find one of the earliest recognitions of

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il carattere di elemento, e quindi la sua introduzione non fa che spostare lasciandolo irresoluto, il problema della costituzione del continuo."

[154] [Vivanti 1891a, p. 146]: "1. Il calcolo infinitesimale non ha bisogno che delle sole quantità finite;

2. Il campo d'esistenza d'una variabile reale positiva consta unicamente di quantità finite, ad eccezione di due sole, 0 e ∞, e l'infinitesimo attuale, come grandezza estensiva, cioè rappresentabile mediante un segmento, non esiste."

[155] [Vivanti 1891a, p.146]: "Ma se l'infinitesimo attuale non esiste, come abbiamo ormai stabilito, nel campo delle quantità reali, non è escluso che in altri campi pur essi soggetti al dominio delle Matematiche possano definirsi enti dotati di proprietà analoghe a quelle degl'infinitesimi. Benchè tali enti sieno estranei all'argomento propostoci, non parrà, speriamo, del tutto inopportuno il dirne qualche cosa."

the relative nature of finite, infinite and infinitesimal magnitude. Vivanti expresses the matter thus:

> We have already pointed out what we understand by magnitude in general, and by one-dimensional magnitude in particular; and now let us add that the magnitude:
>
> $$S(A, A, A, \dots, A)$$
> $$\quad 1 \quad 2 \quad 3 \qquad m$$
>
> is called the *m*-multiple of the magnitude *A*. If we proceed to consider more general magnitudes, the previously given definition of *finite*, which is essentially based upon the concept of real quantities, will not have any meaning. Not being able to preserve the absolute meaning of the word *finite*, let us attempt at least to maintain the relative meaning of the three words *finite, infinitesimal* and *infinite*. For this purpose, given a class of one-dimensional magnitudes and having agreed to regard as finite a determinate magnitude *A*, we need to establish a criterion to decide which magnitudes of the class should be called finite, infinitesimal, or infinite *with respect to* *A*: and we should choose such criteria in such a way that the meanings of these words would vary as little as possible from the meanings they have in the theory of ordinary quantities.
>
> Let us call
>
> *finite* with respect to *A* every magnitude *B* such that there are two successive multiples of *A* between which an assigned multiple of *B* is contained;
>
> *infinitesimal* with respect to *A* every magnitude *C* all of whose multiples are smaller than *A*;
>
> *infinite* with respect to *A* every magnitude [*D*] greater than all the multiples of *A*.
>
> It can be easily shown that a magnitude that is infinitesimal (or infinite) with respect to *A* is likewise with respect to any other magnitude that is finite with respect to *A*. Therefore, if in the class in question there actually exist magnitudes of the characters of *B*, *C*, *D*, the class will be divided into three subclasses β, γ, δ containing magnitudes that are finite, infinitesimal and infinite, respectively, [with respect to *A*].[156]

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[156] [Vivanti 1891a, pp. 146–147]: "Abbiamo già accennato che cosa s'intenda per grandezza in generale, e per classe di grandezze ad una dimensione; ed aggiungiamo ora che la grandezza:

$$S(A, A, A, \dots, A)$$
$$\quad 1 \quad 2 \quad 3 \qquad m$$

si dice *m*-upla della grandezza *A*. Se prendiamo a considerare le grandezze più generali, la definizione già data di *finito*, la quale si basa essenzialmente sul concetto reale di quantità, non ha più alcun significato. Non potendo però conservare il valore assoluto della parola *finito*, tentiamo di mantenere almeno il valore relativo delle tre parole *finito*, *infinitesimo*, *infinito*. A tal uopo, essendo data una classe di grandezze ad una dimensione, e convenendo di riguardare come finita una grandezza determinata *A*, occorre stabilire un criterio per decidere quali grandezze della classe debbano *respetto ad A* chiamarsi finite, quali infinitesime, quali infinite: e conviene scegliere tali criteri per modo che il senso di queste parole si scosti il meno possibile da quello che esse hanno nella teoria delle quantità ordinarie.

Chiameremo *finita* rispetto ad *A* ogni grandezza *B* tale che possano trovarsi due multipli successivi di *A* fra cui sia compreso un multiplo assegnato di *B*;

infinitesima ogni grandezza *C* di cui tutti i multipli sono minori di *A*;

infinita ogni grandezza maggiore di tutti i multipli di *A*.

Si dimostra senza alcuna difficoltà, che una grandezza infinitesima (o infinita) rispetto ad *A* lo è parimenti rispetto a qualunque altra grandezza finita rispetto ad *A*. Se dunque esistono effettiv-

Finally, following in the footsteps of Bettazzi, Vivanti goes on to add that depending upon their respective contents, the classes γ and δ might also be so partitioned, and the resulting classes so partitioned, and so on.

> In this way [says Vivanti] infinitesimals and infinities of various orders shall be obtained.[157]

15.   Does the actual infinitesimal exist? The Bettazzi-Vivanti debate II: Bettazzi's response to Vivanti

Bettazzi begins his *Observations on the article of Vivanti* by reiterating Vivanti's findings and by emphasizing that, like Vivanti, he is solely concerned with the "actual infinitesimal" that "has the properties of a fixed quantity." This, says Bettazzi, stands in opposition to the "*potential* [infinitesimal] employed in all the treatises on the calculus, which is defined as a finite, variable quantity approaching zero," a concept which "undoubtedly is very rigorous and evidently sufficient to treat the calculus" [Bettazzi 1891, p. 175].

Unlike Vivanti, however, Bettazzi was well aware of the shortcomings of the earlier attempts, including Vivanti's, to define the actual infinitesimal by means of "division into infinitely many equal parts." As Bettazzi puts it:

> It is preferable to derive the definition of actual infinitesimal from the words of the author [Vivanti] .... "when repeated any finite number of times, it (the infinitesimal) never constitutes ... any finite determined quantity" than from the same author's words ... which are not well defined ... according to which the infinitesimal would be obtained by means of division into infinitely many equal parts, since the word *repeat* is understood in the ordinary manner of multiplication.[158]

"Having defined an actual infinitesimal in such a way," says Bettazzi, "it seems to me that the question posed by the author has already been resolved" [Bettazzi 1891, p. 175]. More specifically, as Bettazzi explains:

> In mathematics it is said that an object exists when its definition does not contradict the definitions and properties of the objects already admitted .... For this type of existence it is not necessary that an object of mathematics should be met in reality, otherwise we would not study, for example, zero, the infinite, spaces of more than three dimensions, non-Euclidean geometry, etc.; it is enough that the already admitted postulates are not contradicted.

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amente nella classe considerata grandezze della natura di B, C, D, la classe resterà divisa in tre sottoclassi β, γ, δ contenenti rispettivamente le grandezze finite, infinitesime e infinite."

[157] [Vivanti 1891a, p. 147]: "Si avranno così infinitesimi ed infiniti di vari ordini."

[158] [Bettazzi 1891, p. 175]: "La definizione dell'infinitesimo attuale, meglio che dalle parole dell'autore ... per le quali l'infinitesimo si otterrebbe dalla divisione in infinite parti uguali, non ben definita, si ha dalle altre dello stesso autore, che fanno seguito a quelle citate: ".... esso (l'infinitesimo), ripetuto un numero finito qualsiasi di volte, non forma giammai ... una quantità finita determinata qualunque" purchè si prenda la parola *ripetere* nell'ordinario significato della moltiplicazione."

Thus, the question posed by the author ... is equivalent to this: "Does the concept of an infinitesimal contradict itself or any other concept of mathematics?" This question has already been answered in a negative fashion by the introduction of examples of classes of objects for which ... the condition called the postulate of Archimedes is not satisfied while all the other ordinary properties of magnitudes are satisfied: and the author himself reports such examples in the Appendix .... Thus we can conclude: "the infinitesimal does exist."[159]

Having established to his satisfaction that the actual infinitesimal is not self-contradictory and therefore exists in this sense and, according to Bettazzi, the only sense that is of relevance to mathematics, Bettazzi adds: "Now we have to examine the importance that such an infinitesimal can have in mathematics." His examination begins with the following prophetic remark.

Its study will certainly have an abstract importance like that of any other existing object, and it will probably render us important service, if nothing else a treatment of the calculus other than the ordinary one ....[160]

"[H]owever," Bettazzi goes on to add,

we should ask ourselves if it [the infinitesimal] would have any value when confronting reality or the usual manner of considering magnitudes.

This is, after all, the question the author [Vivanti] really wants to pose as is apparent from the course of his article. Moreover, it should be observed that if we shall discover that the concept of an infinitesimal contradicts some other concept ... this will not testify against the existence of infinitesimals, but rather against the use of infinitesimals with that concept in the same way that the impossibility of applying complex numbers to continuous segments does not testify against the use of complex numbers in many other parts of mathematics.

The author in reality seeks to find out if the infinitesimal exists in a *particular* domain, that of linear magnitudes, that is the magnitudes corresponding to segments, and as such his question can be formulated in the following way: "Does an actual infinitesimal segment exist?"

In virtue of what has already been said, answering this question consists of deciding whether the concept of an infinitesimal segment agrees with the postulates that are

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[159] [Bettazzi 1891, pp. 175–176]: "In matematica si dice che un ente esiste quando, non contraddicendo esso per la sua definizione alle definizioni ed alle proprietà degli enti già ammessi .... Per tale esistenza dunque non occorre che un ente della matematica abbia riscontro nella realtà, senza di che non si studierebbero, p.es., lo zero, l'infinito, gli spazi a più di tre dimensioni, la geometria non euclidea, ecc.; ma basta che non contraddica i postulati già ammessi e le loro conseguenze.

Allora la domanda dell'autore ... equivale all'altra: "Il concetto di infinitesimo ha contraddizioni in sè o con gli altri concetti generali della matematica?" A questa domanda è stato già risposto negativamente col dare esempi di classi di enti dei quali ... cioè non è soddisfatta la condizione che suol dirsi *Postulato d'Archimede*, mentre lo sono tutte le altre proprietà ordinarie delle grandezze: e l'autore stesso riporta ... i relativi esempi nell'appendice .... Possiamo quindi concludere: "L'infinitesimo attuale esiste."

[160] [Bettazzi 1891, p. 176]: "Importanza astratta avrà certamente il suo studio come quello di qualunque altro ente esistente, e forse esso potrà rendere importanti servigi, se non foss'altro in una trattazione del calcolo diversa dall'ordinaria ...."

employed to define a segment as a magnitude, and thus depends on such postulates. And since the choice of postulates for a segment, as for any other object, is arbitrary and is made only according to the purpose that is put forth in the study of it, we shall therefore have to come to an understanding of which postulates we want to be valid for the segment from the outset.

In this choice we can be guided either by purely theoretical concepts or by the desire to have a segment that will better render to the mind the nature of a straight line and its parts that are employed in common practice. It seems that the author has taken the second approach: as such clearly among the postulates to be posited for a segment is the postulate of Archimedes, which is equivalent to the assertion that the class of segments is connected, ... the upshot of which is, as the author correctly points out, that an actual infinitesimal rectilinear segment does not exist.[161]

On the other hand, as Bettazzi notes, the choice of which postulates to adopt "can be guided ... by purely theoretical concepts" and as such the adoption of the denial the postulate of Archimedes "while not in correspondence to what is met in practice, *at least as far as observation can reach,* would be admissible in theory" [Bettazzi 1891, p. 177]. Still, as Bettazzi goes on to caution, regardless of which approach is adopted, care must be taken in what conclusions about the existence of infinitesimals are actually drawn. Indeed, says Bettazzi:

If we hold that the domain of our observations is too restricted to enable us to judge whether we are closer to reality when we admit that the class of segments is connected

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[161] [Bettazzi 1891, pp. 176–177]: "ma ci si può chiedere se esso avrà valore di fronte alla realtà o al modo consueto di considerare le grandezze.

Questa è del resto la questione che veramente vuol porsi l'autore, come appare dal corso dell'articolo. Occorre per altro osservare che se si troverà che il concetto d'infinitesimo ripugni a qualche altro concetto ... ciò non deporrà contro l'esistenza dell'infinitesimo, ma bensì contro l'uso dell'infinitesimo in quel concetto: così come non si oppone all'uso del numero complesso in tante parti della matematica il fatto che esso non si può applicare al continuo dei segmenti.

L'autore cerca in realtà se l'infinitesimo esiste in uno *speciale* campo, quello delle grandezze lineari, cioè delle grandezze corrispondenti ai segmenti: talchè possiamo così formulare la sua domanda: "Esiste il segmento attuale infinitesimo?"

Tale ricerca consiste, per quanto si è già detto, nel giudicare se il concetto d'infinitesimo va d'accordo o no coi postulati che servono a definire il segmento come grandezza, e quindi dipende da tali postulati. E siccome la scelta dei postulati pel segmento, come per qualunque altro ente, è arbitraria e si fa soltanto a seconda dello scopo che ci proponiamo nello studio di esso, così bisognerà dapprima intendersi sui postulati che vogliamo validi per il segmento.

In questa scelta ci possiamo lasciar guidare o da concetti puramente teorici o dal desiderio di avere un segmento che meglio renda alla mente il fatto di retta e di sue parti che si ha nella pratica comune. In questo secondo modo di vedere sembra che sia l'autore: ed allora, poichè è chiaro che fra i postulati da porsi per il segmento v'è il postulato d'Archimede, il quale equivale all'altro che la classe dei segmenti è connessa, ... si conclude, come giustamente avverte l'autore, che il segmento rettilineo attualmente infinitesimo non esiste."

or when we admit the opposite hypothesis, then the question of the infinitesimal [in the physical world] will remain unresolved ....[162]

Having dealt with Vivanti's principal argument against the existence of infinitesimals, Bettazzi turns to Cantor's argument, an argument he believes "does not have the value of the first" [Bettazzi 1891, p. 177]. While admitting that the argument "is incomplete and ... thus a rigorous conclusion cannot be drawn from it," he nevertheless suggests that it would be useful "to assume that the demonstration has been completed or is easy to complete, and thus that the conclusion of Cantor is proven." That is, we should assume it is proven that: (*)

> if in a class of linear magnitudes there are magnitudes α and β such that α is infinitesimal while β is finite and accordingly $n\alpha < \beta$ for each integer n, then $n\alpha < \beta$ for each transfinite number n as well: a conclusion that, according to Cantor, contradicts his conception of a linear magnitude, and therefore demonstrates the falsity of the first hypothesis $n\alpha < \beta$ [for each integer n] and, thereby, the falsity of the existence of the infinitesimal α.[163]

Having adopted this assumption, says Bettazzi:

> We can then ask ourselves if this contradiction with the concept of linear magnitudes really exists, and if this [i.e., Cantor's] concept of magnitude is the most legitimate one.[164]

"As to the first question," says Bettazzi,

> $n\alpha < \beta$ for each finite or transfinite number n, implies that a magnitude equal to or greater than β can never be composed from magnitudes equal to α whether they are taken a finite or infinite number of times: now, since the concept of linear magnitudes (segments) given by Cantor requires that with a *sufficiently large* number of magnitudes [equal to] α it will be possible to reach or even surpass β, then the contradiction would only exist if the transfinite numbers would exhaust the series of sufficiently large infinite numbers. Can we really say that this is the case? And if this has not been demonstrated, does it not seem that the preceding results instead of containing a contradiction that leads to the negation

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[162] [Bettazzi 1891, p. 177]: "Qualora per altro si ritenga che il campo delle nostre osservazioni sia troppo ristretto per poter giudicare se meglio ci si accosti alla realtà con l'ammettere che la classe dei segmenti sia connessa piuttosto che con l'ipotesi opposta, resterà insoluta la questione dell'infinitesimo ...."

[163] [Bettazzi 1891, p. 178]: "che cioè se in una classe di grandezze lineari si suppone che esistano due grandezze α e β di cui α sia infinitesima di fronte a β finita e quindi sia $n\alpha < \beta$ con qualunque n intero, dev'essere $n\alpha < \beta$ anche se n è un numero qualunque transfinito: conclusione che, secondo il Cantor, ripugna col concetto da lui assunto per le grandezze lineari e che quindi dimostra f'alsa la prima ipotesi $n\alpha < \beta$ perciò falsa l'esistenza dell'infinitesmio α."

[164] [Bettazzi 1891, p. 178]: "Possiamo allora chiederci se realmente questa contraddizione col concetto di grandezza lineare c'è, ed inoltre se questo concetto di grandezza è il più giusto."

of the infinitesimal points to the insufficiency of transfinite numbers because of the ability of α to surpass β?[165]

Indeed, as Bettazzi goes on to add:

> The transfinite numbers are immensely larger than any finite number; but since it is not *demonstrated* that they represent the highest expression of number, and since it is a mere assertion of Cantor (*Acta mathem.*, vol. 2°, page 390) that it is possible to reach all the diverse powers that are encountered in the material and non-material natural world with the help of them, we must only conclude that they are insufficient to render $n\alpha > \beta$, and accordingly we shall need other [numbers] ν of a broader conception for which $\nu\alpha > \beta$ is the case.[166]

That is, according to Bettazzi, if we embrace Cantor's conception of a linear magnitude and assume that (*) has been proven, then:

> If we say for two numbers n and m of any kind that $m < n$ if $m\alpha < n\alpha$, then the numbers [ν] we need [to assure that $\nu\alpha > \beta$] must be larger than all those of Cantor.[167]

Actually, Bettazzi seems to be on shaky ground here, since his argument rests on the assumption that "the concept of linear magnitudes (segments) given by Cantor requires that with a *sufficiently large* number of magnitudes [equal to] α it will be possible to reach or even surpass β." However, what the concept of linear magnitudes given by Cantor actually seems to require is that with a *sufficiently large* number, say η, of magnitudes equal to α, *where η is one of Cantor's ordinals (finite or transfinite)*, it will be possible to reach or even surpass β. Accordingly, if, for the sake of argument, we embrace Cantor's concept of linear magnitudes, then Bettazzi's suggested way out of Cantor's argument does not seem to be available.

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[165] [Bettazzi 1891, p. 178]: "se essendo n un numero qualunque finito o transfinito si ha sempre $n\alpha < \beta$, vuol dire che con grandezze uguali ad α, sia che si prendano un numero finito od un numero transfinito di volte, non si compone mai una grandezza uguale o maggiore di β: ora poichè il concetto di grandezze lineari (segmenti) dato dal Cantor esige che con un numero *sufficiente-mente grande* di grandezze α si possa raggiungere o superare β, la contraddizione si avrebbe solo se i numeri transfiniti esaurissero la serie dei numeri infiniti sufficientemente grandi. Puo veramente dirsi che ciò sia? E se ciò non è dimostrato, non sembra che i risultati precedenti, invece che contenere una contraddizione che condurrebbe alla negazione dell'infinitesimo accenino ad una insufficienza dei numeri transfiniti a potere dalla grandezza α passare a quella β?"

[166] [Bettazzi 1891, p. 178]: "I numeri transfiniti sono immensamente più grandi di qualunque numero finito; ma non essendo *dimostrato* che essi rappresentano la più alta espressione del numero ed essendo, a mio credere, una pura asserzione quella del Cantor (*Acta mathem.*, vol. 2°, pag. 390) che si possa con essi arrivare a tutte le potenze diverse che s'incontrano nella natura materiale ed immateriale, dobbiamo concludere solo che essi sono insufficienti a rendere $n\alpha > \beta$, e che quindi ne occorreranno altri ν di concetto più vasto, in modo che con essi sia $\nu\alpha > \beta$."

[167] [Bettazzi 1891, p. 178]: "Se di due numeri di qualunque specie m, n si dice $m < n$ se $m\alpha < n\alpha$, i numeri che occorrono devono essere maggiori di tutti quelli di Cantor."

Be this as it may, Bettazzi was not prepared to accept Cantor's conception of linear magnitude in any case. Indeed, writes Bettazzi:

> [I]s the concept of linear magnitude or of segment given by Cantor really the most legitimate one or, rather, the most opportune one? Is it required by the concept of magnitude that a magnitude be formed from a determinate finite or infinite number of other magnitudes? Note the indetermination that reigns in the phrase "infinite number of times," a phrase that will be incomplete until *all* the infinite numbers have been subjected to an adequate and rigorous study. It is true that such a study has been done for the whole transfinite numbers; but I already noted how disadvantageous it is to be limited to them since they do not represent the most general conception of infinite number, and, as such, in my opinion, the condition of formation we are talking about seems inopportune.[168]

With this remark the reader is only beginning to get a hint of what is really worrying Bettazzi about Cantor's "condition of formation," a worry that Bettazzi will not clearly state until his second installment in the debate–namely: "if a magnitude cannot be defined as a well-ordered collection which transfinite number will correspond to it?" On the other hand, if one wants to limit oneself to observable phenomena, says Bettazzi, then a satisfactory finitary version of Cantor's "condition of formation" is acceptable. "In fact," says Bettazzi,

> such a condition is put forth by Du Bois-Reymond in his *Allgemeine Functionentheorie* for what he calls linear magnitudes [see Appendix I]. Then, since this condition is the postulate of Archimedes, one immediately has the nonexistence of infinitesimals, and the demonstration of Cantor becomes unnecessary.[169]

Following his discussion of Cantor's argument, Bettazzi turns his attention to the purported proofs for the existence of infinitesimals based on the theories of probability and transfinite numbers we alluded to earlier. Bettazzi not only rejects the arguments but goes on to suggest that actual infinitesimals have nothing to fear from these theories as well. Having done so, he goes on to summarize his views on the existence of an actual infinitesimal segment as follows:

> it seems to me that the question, as it has been posed by me, is well resolved ...: the existence of the actual infinitesimal is a postulate that is not opportune for segments intended for ordinary use.

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[168] [Bettazzi 1891, p. 179]: "il concetto di grandezza lineare o del segmento dato dal Cantor è davvero il più giusto o, meglio, il più opportuno? È necessario nel concetto di grandezza quello di formazione di essa con un determinato numero finito od infinito di altre? Faccio osservare l'indeterminazione che regna nella frase "numero infinito di volte", frase che sarà incompleta finchè *tutti* i numeri infiniti non saranno assoggettati ad uno studio sufficiente e rigoroso. Questo studio è fatto, è vero, per i numeri interi transfiniti; ma ho già notato come non sia conveniente limitarsi ad essi, che non rappresentano il concetto più vasto del numero infinito, e quindi la condizione di formazione di cui si tratta apparisce, a mio credere, inopportuna."

[169] [Bettazzi 1891, p. 179]: "Tale condizione pone infatti il Du Bois-Reymond nella sua *Allgemeine Functionentheorie* per quelle che egli dice quantità lineari. Allora, siccome questa condizione è il postulato d'Archimede, si ha immediatamente che con essa l'infinitesimo non esiste, e la dimostrazione di Cantor diviene inutile."

It seems to me that this is the conclusion the author [Vivanti] wanted to come to even though in some passages it is not so evident: in any case, I find mistaken or at least unclear his assertion that "The actual infinitesimal does not exist in the field of real quantities." In fact, if by the field of real quantities is meant that of ordinary rational or irrational numbers, it should be noted immediately that it satisfies the postulate of Archimedes, and thus among them infinitesimals do not exist. If by the real quantities are meant those quantities representing reality, then the non-existence of infinitesimals among them is, as we have seen, only a consequence of a postulate deduced from observation, and it can be dispensed with whenever we want. There is no doubt that if a segment is to be useful in material practice it must be defined with the postulate of Archimedes, which excludes the infinitesimal; but just as the existence of a plane where there can exist only one parallel line drawn to another line through a given point does not exclude the importance of a geometry similar to that of a pseudo-sphere, where the parallels are infinite, in the same manner the appropriateness of segments without infinitesimals does not exclude the interest of those having them.[170]

Thus Bettazzi, while making no reference to Veronese's forthcoming work on non-Archimedean geometry, was willing to countenance the mathematical possibility and interest of such a geometry. Following this prophetic remark, Bettazzi closes his paper by listing "the conclusions that seem ... to be rigorously drawn," namely:

1° The existence of the actual infinitesimal does not imply contradictions, and accordingly the actual infinitesimal does exist.
2° It is not convenient to admit the existence of an actual infinitesimal segment if we *only* want to create a geometry representing the most common facts of our ordinary limited observations.
3° Nothing can be deduced *against* the actual infinitesimal from either the theory of probability or transfinite numbers.
4° The infinitesimal calculus does not need actual infinitesimals but will be able to use them when the study of them is completed; then it will only be left to decide whether

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[170] [Bettazzi 1891, pp. 181–182]: "la questione, come da me è stata posta, mi sembra ben risoluta ...: l'esistenza dell'infinitesimo attuale è un postulato, il quale non è per altro opportuno per i segmenti destinati all'uso ordinario.

Questa è la conclusione a cui appare voglia giungere l'autore, sebbene in qualche frase ciò non sia così evidente: comunque trovo non corretta, o almeno non chiara la sua proposizione (Appendice): "L'infinitesimo attuale non esiste nel campo delle quantità reali". Infatti, se per campo delle quantità reali s'intende quello dei numeri ordinari razionali ed irrazionali, per essi è noto immediatamente che soddisfano al postulato d'Archimede, e che quindi fra essi non esistono infinitesimi: se s'intende per quantità reali quelle che sono destinate a rappresentare la realtà, il non esistere fra esse l'infinitesimo è solo effetto, come si è visto, di un postulato desunto dall'osservazione, e che può sopprimersi quando si voglia. Nessun dubbio che il segmento, per essere utile nella pratica materiale, debba essere definito col postulato d'Archimede, che esclude l'infinitesimo; ma come l'esistenza di un piano in cui di rette parallele condotte da un punto ad una retta non ve n'è che una (il che corrisponde alla pratica dentro i limiti delle nostre osservazioni) non esclude l'importanza di una geometria simile a quella della pseudosfera, dove le parallele sono infinite, così l'opportunità dei segmenti senza infinitesimi non esclude l'interesse di quelli che ne posseggono."

it is better to develop it with their use or with the use of the potential infinitesimals that are usually employed.[171]

16. Does the actual infinitesimal exist? The Bettazzi-Vivanti debate III: Vivanti's response to Bettazzi

Whether Peano had intended Vivanti to write a response to Bettazzi's *Observations* is not clear. However, whatever the original intention may have been, Vivanti makes it clear in the opening remarks of his second installment that he felt the need to clarify his views. He writes:

> The *Observations* of ... Professor R. Bettazzi ... have convinced me that the fear of being too prolix made me appear obscure in some fundamental points of the discussion. And in fact if not the author, then the readers of the *Observations* suggest that I have been preoccupied with *a problem which is not really a problem* and which reduces to nothing more than a play on words or a question of expediency.[172]

To help counter this perception Vivanti offers the following explanation of why he interpreted the question of the existence of infinitesimals in the manner he did.

> The human sciences in order to study natural phenomena considered it necessary to decompose them into parts as small as possible .... It is easy to see how this idea ... gave rise to the concept of an actual infinitesimal considered as an actually existing quantity....
>   The quantities that have figured exclusively in analysis, at least until modern times when the breeze of generalization started to blow upon all the fields of mathematics, are those which constitute the substratum of natural phenomena, that is durations, lengths, velocities, temperatures, etc. Thus it is only within the field of these quantities that the question [of the existence of infinitesimals] I just mentioned must be raised; and to bring it outside this field would distort it. But these quantities have a common character that is reducible to a single type arbitrarily chosen among them; thus for example the class of linear segments can be taken. That is why the question can be limited to this class

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[171] [Bettazzi 1891, p. 182]:
"1° L'esistenza dell'infinitesimo attuale non involge contraddizione, e quindi l'infinitesimo attuale esiste.
2° Non è conveniente ammettere l'esistenza di un segmento infinitesimo attuale, *solamente* se si vuole una geometria che rappresenti i fatti più comuni delle nostre ordinarie limitate osservazioni.
3° Non può dedursi niènte *contro* l'infinitesimo attuale dalla teoria delle probabilità e dei numeri transfiniti.
4° Il calcolo infinitesimale non abbisogna degli infinitesimi attuali, ma potrà usarli quando ne sia stato fatto completo lo studio, restando poi da decidersi se meglio si svolga con essi o cogli infinitesimi potenziali di cui si serve ordinariamente."
[172] [Vivanti 1891b, p. 248]: "Le *Osservazioni* del ... Prof. R. Bettazzi ... mi hanno convinto che il timore di essere troppo prolisso m'ha fatto riuscire oscuro in alcuni punti capitali della trattazione. Ed invero, se pure già non era nell'autore, s'ingenera almeno nei lettori delle *Osservazioni* l'idea, aver io trattato *un problema che non è problema*, e che si riduce a poco più d'un giuoco di parole o d'una questione d'opportunità."

of magnitudes without detriment to generalization. Therefore the question of my first article: "whether an actual infinitesimal segment exists" is nothing else but a translation into a simpler form of the age-old and, I would say, *classical* question of the existence of infinitely small constant quantities.[173]

Vivanti's words lend credence to Bettazzi's suspicion that when in the main body of Vivanti's earlier work Vivanti argued against the existence of actual infinitesimals he was primarily concerned with "those [quantities] which constitute the substratum of natural phenomena." "After all," as Vivanti later adds:

> it is natural that, when the question [of the existence of infinitesimals] is understood in all its generality, one must answer it affirmatively; and in order to clarify this I referred to a few examples of infinitesimal magnitudes in §12.[174]

Having made clear the origins and importance of the question of the existence of an actual infinitesimal line segment, says Vivanti, "I will examine its true nature" [Vivanti 1891b, p. 251]. Interestingly, according to Vivanti, this is to be found in the philosophy of the continuum. Indeed, while admitting that both he and Bettazzi agree on the definition of the continuum, it is with respect the philosophical underpinnings of the concept that he finds real divergence and, ultimately, the source of fault in Bettazzi's view. He begins by expressing the matter as follows in terms of a class of line segments that is continuous in the sense of Bettazzi:

> Essentially, according to Bettazzi, the continuity of the class *A* (and consequently the validity of the postulate of Archmedes) is the result of experience, which, as such, can be contradicted tomorrow by more accurate observations.
>
> Is this way of seeing the matter correct?
>
> The question, though unaltered, will be better seen in its true light if for a standard of a quantity we shall take time rather than the straight line .... There is a fact about which there can be no disagreement, and it is that the continuity of time is not based upon experience,

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[173] [Vivanti 1891b, pp. 248–249]: "La scienza umana, per sottoporre a misura ed a studio i fenomeni naturali, ha ritenuto necessario scomporli in fasi quanto è possibile piccole .... È facile comprendere come questa idea ... dato origine al concetto dell'infinitesimo attuale considerato come quantità effettivamente esistente ....

Le quantità che hanno figurato esclusivamente nell'analisi, prima almeno che la moderna aura generalizzatrice venisse a soffiare su tutti i campi delle matematiche, sono quelle che costituiscono il substrato dei fenomeni naturali, cioè tempi, lunghezze, velocità, temperature, ecc. Quindi è soltanto entro il campo di queste quantità che si è agitata e deve tuttora agitarsi la questione a cui ho testè accennato; portarla fuori di esso sarebbe volerla snaturare. Ma quelle quantità hanno il carattere comune di potere essere ridotte ad un unico tipo scelto ad arbitrio fra esse; come tale può prendersi p. es. la classe dei segmenti rettilinei. Ecco perchè la questione può, senza danno della generalità, limitarsi a questa classe di grandezze. Pertanto il problema fondamentale del mio primo articolo: "Cercare se possa darsi un segmento attualmente infinitesimo" non è che una traduzione sotto forma più semplice della questione secolare e, direi, *classica* della esistenza di quantità infinitamente piccole costanti."

[174] [Vivanti 1891b, p. 253]; "Del resto è naturale che, presa la domanda in tutta la sua generalità, si debba rispondere ad essa affermativamente; ed appunto per mettere in chiaro ciò io ho recato nel §12 alcuni esempi di grandezze infinitesime."

and cannot be so, because the idea of time itself does not result from observations, but rather is the condition necessary for the possibility of any empirical observation.[175]

Appended at this point in the passage is a note that reads: "Here the theory of space and time of Kant comes to mind ...." Having so informed the reader Vivanti continues:

> In other words, it is not our *limited observations* that tell us that time is continuous; it is our mind that refuses to conceive of it in any other way, it is our mind which is not capable of conceiving of time passing from one instant to another instant without passing through all the instants in between. And the same can be said about lengths, velocities, temperatures, etc. I would like to add that the question directs us to the *conceivability* of the infinitesimal rather than to the existence of it in the field of ordinary quantities. And the question is quickly resolved: since continuity excludes the existence of the infinitesimal, and because it is the fundamental characteristic of quantities . . . actual infinitesimal durations, lengths, temperatures, etc., are absurd, inconceivable entities (*impossible, i.e., contradictory in thought,* as Cantor says). As long as the human mind remains the same, this conclusion cannot change; there is nothing to hope for or to fear of in the future from more perfect observations.
>
> This is not true for the parallel postulate, which, instead, is based upon experience; and thus the comparison between the two cases will not stand. And it is possible to cite as a distinctive fact, that Helmholtz, one of the strongest supporters of the empirical origins of the geometrical axioms, repeatedly declares that his ideas do not oppose the Kantian conceptions of space and time.[176]

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[175] [Vivanti 1891b, p. 252]: "In sostanza, secondo il Bettazzi, la continuità della classe *A* (e quindi la validità nella stessa del postulato d'Archimede) è un risultato dell'esperienza, che, come tale, può domani venire contraddetto da osservazioni più accurate.

É giusto questo modo di vedere?

La questione, pur restando inalterata, apparirà meglio nella sua vera luce se come tipo delle quantità di cui nel prenderemo, non più la linea, ma il tempo .... V'ha un fatto su cui non può cadere disaccordo, ed è, che la continuità del tempo non è fondata sull'esperienza, nè potrebbe esserlo, giacchè l'idea stressa di tempo non risulta dalla osservazione, ma è bensì una delle idee che costituiscono le condizioni necessarie per la possibilità di qualsiasi osservazione empirica."

[176] [Vivanti 1891b, pp. 252–253]: "In altre parole, non sono già le nostre *limitate osservazioni* che ci dicono il tempo essere continuo; e la nostra mente che si rifiuta a concepirlo in modo diverso, che non è capace di seguire il tempo che scorre da uno ad un altro istante senza passare per tutti gli istanti intermedi. E ciò che si dice del tempo può ripetersi delle lunghezze, velocità, temperature, ecc. Ben dissi dunque sopra che la questione, meglio che sulla esistenza, versa sulla *concepibilità* dell'infinitesimo, nel campo delle quantità ordinarie. E la questione è ben presto risolta: poichè la continuità esclude l'esistenza dell'infinitesimo, e poichè essa è il carattere fondamentale delle quantità . . . i tempi, le lunghezze, le temperature, ecc., attualmente infinitesime, sono enti assurdi, inconcepibili *(unmögliche, d. h. in sich widersprechende Gedankendinge*, come dice Cantor). Fin che la mente umana resta la stessa, questa conclusione non può mutare; nulla v'ha a sperare nè a temere da future e più perfette osservazioni.

Così non è del postulato delle parallele, il quale invece si fonda realmente sull'esperienza; epperò il paragone fra i due casi non regge. E si può citare come fatto caratteristico, che Helmholtz, uno dei più caldi sostenitori dell'origine empirica degli assiomi geometrici, dichiara ripetutamente le sue idee non essere punto in opposizione col concetto kantiano di tempo e spazio."

Turning his attention to the first part of Bettazzi's critique of Cantor's purported proof, "a proof, which, to tell you the truth, should not be discussed since it is incomplete" [Vivanti 1891b, p. 253], Vivanti writes:

> First of all, according to Bettazzi, it is not proven that transfinite numbers are the highest expression of number, and it is a pure assumption of Cantor's that it is possible to reach the diverse powers encountered in the material and nonmaterial natural world with their aid.– To prove that this is not the case it might be enough to recall what a transfinite number is. A transfinite number is nothing more than a concept obtained from a well-ordered collection by abstracting from the peculiar nature of its elements in such a way that to every well-ordered collection a transfinite number *ipso facto* corresponds. Now, in our case, we have to deal precisely with a well-ordered collection, that is, with a series of infinitesimal segments, all of them equal, placed end to end upon a straight line; thus however much the series of segments be extended, there is always a transfinite number that represents it.[177, 178]

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[177] [Vivanti 1891b, pp. 253–254]: "Anzitutto, secondo il Bettazzi, non è provato che i numeri trasfiniti sono la più alta espressione del numero, ed è una pura asserzione quella di Cantor, che si possa con essi arrivare a tutte le potenze diverse che s'incontrano nella natura materiale ed immateriale.–Per mostrare che ciò non è esatto, basta rammentare che cosa sia un numero trasfinito. Un numero trasfinito non è altro che il concetto che si ottiene da un insieme ben ordinato facendo astrazione dalla natura speciale dei suoi elementi; per modo che a qualunque insieme ben ordinato corrisponde *ipso facto* un numero trasfìnito. Ora nel caso nostro abbiamo a fare appunto con un insieme ben ordinato, e cioè con una serie di segmenti infinitesimi tutti eguali disposti l'uno di seguito all'altro sopra una linea retta; quindi, per quanto sia estesa la serie dei segmenti, sempre v'ha un numero trasfinito che la rappresenta."

[178] Although Vivanti would have had no way of knowing it, he is not on firm *twentieth-century* mathematical ground when he contends: "to every well-ordered collection a [Cantorian] transfinite number *ipso facto* corresponds." It is of course true that to every *well-ordered set of Zermelo-Fraenkel set theory (ZF)*, a Cantorian ordinal corresponds. However, there are set theories such as those due to von Neumann-Bernays-Gödel, Morse, and Ackermann that countenance well-ordered proper classes (in the set-theoretic sense–*not Bettazzi's sense*) that correspond to no set of *ZF* and, hence, to no Cantorian ordinal. The best known example of such a class is, of course, the well-ordered class, On, of all Cantorian ordinals–a class that is itself an "ordinal," albeit not a Cantorian ordinal. In these theories, however, one can introduce even greater "ordinals" such as $On + 1, On + 2, On + 3, \ldots$ although in the theories of von Neumann-Bernays-Gödel and Morse, where a proper class may not be a member of a class, well-known special techniques must be employed in defining such entities (cf. [Ehrlich 2001, p. 1233]). In Ackermann's theory, on the other hand, where proper classes may be members of proper classes, these "ordinals" may be defined using a straightforward generalization of the standard von Neumann ordinal construction. Thus, while Bettazzi certainly did not anticipate anything like the theoretical frameworks that would lend credence to such numbers, it is not misguided, as Vivanti contends, to envision the possibility of numbers that correspond to well-ordered collections that are greater than each of Cantor's ordinals. For good overviews of the theories of von Neumann-Bernays-Gödel, Morse, and Ackermann, see [Fraenkel, Bar-Hilllel, Levy 1973, pp. 119–153].

"The second remark of Bettazzi," says Vivanti,

concerns the concept of linear magnitude adduced by Cantor; the concept that, according to him, is neither the most legitimate nor the most opportune. Let us carefully examine this concept, and see upon what it is based.

As I have mentioned earlier, a linear segment could be imagined to be *composed* either of points or of infinitesimal segments. The characteristic that distinguishes these two types of elements is that a point does not have any dimensions, while a segment has a length. In other words: we cannot understand how a point can be placed next to another point without the two coinciding, while, on the contrary, the juxtaposition of two segments allows the formation of another segment, composed of the two though different than each. In my opinion, this is what Cantor means when he says that according to the concept of a linear magnitude any such magnitude must be regarded as an integral part of another analogous one. And the demonstration of Cantor is reduced to showing that an infinitesimal segment, if it existed, would not have this characteristic property, since nothing would distinguish it from an unextended point.[179]

17. Does the actual infinitesimal exist? The Bettazzi-Vivanti debate IV: Bettazzi's closing remarks

Bettazzi begins his second and final response by remarking that:

I state with pleasure that a complete accord reigns between us [Vivanti and himself] in regard to the principal questions: $1°$ The concept of an infinitesimal is not contradictory, as there are (proper) classes of magnitudes (of the 2nd kind) in which for two opportune magnitudes A, B, we have $nA < B$ for any integer n, and for which therefore A is said to be an (actual) infinitesimal with respect to B. $2°$ In the classes of segments intended for the study of natural phenomena the infinitesimal segment is to be excluded.

I note that the author does not approve of my remarks regarding the second question, where I say that the actual infinitesimal segment does not exist by reason of analogy with what we encounter in practice *within the limits of our observations*; he maintains it is *necessary* in virtue of the very nature of our minds that which I consider merely an *opportune* representation of reality based upon necessarily limited observations, namely,

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[179] [Vivanti 1891b, p. 254]: "Il secondo appunto del Bettazzi riguarda il concetto di grandezza lineare adottato da Cantor; concetto che, secondo lui, non è nè il più giusto nè il più opportuno. Esaminiamo bene questo concetto, e vediamo su che esso si fondi.

Come già dissi, un segmento lineare può immaginarsi come *composto*, o di punti, o di segmenti infinitesimi. Il carattere che distingue tra loro tali due specie di elementi è questo, che il punto non ha dimensioni, mentre il segmento ha una lunghezza. In altre parole: noi non sappiamo concepire che si possa porre un punto immediatamente accanto ad un altro senza che i due coincidano, mentre al contrario la giustapposizione di più segmenti dà luogo alla formazione di un segmento composto dei medesimi e diverso da ciascuno di essi. Ecco che cosa significa, a mio credere, l'espressione di Cantor, che secondo il concetto di grandezza lineare ciascuna di tali grandezze deve immaginarsi come parte integrante di altre analoghe. E la dimostrazione di Cantor si riduce a far vedere che il segmento infinitesimo, se esistesse, sarebbe privo di questa proprietà caratteristica, sicchè nulla più lo distinguerebbe dal punto inesteso."

the continuity of the class of the ordinary segments, which excludes the existence of the
infinitesimal. Thus the question enters the field of philosophy, where I did not intend to
lead it; being quite ignorant of the relevant studies, I do not intend to open a discussion
in that field. I will limit myself to saying that the strictly mathematical question to which
I intended to restrict myself is exhausted; as long as the straight line is defined and the
segments are defined as its parts retaining for themselves the usual concepts of greater,
smaller, sum, and difference, the choice between the two postulates is *logically* free ….[180]

Turning next to Vivanti's reaction to his critique of Cantor's "proof," Bettazzi goes
on to write:

I called *unproven* that the transfinite numbers of Cantor are the highest expression of
number and Dr. Vivanti judges me to be mistaken, because, as he writes "A transfinite num-
ber is nothing more than a concept obtained from a well-ordered collection by abstracting
from the peculiar nature of its elements in such a way that to every well-ordered collection
a transfinite number *ipso facto* corresponds." But I ask, if a magnitude cannot be defined
as a well-ordered collection which transfinite number will correspond to it? Are all mag-
nitudes to be considered well-ordered collections? Since the latter is not in general the
case, given the common concept of magnitude, the insufficiency of the transfinite number
as a concept corresponding to the magnitudes seems clear to me, unless they are either
limited to special classes or something more is added to the generally accepted conception
of them.

The author abides by this way of thinking for the case of segments for the remainder of
the article and continues: "Now, in our case, we have to deal precisely with a well-ordered
collection, that is, with a series of infinitesimal segments, all of them equal, placed end to
end upon a straight line." It seems clear to me that a new and independent hypothesis is
stated here, and not a truth necessitated by the concept of an infinitesimal segment. The
common magnitudes are composed of magnitudes which follow one another, but they are
finite, that is to say of the same nature of the magnitudes they constitute; on the other
hand, it seems to me that nothing in observation or logic teaches us that a magnitude must
be conceived as a collection composed of other magnitudes which are not of its same

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[180] [Bettazzi 1892, pp. 38–39]: "e constato con piacere come regni fra noi due l'accordo comp-
leto sulle questioni principali–1° Il concetto d'infinitesimo attuale non è contraddittorio, in quanto
esistono classi (proprie) di grandezze nelle quali per due grandezze opportune A, B si ha $nA < B$
qualunque sia n intero, e quindi A è da dirsi un infinitesimo (attuale) rispetto a B (classi di 2^a
specie). 2° Nella classe dei segmenti destinati allo studio dei fenomeni naturali è da escludersi il
segmento infinitesimo.

Noto peraltro che l'autore non approva i miei argomenti relativi alla 2^a questione, là dove
io dico che il segmento attualmente infinitesimo non esiste per ragione di analogia con quello
che si riscontra in pratica *dentro i limiti delle nostre osservazioni*; ritenendo egli esser *necessario*
per la natura stessa della nostra mente il fatto che io ho detto essere soltanto *opportuno* come
rappresentazione della realtà basata su osservazioni necessariamente limitate, vale a dire la con-
tinuità della classe degli ordinari segmenti, che esclude l'esistenza dell'infinitesimo. Essendo la
questione entrata così nel campo filosofico, dove io non volevo condurla perchè affatto profano
ai relativi studi, non intendo aprire la discussione in quell'indirizzo. Mi limito ad osservare che la
questione strettamente matematica alla quale intendevo arrestarmi può dirsi esaurita; inquantochè
definita la retta, e definiti i segmenti come sue parti, ritenendo per essi i consueti concetti di uguale,
maggiore, minore, somma e differenza è *logicamente* libera la scelta fra i due postulati …."

nature. And if there is no well-ordered collection of infinitesimal segments out of which a finite segment is composed, then the support that the author seeks for Cantor's proof of the non-existence of infinitesimals is missing.[181]

Although Bettazzi did not say as much, he undoubtedly was aware that he could have illustrated his first point by appropriately choosing an element from any of the discrete non-Archimedean classes of units that he investigated in his *Teoria Delle Grandezze* [1890, §129; 1893, pp. 119–123]. Consider, for example, the element $1 \cdot 1_1 = 1_1$ in the subclass A of $B(2)$ consisting of all elements of the form $\alpha^{(0)}1_0 + \alpha^{(1)}1_1$, where $\alpha^{(0)}$ is a positive integer and $\alpha^{(1)}$ is an integer. With the addition and order inherited from $B(2)$, A is a limited proper class (in Bettazzi's sense); moreover, the ordered set of elements of A less than or equal to 1_1 is given by

$$1_0, 21_0, \ldots, n1_0, \ldots \quad \ldots, 1_1 - n1_0, \ldots, 1_1 - 21_0, 1_1 - 1_0, 1_1$$

But this ordered set has order type $\omega +^* \omega$, which is not the order type of a well-ordered set.

On the other hand, what Bettazzi plainly did not anticipate is that among the properties of the non-degenerate line segments of the various non-Archimedean geometrical spaces that would soon be developed by such writers as Veronese [1891], Levi-Civita [1892–93], Hilbert [1899], Dehn [1900], Schur [1899; 1902; 1903; 1905; 1909], Hessenberg [1905; 1905a] and Hjelmslev [1907], is that every such segment *can* be decomposed into a discrete ordered set[182] $\langle D_s, <_{D_s} \rangle$ of non-degenerate congruent subsegments that

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[181] [Bettazzi 1892, pp. 39–40]: "Io dissi *non esser provato* che il numero transfinito del Cantor sia la più alta espressione del numero: e il Dr Vivanti mi giudica in errore, giacchè, come egli scrive, "Un numero transfinito non è altro che il concetto che si ottiene da un insieme ben ordinato, facendo astrazione dalla natura speciale dei suoi elementi: per modo che a qualunque insieme bene ordinato corrisponde *ipso facto* un numero transfinito". Ma, domando, e se una grandezza non si può definire come un insieme bene ordinato, quale numero transfinito le corrisponderà? O piuttosto tutte le grandezze sono da ritenersi quali insieme bene ordinati? Siccome quest'ultima cosa non è da affermarsi in generale, dato il concetto comune di grandezza, così mi pare chiara la insufficienza del numero transfinito come concetto corrispondente alle grandezze, a meno che o ci si limiti a loro classi speciali, o si aggiunga qualcosa di più all'idea che ordinariamente si ha di esse.

A quest'ultimo modo si attiene del resto per il caso dei segmenti l'autore, il quale così prosegue: "Ora nel caso nostro abbiamo a fare appunto con un insieme bene ordinato, e cioè con una serie di segmenti infinitesimi tutti eguali, disposti l'uno di seguito all'altro sopra una linea retta". Mi pare chiaro che qui si enuncia una ipotesi nuova ed indipendente, e non un fatto necessariamente incluso nel concetto di segmento infinitesimo. Le grandezze comuni sono composte di parti che si seguono, ma quando queste sono finite, cioè della stessa natura delle grandezze che esse ricompongono; nulla invece mi pare che l'osservazione o la logica c'insegnino circa il modo in cui si deve concepire una grandezza come insieme di altre che non sono della sua stessa natura. E se manca questa disposizione dei segmenti infinitesimi che fa del segmento finito un insieme bene ordinato, manca l'appoggio che per il suo assunto l'autore chiede alla dimostrazione del Cantor circa la non esistenza dell'infinitesimo."

[182] An ordered class is said to be *discrete* if every member, unless it be the first, has an immediate predecessor and every member, unless it be the last, has an immediate successor.

are infinitesimal relative to S where D_s has a first member and a last member and $<_{D_s}$ is defined by the condition: $S' <_{D_s} S''$ if and only if no point of S' succeeds (in the ordering of points on the line) any point of S''.[183] Since the set of points on each of the lines of these spaces is dense, it follows from the above that in these decompositions the last point of each subsegment S' with the exception of the last point of S itself coincides with the first point of the subsegment that is the immediate successor of S'. That the said segments admit such a decomposition follows from an elementary result about ordered Abelian groups (see Note 184) and the fact that the set of points on a line in each of the aforementioned spaces together with order and addition suitably defined either constitutes an ordered Abelian group in which for each strictly positive element a there is a strictly positive element b that is infinitesimal relative a, or, as in case of an elliptic space, is suitably related to such a structure.[184] It is worth noting, however, that the only *general* proofs of the existence of these decompositions that we are aware of require the Axiom of Choice (or some equivalent thereof), an assumption that, as we have already mentioned, Bettazzi was not prepared to accept. However, what role, if any, the possible dependence of the general existence of these decompositions on the Axiom of Choice

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[183] In a space such as an elliptic space where there is no global total order on the points of a line the order is defined locally on the segment itself.

[184] To be a bit more precise, the set of points on a line in each of the aforementioned spaces together with order, addition, and multiplication suitably defined either constitutes a non-Archimedean ordered field, or, as in case of an elliptic space, is suitably related to such a field. However, the ordered additive structure of every non-Archimedean ordered field is an ordered Abelian group in which for each strictly positive element a there is a strictly positive element b that is infinitesimal relative a.

The existence of the aforementioned decomposition, however, depends solely on the ordered additive structure. To state the theorem in question, we require the following definition in which if $[a, b]$ and $[c, d]$ are nondegenerate closed intervals of an ordered Abelian group G, then $[a, b]$ is said to be *as long as* (*smaller than*) $[c, d]$, if $b - a = d - c (b - a < d - c)$. If I and I' are nondegenerate closed intervals of an ordered Abelian group $\langle G, +, > \rangle$ and I' is smaller than I, then I will be said to have an I' - *covering* if I can be decomposed into (i.e., is the union of) an ordered class $\langle C_{I'}, <_{C_{I'}} \rangle$ of non-degenerate closed intervals having the following properties: i. $\langle C_{I'}, <_{C_{I'}} \rangle$ is a discrete ordered class (see Note 182) having a first member and a last member whose order is defined by the condition for all $X, Y \in C_{I'}$, $X <_{C_{I'}} Y$ if and only if no member of X is $>$ any member of Y; ii. each interval in $C_{I'}$ is as long as I'.

Theorem *If I and I' are nondegenerate closed intervals of an ordered Abelian group and I' is infinitesimal relative to I, then there is an I'- covering of I. Any such I'- covering of I is not well ordered.*

A proof of the above theorem was given by the author in paper entitled "Zeno's Paradox of Extension and its Solution" that was presented at the Pacific Meeting of the American Philosophical Association in Long Beach, California in 1984. A revised version of the paper is being prepared for publication.

A closely related result, whose proof is attributed to R. Hartshorne, is presented in [Moore 2002, p. 317: Proposition 30].

played in Bettazzi's thinking about the matter we are in no position to say. On the other hand, what does perhaps reflect favorably on Bettazzi's observations is that the ordered sets of segments associated with these decompositions are *never* well-ordered sets.

18. A prelude to Veronese's non-Archimedean geometry and Peano's reaction thereto

Appended to the end of Bettazzi's second installment of his debate with Vivanti is an editorial footnote by Peano that reads: "In an upcoming issue I will develop Cantor's proof of the impossibility of a constant infinitesimal line segment" [Bettazzi 1892, p. 40]. The development appeared soon thereafter in Peano's "*Dimostrazione dell'impossibilità di segmenti infinitesimi costanti*" (Demonstration of the Impossibility of Constant Infinitesimal Segments) [1892]. In this paper–contrary to what is sometimes suggested (cf. [M. Klein 1980, p. 274])–Peano does not argue against the possibility of infinitesimals *per se* but rather against the possibility of infinitesimal line segments and other "commonplace magnitudes" more generally. Indeed, as is evident from the opening remarks of Peano's paper, unlike Cantor he embraces the idea that the order of infinity of one real function can be infinitesimal with respect to the order of infinity of another such function. In particular, he writes

> We will say that a magnitude u is infinitesimal with respect to a magnitude v if every finite whole numerical multiple of u is less than v. The existence or nonexistence of infinitesimal magnitudes depends upon the meaning we attribute to the word *magnitude*. And there have actually been formed categories of entities in which it is possible to define the relationships and operations similar to those performed in algebra with numbers, and in these categories of entities one can find infinitesimals. Thus the order of infinity of one function can be infinitesimal with respect to the order of infinity of another function. In my work [*Sulla formula di Taylor*, 1891] I have already shown that in one and the same Taylor formula we can choose to consider the successive terms either as variable infinitesimals or as constant infinitesimals of different orders.[185]

Having established that he accepts the existence of at least some types of infinitesimals he goes on to add:

> In all these cases the entity is determined by a real function of a real variable. But do infinitesimals exist among the commonplace magnitudes, for example, among the segments of a straight line?

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[185] [1892, p. 58; Opere Scelte III, p. 110]: "Si dice che una grandezza u è infinitesima rispetto alla grandezza v, se ogni multiplo di u, secondo un numero intero finito, è minore di v. L'esistenza o meno di grandezze infinitesime dipende dal significato che attribuiamo alla parola *grandezza*. Ed effettivamente si sono formate delle categorie di enti, sui quali si possono definire le relazioni e operazioni analoghe a quelle dell'algebra sui numeri, nelle quali categorie di enti si trovano degli infinitesimi. Così l'ordine di infinità d'una funzione può essere infinitesimo rispetto all'ordine di infinità d'un'altra. In un mio scritto [*Sulla formula di Taylor*, 1891] già feci vedere che nella stessa formula di Taylor i successivi termini si possono considerare a nostro arbitrio come infinitesimi variabili o costanti d'ordine diverso."

   This question, discussed by Doctors Vivanti and Bettazzi in the *Rivista di Matematica*, is very interesting, and more so because recently there have appeared theories and printed volumes based upon the hypothesis of their existence. Cantor has responded negatively to this question; however, the proof given by this illustrious mathematician against the theory is so concise that it has been considered incomplete. The purpose of this note is to develop this proof.[186]

Foremost among the theories of infinitesimals Peano is referring to is that of Giuseppe Veronese developed in his *Fondamenti di Geometria* [1891], a work to which Peano gave a scathing review [Peano 1892a]. Peano's review appears in the same volume of *Rivista di Matematica* containing his development of Cantor's "proof." Peano's purported proof can be viewed as much as an attack on Verosene's pioneering investigation of non-Archimedean geometry as it can as a development of Cantor's abstract argument. Indeed, his "proof" together with his aforementioned review of Veronese's *Fondamenti* may be regarded as the opening salvo in the multifaceted critique of non-Archimedean geometry and/or Veronese's contributions thereto that ensued following the publication of Veronese's path-breaking work. This being the case, we will defer our discussion of Peano's purported proof to the aforementioned companion to the present work where the emergence of non-Archimedean geometry and the reaction thereto will be the central focus of the discussion.

Appendix I. Du Bois-Reymond's conception of a *linear magnitude* and its relation to Stolz's axiomatiztion of an Archimedean system of absolute magnitudes

   As we mentioned in Note 14, Stolz apparently drew some inspiration for his axiomatiztion of an Archimedean system of absolute magnitudes from du Bois-Reymond's conception of a *linear magnitude* [1882, pp. 43–48] (a conception that should not be confused with Cantor's namesake conception discussed in Sections 6, 7 and 8). According to du Bois-Reymond, the concept of a linear magnitude is grounded in our idea of a straight line. Indeed, says du Bois-Reymond:

> They can essentially be regarded as lines; their differences, their parts and their multiples are also magnitudes of the same kind, as are lines; they are capable of being very small and very large, as are lines, and, like lines, they are comparable and measurable.[187]

In the section of his work entitled "A More Precise Definition of Linear Magnitude" du Bois-Reymond characterizes such systems in terms of the following six assertions

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[186] [1892, p. 58; Opere Scelte III, p. 110]: "In tutti questi casi l'ente è determinato da una funzione reale di una variabile reale. Ma fra le grandezze comuni, p. e. fra i segmenti rettilinei, esistono degli infinitesimi?

   Questa questione, dibattutasi fra i dott. Vivanti e Bettazzi sulla *Rivista di Matematica*, è assai interessante tanto più che negli ultimi tempi sul'ipotesi della loro esistenza si sono fatte teorie e stampati dei volumi. Ad essa rispose negativamente il Cantor; ma la dimostrazione che questo illustre matematico ne diede è così concisa, che fu giudicata incompleta. Scopo della presente nota si è di sviluppare questa dimostrazione."

[187] [1882, p. 23] "Sie sind auf Längen zurückführbar, ihre Unterschiede, Theile und Vielfache sind wie bei den Längen wieder Grössen derselben Art, sie sind wie Längen in der Richtung des Kleinsten und Grössten ausgedehnt, sie sind wie Längen vergleichbar, messbar."

[1882, pp. 44–47].

   I. The linear mathematical magnitudes are either equal or unequal. They are equal if their sensual manifestations produce the same impression under the same conditions. One is larger than the other, if its appearance can be altered through exhaustion so that it completely coincides with the other, and the contrary is impossible.

  II. Among the linear magnitudes of a kind, e.g. all possible segments, none is distinguished, and consequently we have no conception of a necessary limit either for the smallness or for the greatness of a [linear] magnitude.

 III. If two or more magnitudes of the same kind are combined it results again in a magnitude of the same kind, which is larger than the components. On the other hand, each magnitude can be divided into arbitrarily many others of the same kind, and each such part is smaller than the undivided magnitude.

 IV. If a magnitude is larger than another, then there is always a third of the same kind as the two that combined with the second results in the first.

  V. One can always combine equal or unequal magnitudes, whose smallest need not be smaller than an arbitrarily small given magnitude, in sufficient number, to obtain a magnitude that is not smaller than any given one of the same kind.

 VI. A magnitude is divisible into smaller magnitudes in innumerable ways, the divisions being distinguished in that the magnitude is divided into two, three or more mutually equal magnitudes. The division of a magnitude can be continued till all the parts become smaller than an arbitrarily small given magnitude of the same kind. However, no matter how far the division is carried out the parts are always magnitudes of the same kind.[188]

However, while du Bois-Reymond's conception of a linear magnitude may be regarded as a forerunner of Stolz's concept of an Archimedean system of absolute magni-

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[188] "I. Die linearen mathematischen Grössen sind entweder gleich oder ungleich. Gleich sind sie, wenn ihre sinnlichen Erscheinungen unter denselben Bedingungen denselben Eindruck hervorbringen. Die eine ist grösser wie die andere, wenn ihr sinnliches Bild durch Exhaustion so abgeändert werden kann, dass es das der anderen vollständig enthält, und das umgekehrte Verhalten unmöglich ist.

II. Keine besondere unter den linearen Grössen einer Art, z.B. keine besondere Strecke unter allen möglichen Strecken besitzt an sich einen Vorzug, und wir haben mithin keine Vorstellung einer nothwendigen Schranke weder für die Kleinheit noch für die Grossheit einer Grösse.

III. Zwei oder mehrere Grössen derselben Art zusammengefügt ergeben wiederum eine Grösse derselben Art, die grösser ist als ihre Bestandtheile. Andererseits kann jede Grösse in beliebig viele andere derselben Art getheilt werden, und jeder Theil ist kleiner als die ungetheilte Grösse.

IV. Wenn eine Grösse grösser ist als eine andere zweite, so giebt es stets eine dritte von derselben Art, wie jene beiden, mit welcher vereinigt die zweite die erste ergiebt.

V. Man kann stets gleiche oder ungleiche Grössen, deren kleinste nicht unter eine beliebig klein anzunehmende Grösse fallen soll, in genügender Anzahl zusammenfügen, um eine Grösse zu erhalten, die nicht kleiner ist, als irgend eine vorgelegte derselben Art.

VI. Eine Grösse ist auf unzählige Weise in kleinere theilbar, unter welchen theilungen sich die auszeichnet, bei der sie in zwei, drei oder mehr untereinander gleiche Grössen zerfällt. Die Theilung einer Grösse kann so lange fortgesetzt werden, bis die Theile sämmtlich kleiner werden als eine beliebig klein anzunehmende Grösse derselben Art. Wie weit getrieben die Theilung aber auch gedacht werden mag, stets sind die Theile Grössen derselben Art."

Appendix II (see Note 57). A critique of Laugwitz's assessment of Cantor's argument against the possibility of infinitesimals of the form $\frac{1}{\omega}$ contained in Cantor's letter to Kerry of February 4, 1887

In [Laugwitz 2002], immediately following his summary of the just-cited argument (see Section 6), Laugwitz writes:

> This is a nice argument, and one wonders why Cantor did not repeat it in letters to mathematicians. He communicated the result without proof in a letter to Weierstrass on May 16, 1887, ... which is also mentioned in a footnote of his *Mitteilungen* ....
>
> Perhaps Cantor saw that transfinite numbers were not at all essential for his argument. In modern terms, he considered an ordered semigroup extension of the additive semigroup of non-negative real numbers which had the least upper bound property. The subset of infinitely small elements had to have a least upper bound ξ, which must either be infinitesimal or not. In the first case $\xi + \xi$ was infinitesimal, so ξ was not an upper bound. In the second case, a real positive $p < \xi$ existed, so ξ was not the least upper bound.
>
> It follows that the result was not an application of Cantor's transfinite numbers, a fact which he might have found hard to admit. [Laugwitz 2002, pp. 113–114]

As we mentioned above (see Note 57), [Laugwitz 2002] was published posthumously. Accordingly, one does not know if upon further reflection Laugwitz would have revised the above remarks. Unfortunately, however, as they stand they are historically and mathematically misleading.

To begin with, as we point out in Section 8, the result that Cantor communicated to Weierstrass and which is referred to in a footnote of his *Mitteilungen* is not the "result" about $\frac{1}{\omega}$ from his letter to Kerry but rather his more general "result," a "result" which, we might add, is not discussed in Laugwitz's paper. Moreover, immediately following his purported proof-sketch of the latter, Cantor writes: "It seems to me that this is an important application of the theory of transfinite numbers .... I also don't believe that this result can be reached fully and strictly in any other way" [1887 in 1932, p. 408]. Given this, Laugwitz's contention that "[p]erhaps Cantor saw that transfinite numbers were not at all essential for his argument" seems doubtful. In any case, as we alluded to above, the argument that Laugwitz suggests Cantor might have been aware of is flawed–albeit only slightly.

Indeed, contrary to the contention that is implicit in Laugwitz's remark, *there are ordered semigroup extensions of $\mathbb{R}^+$ having the least upper bound property that contain elements that are infinitesimal relative to the members of $\mathbb{R}^+$*. One can state the matter in terms of the following

Theorem *There is a positively ordered semigroup extension of $\mathbb{R}^+$ without a zero that has the least upper bound property and contains elements that are infinitesimal relative to the members of $\mathbb{R}^+$; moreover, if Γ is such an extension of $\mathbb{R}^+$, then Γ contains a maximal infinitesimal μ (which is, of necessity, an idempotent element, i.e., $\mu +_\Gamma \mu = \mu$).*

Proof. An ordered semigroup Γ is positively ordered if $a + b \geq a, b$ for all $a, b \in \Gamma$. Plainly then, if Γ contains a maximal infinitesimal μ, then $\mu +_\Gamma \mu = \mu$. To establish the non-trivial portion of the theorem, consider, for example, the structure $\langle A, +_A, <_A \rangle$

whose universe, A, is the set of all a such that either $a = r$ or $a = r\omega^{-1}$ for some $r \in R^+$ or $a = \mu$, and where $<_A$ and $+_A$ are defined as follows where $<_{\mathbb{R}^+}$ and $+_{\mathbb{R}^+}$ are the standard order and addition in $\mathbb{R}^+$, and r and r' are arbitrary members of $\mathbb{R}^+$: $r\omega^{-1} <_A r'\omega^{-1}$ if $r <_{\mathbb{R}^+} r'$; $r\omega^{-1} <_A \mu$; $r\omega^{-1} <_A r'$; $\mu <_A r$; $r <_A r'$ if $r <_{\mathbb{R}^+} r'$; $a +_A b = r +_{\mathbb{R}^+} r'$ if $a = r$ and $b = r'$; $a +_A b = b +_A a = r$ if $a = r$ and $b = \mu$ or $b = r'\omega^{-1}$; $a +_A b = b +_A a = \mu$ if $a = \mu$ and $b \leq \mu$; $a +_A b = (r +_{\mathbb{R}^+} r')\omega^{-1}$ if $a = r\omega^{-1}$ and $b = r'\omega^{-1}$. The reader can readily verify that $\langle A, +_A, <_A \rangle$ is a positively ordered semigroup extension of $\langle \mathbb{R}^+, +_{\mathbb{R}^+}, <_{\mathbb{R}^+} \rangle$ without a zero consisting of a continuous set $\{r\omega^{-1} : r \in \mathbb{R}^+\}$ of infinitesimal elements followed by the single infinitesimal element μ followed by the continuous set of elements of $\mathbb{R}^+$. To see that $\langle A, +_A, <_A \rangle$ is continuous (and thus has the least upper bound property) simply note that the order type of $\langle A, <_A \rangle$ is $\theta + 1 + \theta$ where θ is the order type of $\mathbb{R}^+$.

In virtue of the above, it is not difficult to identify the error in Laugwitz's argument and to see how to overcome it. Laugwitz assumes that if ξ is an infinitesimal (which is tacitly assumed to be greater than 0), then $\xi + \xi > \xi$. However, if we assume that the semigroup is positively ordered (i.e., $a + b \geq a, b$), then Laugwitz's assumption will hold if and only if ξ is not an idempotent element. Of course, if we assume that the semigroup is strictly positively ordered (i.e., $a + b > a, b$), then ξ cannot be an idempotent element and Laugwitz's argument will go through. On the other hand, one does not need the full strength of the latter assumption to make Laugwitz's argument work. Indeed, the very nature of Cantor's argument not only presupposes that if $\frac{1}{\omega}$ is a linear number then it is not idempotent, but the stronger assertion that $\frac{1}{\omega}$ has *infinite order*, i.e., the assertion that $\frac{1}{\omega} < \frac{1}{\omega} + \frac{1}{\omega} < \frac{1}{\omega} + \frac{1}{\omega} + \frac{1}{\omega} < \ldots$ All the elements of a positively ordered semigroup A have infinite order if and only if A is *idempotent-free* (i.e., A has no idempotent elements) if and only if $\xi + \xi > \xi$ for each $\xi \in A$ [cf. Clifford 1958, pp. 306–308; Clifford and Preston 1961, pp. 19–20]. Although Cantor never explicitly says as much, we strongly suspect that Cantor always tacitly supposed that $\xi + \xi > \xi$ is a property of an arbitrary linear number ξ.

Appendix III (see Notes 71, 73, 74 and 76). Parenthetical observations related to Cantor's argument against the possibility of infinitesimals

Parenthetical Observation I. In Note 71 we observed that for Cantor writing in 1887, it is by no means evident that the phrase "*numbers which may be regarded as bounded, continuous lengths of straight lines*" carries with it either the implications of strict positivity, cancellativity or left-solvabiltiy. On the other hand, since Cantor had hoped to show that the Archimedean axiom is implied by the concept of a linear magnitude, it is perhaps worth adding that the following result, which is an immediate consequence of work of Clifford [1954b; also see Fuchs 1963, pp. 163–165: Lemma C and Theorem 2] and Satyanarayana and Nagore [1979; also see Satyanarayana 1979, p. 57; and Behrend 1956], suffices to show that the three just-cited properties are consequences of the Archimedean axiom together with properties specified in LO (or, even, L0*) along with L1* (see Section 8).

Theorem *Every Archimedean, right-solvable, positively ordered semigroup without a least element is cancellative, strictly positive and solvable. (See Note 7 for definitions).*

This result, which is a strengthening of a classical result of Hölder [1901], is a special case of a more general theorem that is formulated using a version of the Archimedean axiom that is appropriate for positively ordered (as opposed to merely strictly positively ordered) structures.

Parenthetical Observation II. In Note 73 we noted that there are continuous proper extensions of $\langle \mathbb{R}^+, +_{\mathbb{R}^+}, <_{\mathbb{R}^+} \rangle$ that satisfy L0 together with the Archimedean axiom. We now establish this using a variation on the structure $\langle A, +_A, <_A \rangle$ introduced in Appendix II.

Let A_I be the set of infinitesimals from the just-mentioned structure $\langle A, +_A, <_A \rangle$; that is, $a \in A_I$ if and only if either $a = r\omega^{-1}$ for some $r \in \mathbb{R}^+$ or $a = \mu$. Also let $+_{A_I}$ and $<_{A_I}$ be the addition and order in $\langle A, +_A, <_A \rangle$ restricted to the members of A_I, i.e., for all $r, r' \in \mathbb{R}^+$, let $r\omega^{-1} <_{A_I} r'\omega^{-1}$ if $r <_{\mathbb{R}^+} r'$; $r\omega^{-1} <_{A_I} \mu$; $a +_A b = b +_A a = \mu$ if $a = \mu$ and $b \in A_I$; $a +_A b = (r +_{\mathbb{R}^+} r')\omega^{-1}$ if $a = r\omega^{-1}$ and $b = r'\omega^{-1}$. Finally, let $\langle B, +_B, <_B \rangle$ be the structure whose universe, B, consists of all $r \in \mathbb{R}^+$ together with all formal sums of the form $r + a$ where $r \in \mathbb{R}^+$ and $a \in A_I$, and whose order and addition are defined by the following conditions where, for the sake of convenience, a real number $r \in B$ is written "$r + 0$": $r + a <_B r' + a'$ if $r <_{\mathbb{R}^+} r'$ or $r = r'$ and either $a <_{A_I} a'$ or, $a = 0$ and $a' \in A_I$; $r + a +_B r' + a' = (r +_{\mathbb{R}^+} r') + \bar{a}$, where $\bar{a} = a +_{A_I} a'$ if $a, a' \in A_I$, $\bar{a} = a$ if $a' = 0$, and $\bar{a} = a'$ if $a = 0$.

To see that $\langle B, <_B \rangle$ is continuous one need only note that $\langle B, <_B \rangle$ arises from $\langle \mathbb{R}^+, <_{\mathbb{R}^+} \rangle$ by replacing each $r \in \mathbb{R}^+$ by a closed continuous interval–the closed continuous interval whose first member is r, whose last member is $r + \mu$, and whose intermediate members are the elements of B of the form $r + r'\omega^{-1}$ where $r' \in R^+$.

Parenthetical Observation III. In Note 74 we remarked that L0*, L1, L1* (see Section 8) and the absence of non-zero infinitesimal members of L collectively suffice to show that $\langle L, +, < \rangle$ coincides with $\langle \mathbb{R}^+, +_{\mathbb{R}^+}, <_{\mathbb{R}^+} \rangle$. This is a consequence of the following result and the elementary observation that L0*, L1 and the absence of non-zero infinitesimal members of L jointly imply the Archimedean nature of $\langle L, +, < \rangle$.

Theorem *If $\langle L, +, < \rangle$ is an Archimedean right-naturally ordered semigroup without a least element, then $\langle L, +, < \rangle$ can be embedded in $\langle \mathbb{R}^+, +_{\mathbb{R}^+}, <_{\mathbb{R}^+} \rangle$ in precisely one way that maps a given member of L to a given member of R^+. Accordingly, if $\langle \mathbb{R}^+, +_{\mathbb{R}^+}, <_{\mathbb{R}^+} \rangle \subseteq \langle L, +, < \rangle$, the embedding is a surjection and, as such, $\langle L, +, < \rangle = \langle \mathbb{R}^+, +_{\mathbb{R}^+}, <_{\mathbb{R}^+} \rangle$.*

Historical Proof-Sketch. Hölder [1901] established the first and non-trivial portion of the above theorem for the case where $\langle L, +, < \rangle$ is strictly positive and naturally ordered. Behrend [1956, p. 340: Theorem 1] improved upon Hölder's result by showing that naturally ordered could be replaced by right-naturally ordered albeit with strict positivity retained. Finally, as a result of the theorem collectively due to Clifford, Satyanarayana and Nagore mentioned in Parenthetical Observation I, it is now known that naturally ordered and strict positivity can be replaced by right-naturally ordered alone.

Parenthetical Observation IV. In Note 76 we remarked that if L2 is interpreted as L2$_c$, then L3, L3* and L3** are equivalent. This is a consequence of the following elementary theorem and the fact that L2$_c$ implies:

(**L2***) For all linear numbers ζ, all ordinal numbers α and all positive integers n, the n-fold sum of $\zeta \cdot \alpha$ is given by $\zeta \cdot (\alpha \cdot_c n)$ where $\cdot_c$ is the familiar Cantorian product of ordinals.

Theorem *If the system $\langle L, +, < \rangle$ of non-zero linear numbers satisfies L0*, and there is a multiplication on L that satisfies L2 and L2*, then L3, L3* and L3** are equivalent.*

Proof. Since it is evident that L3* implies L3 which in turn implies L3**, it only remains to show L3** implies L3*. Accordingly, suppose L3** is the case and let $\zeta \in L$. Then $\zeta \cdot \alpha = r'$ for some finite $r' \in L$ and some ordinal α. If $r < \zeta \cdot \alpha$, then by letting $\alpha = \eta$ we are finished. On the other hand, if $\zeta \cdot \alpha \leq r$ then since $\zeta \cdot \alpha$ and r are both finite, the n-fold sum of $\zeta \cdot \alpha$ is greater than r for some positive integer n, and so by L2*, $\zeta \cdot \eta > r$ where $\eta = \alpha \cdot_c n$, thereby proving L3** implies L3*.

Appendix IV. Text of Cantor's "proof" of the impossibility of infinitesimals contained in [Cantor 1887] and based on the text of Cantor's letter to F. Goldscheider of May 13, 1887

Sie erwähnen in Ihrem Schreiben die Frage über die aktual *unendlich kleinen* Größen. An mehreren Stellen meiner Arbeiten werden Sie die Ansicht ausgesprochen finden, daß dies *unmögliche*, d. h. *in sich widersprechende* Gedankendinge sind, und ich habe schon in meinem Schriftchen "Grundlagen e. allg. Mannigfaltigkeitslehre" pag. 8 im § 4, wenn auch damals noch mit einer gewissen Reserve, angedeutet, daß die strenge Begründung dieser Position aus der Theorie der transfiniten Zahlen herzuleiten wäre. Erst in diesem Winter fand sich die Zeit dazu, meine diesen Gegenstand betreffenden Ideen in die Gestalt eines förmlichen Beweises zu bringen. Es handelt sich um den Satz:

"*Von Null verschiedene lineare Zahlgrößen* ζ (*d.h. kurz gesagt, solche Zahlgrößen, welche sich unter dem Bilde begrenzter geradliniger stetiger Strecken vorstellen lassen), welche kleiner wären als jede noch so kleine endliche Zahlgröße, gibt es nicht*, d. h. *sie widersprechen dem Begriff der linearen Zahlgröße.*" Der Gedankengang meines Beweises ist einfach folgender: ich gehe von der *Voraussetzung* einer linearen Größe ζ aus, die so klein sei, daß ihr n-faches

$$\zeta \cdot n$$

für *jede noch so große endliche ganze Zahl n* kleiner ist als die Einheit, und beweise nun aus dem Begriff der linearen Größe und mit Hilfe gewisser Sätze der transfiniten Zahlenlehre, daß alsdann auch

$$\zeta \cdot \nu$$

kleiner ist als *jede noch so kleine endliche Größe*, wenn ν irgendeine noch so große *transfinite* Ordnungszahl (d. h. Anzahl oder Typus einer wohlgeordneten Menge) aus irgendeiner noch so hohen Zahlenklasse bedeutet. Dies heißt aber doch, daß ζ *auch durch*

keine noch so kräftige actual unendliche Vervielfachung *endlich* gemacht werden, also sicherlich nicht *Element* endlicher Größen sein kann. Somit widerspricht die gemachte Voraussetzung dem Begriff linearer Größen, welcher derartig ist, daß nach ihm jede lineare Größe als integrierender Teil von anderen, im besonderen von endlichen linearen Größen gedacht werden muß. Es bleibt also nichts übrig, als die Voraussetzung fallen zu lassen, wonach es eine Größe ζ gäbe, die für jede endliche ganze Zahl *n* kleiner wäre als $1/n$, und hiermit ist unser Satz bewiesen.

Es scheint mir dies eine wichtige Anwendung der transfiniten Zahlenlehre zu sein, ein Resultat, welches alte, weit verbreitete und tiefwurzelnde Vorurteile zu beseitigen geeignet ist.

Die Tatsache der aktual-unendlich großen Zahlen ist also so wenig ein Grund für die Existenz aktual-unendlich kleiner Größen, daß vielmehr gerade mit Hilfe der ersteren die Unmöglichkeit der letzteren bewiesen wird.

Ich glaube auch nicht, daß man dieses Resultat auf anderem Wege *voll* und *streng* zu erreichen imstande ist.

Das *Bedürfnis* unseres Satzes ist besonders einleuchtend gegenüber neueren Versuchen von O. Stolz und P. Dubois-Reymond, welche darauf ausgehen, die Berechtigung aktual-unendlich kleiner Größen aus dem sogenannten "Archimedischen Axiom" abzuleiten. (Vgl. 0. Stolz, Math. Annalen Bd. 18, S. 269; ferner seine Aufsätze in den Berichten des naturw. medizin. Vereines in Innsbruck, Jahrgänge 1881—82 und 1884; sie sind betitelt: "Zur Geometrie der Alten, insbesondere über ein Axiom des Archimedes" und "Die unendlich kleinen Größen"; endlich vergleiche man desselben Autors: "Vorlesungen über allgemeine Arithmetik", Leipzig 1885, I.Teil, S. 205.)

Archimedes scheint nämlich zuerst darauf aufmerksam geworden zu sein, daß der in Euklids Elementen gebrauchte Satz, wonach aus jeder noch so kleinen begrenzten geradlinigen Strecke durch *endliche*, *hinreichend große* Vervielfachung *beliebig große endliche* Strecken erzeugt werden können, eines Beweises bedürftig sei, und er glaubte darum diesen Satz als "Annahme" *(λαμβανόμενον)* bezeichnen zu sollen.

(Vgl. Eukl. *Elem.* lib. V, def. 4: λόγον ἔχειν πρὸς ἄλληλα μεγέ[θ]η λέγεται, ἃ δύναται πολλαπλασιαζόμενα ἀλλ[ή]λων ὑπερέχειν; ferner insbesondere Elem. lib. X, pr. l. Archimedes: de sphaera et cylindro I, postul. 5 und die Vorrede zu seiner Schrift: de quadratura parabolae.)

Nun ist der Gedankengang jener Autoren (0. Stolz a. a. 0.) der, daß wenn man dieses vermeintliche "Axiom" fallen ließe, daraus ein Recht auf aktual unendlich-kleine Größen, welche dort "Momente" genannt werden, hervorgehen würde. Aber aus dem oben von mir angeführten Satze folgt, wenn er auf geradlinige stetige Strecken angewandt wird, unmittelbar die *Notwendigkeit der Euklidischen Annahme.* Also ist das sogenannte "Archimedische Axiom" *gar kein Axiom*, *sondern ein*, *aus dem linearen Größenbegriff mit logischem Zwang folgender Satz.* [1887 in 1932, pp. 407–409]

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