Traditional Logic and the Early History of Sets, 1854-1908[a]

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JOSÉ FERREIRÓS

Contents

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a This article develops ideas taken from my dissertation, 'El nacimiento de la teoría de conjuntos, 1854-1908' (Universidad Autónoma de Madrid, 1991), written under the direction of Javier ORDÓÑEZ. The paper has profited from comments on earlier versions made by Ralf HAUBRICH, Gregory H. MOORE, Jeremy GRAY, and Volker PECKHAUS. Also useful were the comments after a presentation of the main ideas before the Logic Colloquium at Berkeley, particularly those of Charles CHIHARA. The English was revised and corrected by John W. ADDISON.

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I. INTRODUCTION

A long-standing tradition considers Georg CANTOR (1845-1918) as the 'creator' or founder of set theory.[1] This view, correct when restricted to *transfinite* set theory, stands in need of revision within the context of a general account of the emergence of sets in mathematics. Such an account should face the broad questions: how did sets become important in the different branches of mathematics? how did they emerge as basic mathematical objects, as a fundamental language for mathematics?[2] Simultaneously, one should try to establish how and why some mathematicians began to think of sets as the foundation for mathematics. In fact, within the context of the early history of sets during the second half of the nineteenth century, it is convenient to differentiate three aspects of our picture of set theory: the language of sets, the theory of sets, and the idea that sets form the foundation of mathematics. These aspects are so intertwined today that it may seem artificial to distinguish them, but in my opinion the distinction is useful, and even necessary, if we want to clarify the history of sets. During the second half of the nineteenth century, the interaction between the three aspects was quite complex.

Along these lines, a discussion of the work of Bernhard RIEMANN (1826-66) and Richard DEDEKIND (1831-1916) forms the core of this paper. In my view, both mathematicians offer the most clear examples of the use of set language in mathematics, and the elaboration of foundational views based on the notion of set, prior to the emergence of CANTOR's work. This is why they should figure prominently in a general account of the emergence of sets. The triad RIEMANN/DEDEKIND/CANTOR constitutes a quite closed net offering basic clues for a satisfactory explanation of the ascent of sets. (In a detailed description, subsidiary authors, like GAUSS, DIRICHLET, WEIERSTRASS, and HANKEL, would also be of great importance.) RIEMANN, DEDEKIND, and CANTOR give the main examples of set-theoretical and set-

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1 For early and authoritative instances, cf. the dedication of HAUSDORFF *1914*, ZERMELO's words in the preface to CANTOR *1932*, and several passages of HILBERT *1930*. But see ZERMELO's crucial paper on the axiomatization of set theory, where he said that it had been "created by CANTOR and DEDEKIND" (ZERMELO *1908a*, 200).

2 Around 1870, the language of sets was being applied in geometry, elementary arithmetic, algebra, and even complex function theory. During the last two decades, much has been done to consider these developments, at least for the cases of algebra and real analysis. For the emergence of point-sets within real analysis cf. HAWKINS *1975*, DAUBEN *1979*, COOKE *1992*. DEDEKIND's structural view of algebra is studied in DUGAC *1976* and *1981*, EDWARDS *1980*, SCHARLAU *1981* and *1982*, EDWARDS, NEUMANN & PURKERT *1982*. A wealth of material on both topics can be found in MOORE *1982*, and an attempt to synthesize the foregoing studies in FERREIRÓS *1993a*.

Viewed in the light of such previous work, the present paper offers additional material by showing that the notion of set had also geometrical and function-theoretical origins in the work of RIEMANN.

foundational conceptions within 19th century German mathematics.

The mention of RIEMANN in this context is bound to cause some surprise, since it has escaped the attention of historians that he proposed a set-foundation for mathematics in his famous 1854 lecture on geometry. It will be my purpose to establish this point, but also to analyze the origins of this early proposal and its influence on other mathematicians. RIEMANN's influence on DEDEKIND is highly probable, since they were intimate friends from around 1855, i.e., from the time when RIEMANN had just developed his new conception. This influence was mainly on the level of general views—the nature of mathematical objects and questions of methodology—and it is related to a rather abstract conception of mathematics. DEDEKIND followed his own path through algebra and number theory on his way to set theory.[3]

As for traditional logic, it is constantly intertwined with our story. RIEMANN's general notion of a manifold was stated in implicit reference to common logical ideas of his time. Here lies the main difficulty for an interpretation of his definition of a manifold (as will be apparent later, when we consider how two important commentators have confronted RIEMANN's explanation). The difficulties encountered by commentators simply mirror the conceptual gap that separates traditional logic from contemporary mathematical logic. The dramatic changes which logic underwent in the period from 1850 to our time were mainly due to two processes: the emergence of first-order logic motivated by metalogical studies, and the set-theoretical antinomies. As we will see later on, it is the question of the antinomies which turns out to be crucial for our purposes.

These changes in logical views are also the reason why DEDEKIND's logicism has never been taken seriously. This has not only affected the interpretation of the work of this important mathematician, but also our general understanding of the rise and fall of logicism. In my view, a correct interpretation of DEDEKIND's view will pave the way for a reassessment of the history of logicism as an influential nineteenth-century movement. The most important outcome, in this respect, is that the antinomies distorted the traditional view of logic in a way that, despite RUSSELL's efforts, made the logicist enterprise fail.

The mention of logic and set theory in the nineteenth century brings to mind the British algebra of logic, since it was above all an algebra of classes or sets. This suggests the question whether the British mathematical logicians that worked during the 1840s-60s (particularly BOOLE) may have had an influence on RIEMANN or DEDEKIND. This question, however, will not be

3 The development of DEDEKIND's conception can be traced back to the late 1850s, as was shown by DUGAC in a pioneering and very useful book (DUGAC *1976*, cf. also his *1981*). But DEDEKIND's views still require further discussion, particularly as regards his systematic conception of the foundations of mathematics, and his logicism. Moreover, there are points of detail in which my account differs from DUGAC's, the most important being the evolution of DEDEKIND's notion of mapping.

discussed below, since I have found absolutely no evidence in favor of such a connection.[4] Thus, it seems that the development of set theory was for the most part independent from that of logic until the 1890s. The similarities that can be found between them can be explained in terms of the same traditional conception of logic that we have to invoke in order to understand RIEMANN and DEDEKIND. This conception was widely held throughout the 19th century, and it should be considered a major source of the 'classical logic' of our century.

To end this introduction, I would like to comment on questions which are mainly of a terminological character. The study of the origins of set theory in Germany is complicated by the fact that the German language did not suggest a privileged term for naming the notion of set. For example, both DEDEKIND and CANTOR accepted the French translation 'ensemble', while they used different German words: 'System' in the case of DEDEKIND, 'Mannigfaltigkeit' [manifold] or 'Menge' in that of CANTOR. Following CANTOR's usage in the 1890s, German has finally adopted the word 'Menge' to design sets; in the beginning, this word seems to have been considered somewhat unsatisfactory because it suggested a rather rough meaning, like 'mass'. It was also usual to speak about 'Inbegriffe', a compound from 'Begriff' [concept]; the reason for this will become clear in section II, but the fact is that apparently CANTOR and DEDEKIND were also dissatisfied with that word. Some other terms used during the 19th century are 'Klasse', 'Gesamtheit' [totality], 'Vielheit' [multiplicity], 'Gebiet' [domain], even 'Schaar' [host].

In my translations I will follow present-day usage, employing 'set' *only* as a translation of 'Menge', but the reader should keep in mind that other words, like 'system', 'class', 'collection', 'totality', or 'multitude', were no less appropriate for referring to the notion of set. Whether or not a particular word should be interpreted as denoting this notion is to be judged from the context.

This terminological question becomes particularly critical when we come to the word 'Mannigfaltigkeit', on which the whole question of RIEMANN's place in the history of sets depends. In the context of his famous lecture on geometry, RIEMANN proposed his notion of a 'Mannigfaltigkeit' as the basis for topology and differential geometry; in this respect, the word has the same meaning as present-day 'manifold'. Surprisingly, we can also find the word 'Mannigfaltigkeit' in the work of CANTOR, where it undoubtedly means 'set'. I will defend the claim that there is a direct connection between both uses of the word: RIEMANN's 'manifolds' were nothing but sets, as is shown by his theory of 'discrete' and 'continuous' manifolds, and by his

4 Or, for that matter, in favor of connecting DEDEKIND or RIEMANN with the German and French authors who had contributed previously to the theory of classes (cf. KNEALE & KNEALE *1962*, STYAZHKIN *1969*).

definition of a manifold. Therefore, throughout this paper I will translate 'Mannigfaltigkeit' into 'manifold', but I will interpret this term as a synonym for 'set'.

II. TRADITIONAL LOGIC, CONCEPTS, AND SETS

The situation in logic around 1850 constitutes a crucial background for the purposes of this article, not only in connection with RIEMANN, but even more so for the case of DEDEKIND. Unfortunately, the attempt to identify the sources of their knowledge of logic confronts difficulties, since there is no direct evidence about it. Moreover, the German nineteenth century was surprisingly prolific in the production of logic treatises, and the views exposed in them were exceptionally varied (see UEBERWEG *1882*, 47-79). It suffices to recall the influence exerted by HEGEL who, convinced that "everything rational is real, and everything real is rational", identified logic with metaphysics.

In the case of RIEMANN, however, it is natural to assume that he followed the logical doctrines of the man whom he regarded as his master in philosophy: Johann Friedrich HERBART (1776-1841).[5] Notably, HERBART was one of the most outstanding defenders of a strictly *formal* conception of logic against the abuses of the idealists.[6] That is, HERBART championed the rejection of Hegelian logic and the return to Aristotelian syllogistics, understood as a purely formal doctrine. In fact, internal evidence from RIEMANN's and particularly DEDEKIND's writings suggests that they paid attention almost exclusively to logicians that followed the traditional Aristotelian doctrines.

These logicians presented in their works a core of knowledge the roots of which lie in Antiquity, and which was also incorporated by all other non-idealist logicians (sometimes embedded into psychological, epistemological or metaphysical doctrines). The existence of such a core enables us to produce a schematic portrait of the basic logical doctrine of the time. This proves sufficient for our purposes and makes the question of exact sources irrelevant.

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5 On RIEMANN and HERBART see the interesting paper SCHOLZ *1982a*, although it does not consider HERBART's logical views, nor their connection with RIEMANN's manifolds, which concerns us here.

6 Such emphasis on the formal character of logic, and on its treatment as an autonomous discipline, comes from KANT (cf. UEBERWEG *1882*, 47-51, especially on 48-49); nevertheless, scholars debate whether KANT really kept formal logic separate from the transcendental logic that he developed in KANT *1787*. Be it as it may, HERBART rejected KANT's transcendental logic and went as far as possible in isolating the pure or formal content of logic (o.c., 51-53). In fact, UEBERWEG distinguishes a Kantian and a Herbartian school (o.c., v, 52).

However, it is worth mentioning that one of the most widely read logic treatises of the early nineteenth century was written by a Herbartian, the Leipzig professor of mathematics Moritz Wilhelm DROBISCH (1802-96). His work (DROBISCH *1836*) saw four editions between 1836 and 1875, being "acknowledgedly" the best exposition of logic from the formal viewpoint (UEBERWEG *1882*, 53). Therefore, it is perhaps the most natural candidate for having been read by RIEMANN and DEDEKIND. Interestingly, the first edition of DROBISCH's treatise included an appendix containing an attempt to establish a mathematical calculus of logic (see STYAZHKIN *1969*).

2.1. The doctrine of concepts.

In traditional logic, and even philosophy in general, concepts occupied a very central position, being considered as the nuclear constituents of knowledge. The origins of this conception are almost as old as philosophy: PLATO's dialogues used to discuss some series of related words in order to enable a contemplation of the general concept (idea) behind them. In the same spirit, ARISTOTLE considered concepts as the true object of knowledge, for "there is no science but of universals, as is clear from demonstrations and definitions" (*Metaphysics*, 1086b 32). Demonstrations and definitions were for ARISTOTLE the topic of the discipline for whose creation he is usually credited, namely logic. Below we will indeed see how the Aristotelian doctrine of syllogism shows the centrality of concepts to logic.[7]

It is important to realize that Occidental philosophy used to accept the primacy of concepts or ideas;[8] HERBART and his disciple RIEMANN accepted it too. In 1837, HERBART wrote: "today [...] all sciences have long assumed the logical ordering, and so have recognized the dominion of general concepts in all matters of thought" (HERBART *1837*, 68). Already in 1807, he had recommended that every mature science should be established around a central general concept (SCHOLZ *1982a*, 424-426). Later on, RIEMANN would write: "*natural science* is the attempt to comprehend Nature through precise *concepts*" (RIEMANN *1892*, 521), which implies that concepts are the essential building stones of human knowledge.

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7 After having written this section, I noticed that the primacy of concepts in traditional logic was emphasized in CASSIRER *1910*, 4.

8 Even the classical British empiricists considered ideas as the core elements of knowledge, although they emphasized their empirical origins. But we are considering the work of German mathematicians, and the German tradition was particularly close to the Greeks in this respect. Perhaps it is no coincidence that set theory was born in the land of rationalism, and precisely during the 19th century, when a close relation reigned between scientists and philosophers.

2.1.1. Syllogistic logic is based on that same centrality of concepts. As is well known, traditional logicians considered any reasoning to be reducible to a chain of syllogisms. The typical example of a syllogism is: 'Every A is B; and every B is C; therefore, every A is C'. Here we can observe how a syllogism consists of a structured connection of *judgements*, i.e., propositions affirmed or denied (like 'Every A is B'). Propositions, in their turn, are formed by means of the copula 'is', which connects a subject and a predicate. Finally, subject and predicate were considered to be nothing but *concepts*. Following this picture, the part devoted to pure logic in logic treatises of the 19th century was normally divided into three sections: 'On Concepts', 'On Judgements', and 'On Conclusions' (which include syllogisms).

Reasoning was thus regarded as the result of different kinds of formal combinations of concepts, by means of particles like 'every', 'some', 'is', and 'not'. According to KANT and his followers, concepts contained the whole matter or content of an argument, everything else being purely *formal* or logical. It should be mentioned that the centrality of concepts was sometimes contested: some philosophers considered judgements as more fundamental pieces of knowledge, since they operate a combination or connection of concepts that they thought not reducible to the concepts themselves. For our purposes, however, this debate is of little import; what matters is that concepts, either alone or in combination, were the central pieces of reasoning.

During the second half of the century, this became the basis for the view that the theory of classes is properly a logical theory. To understand this, however, we need to consider one further element belonging to the core of traditional logical doctrine: the distinction between the intension [Inhalt] and the extension [Umfang] of a concept. This was an inevitable component of the section 'On Concepts' in nineteenth-century logic treatises.

2.1.2. The origins of this distinction can be traced back to Antiquity, to the *Isagogé* of PORPHYRY, a third-century commentary on ARISTOTLE that was very influential during the Middle Ages.[9] In modern times the customary reference was to the Port Royal logicians, ARNAULD and NICOLE, who wrote *La Logique ou l'art de penser*, one of the most widely read treatises of the seventeenth century (ARNAULD & NICOLE *1662*). This is how they established the distinction between intension (or comprehension) and extension of concepts:

In these universal ideas, it is very important to correctly distinguish *the comprehension and the extension*.

By the *comprehension* of the idea we understand the attributes which it involves and which cannot be withdrawn without

9 Cf. FRISCH *1969*, 108-114; WALTHER-KLAUS *1987*, 9.

destroying the idea, as the comprehension of the idea of triangle involves extension, figure, three lines, three angles, equality of those angles summed up to two right angles, etc.

By the *extension* of the idea we understand the subjects to which the idea applies, and which are also known as the inferiors of a general term which, in relation to them, is called superior; as the general idea of a triangle extends to all the different species of triangles. (ARNAULD & NICOLE 1662, 51)

It turns out that the notion of extension made it possible to develop a formal analysis of the relations between concepts, i.e., of judgements and conclusions. In this way, ARNAULD and NICOLE were able to develop quasi-mathematically the theories of the conversion of judgements, and of the syllogistic figures of deduction─the core of Aristotelian syllogistics.

For our purposes, it is important to remark that HERBART's expositions of logic followed exactly that early practice. A summary of HERBART's way of dealing with logic is given in HERBART *1808*: this includes the distinction between intension and extension (p. 218) and the use of extensions for explaining the conversion of judgements (p. 220-221) and the syllogistic figures (p. 223-226). In 1837 he defined the intension of a concept as "the sum of its attributes"; the extension, as "the set [Menge] of the other concepts, in which the first appears as an attribute" (HERBART *1837*, 71).[10] In the same work, HERBART makes a nice application of these ideas to the notion of number: the natural numbers form the extension of the concept of number; moreover,

nobody would know what *number* is without first knowing what one, two, three, four are. The intension of this concept [of number] consists therefore in its extension. [HERBART *1837*, 73-74]

2.1.3. It may be doubted whether the idea of the extension of a concept, as handled by authors of the seventeenth to the early-nineteenth century, can be compared to the later notion of class employed by mathematical logicians, or to the mathematical notion of set. Among older logicians there was a tendency to concentrate upon the idea of hierarchies of concepts, analyzing the reciprocal relations genus-species (cf. even DROBISCH *1836*). This turned intension and extension into reciprocal elements: at any point in the hierarchy of concepts, the intensional elements are found in that point and higher ones, while the extension is whatever lies behind. The advances made in the second half

10 This text shows a quite common trait of 19th-century German logic textbooks: there is no reference to classes of objects, since the elements that form the extension are also concepts. But this happened under the assumption that an object is a "bundle of properties" or concepts (HERBART *1964*, vol.6, 199). On the variety of positions regarding this point, cf. PEIRCE *1931/60*, vol.2, 242-246.

of the nineteenth century depended upon a radical simplification: an indiference to the traditional problem of hierarchies coupled with a concentration upon the extensional that led to the notion of class.

In fact, there were indications of this step in the tradition; some authors had conceived concept-extensions as classes of individuals (see FRISCH *1969*; WALTHER-KLAUS *1987*). But again, all of this is not crucial for our purposes. What matters to us is that the centrality of concepts, together with the idea of concept-extensions, created a justification for speaking about a *logical theory of classes*. Talk about extensions of concepts was absolutely common in logic, in connection with the most central syllogistic doctrines. When classes emerged in the practice of mathematical logicians, and sets in that of mathematicians, the scene was set for an understanding of these notions as belonging to logic. The theory of classes and the theory of sets were regarded as parts of logic.

This is shown plainly in a sentence taken from the work of George BOOLE (1815-64). BOOLE was of course the man who realized the long-sought ideal of creating a satisfactory calculus of logic founded upon the model of algebra. In the introduction to *The mathematical analysis of logic* (1847) he gives further evidence of the centrality of concepts and the relation between concepts and extensions (now called classes) in traditional logic:

> That which renders logic possible, is the existence in our minds of general notions,—our ability to conceive of a class, and to designate its individual members by a common name. (BOOLE *1847*, 4)

According to BOOLE's unequivocal statement, concepts and classes are not just central to logic: they are the very foundation of logic.

In fact, the two assumptions of the centrality of concepts (or judgements), and of the direct relation between concepts and classes, were deeply ingrained in the minds of logicians; both can be followed up until the very end of the century. The second, in particular, turned out to be very important, since it is the root of the principle of comprehension that played such a crucial role around 1900. Throughout the rest of this paper we will encounter several examples of the persistence of those assumptions, taken from the works of RIEMANN, DEDEKIND, ZERMELO, etc., but many other cases could be given as evidence. At this point I want to mention a couple of them which seem particularly relevant.

Bernhard BOLZANO introduced several notions related to the idea of set, distinguishing between the cases in which order is or is not regarded, but what is basic to his approach is the explicit assumption that collections or sets [Mengen] are defined through concepts (BOLZANO *1851*, §§ 3-4). In BOLZANO's theory, sets depend on concepts.

The theory of classes ("domains") constitutes the central concern of vol.1 of SCHRÖDER's *Vorlesungen über die Algebra der Logik*, which around the turn of the century was perhaps the most successful German work on mathematical logic (cf. SCHRÖDER *1890/1905*, vol.1, especially 80-107; the concept/class connection is on p. 83). The logical symbolism presented in this work, like most other symbolisms from BOOLE to PEANO, was primarily designed to apply to classes, although it was also interpreted in terms of propositions. This is further confirmation of the centrality of concepts (resp. classes) in logical theory.

2.2. The diffusion of logic in 19th century Germany.

It is precisely to the relation between concepts and extensions or classes that RIEMANN appealed in his general definition of a manifold. But given the fact that in his lecture RIEMANN did not talk about logic explicitly, there could be some doubt whether we can assume the existence of an implicit reference. Fortunately, there is evidence that logic enjoyed a very wide diffusion in nineteenth century Germany. Not only do we find an impressive amount of logic treatises published in that period, but we also have indications that logic was an integral part of secondary education.

In secondary education logic can be found not only in association with philosophy, but also in close relation with language and grammar courses. Both in the well-established Latin and Greek courses, and the emerging teaching of German, the dominant linguistic view in schools and Gymnasien was that of the so-called General Grammar.[11] This movement took as an essential assumption the strict parallelism between sentences and the propositions of logic; to quote a rather late example:

The relation and connection of two concepts into a unity, designated and expressed by words, is called judgement when thought, sentence when spoken. (R. HERMANUZ, *Satzlehre* (1846), quoted in FORSGREN *1992*, 66)

Thus, General Grammar based its theory on traditional logic viewed from a formal standpoint (NAUMANN *1986*, 11, 55 and *passim*; FORSGREN *1985*, 46ff). This is why there are explicit references to grammar in the titles of some logic treatises written during the first half of the century (i.e., SIGWART *1835*). From a pedagogical viewpoint, the aim of this movement was to stimulate students to reflect on the relationship between language and thinking. The teaching of German was intended to be logical propaedeutics, and ultimately to lead to philosophy.[12]

11 Cf. PAULSEN *1897*, vol.2, MATTHIAS *1907*, FRANK *1973*, NAUMANN *1986*, FORSGREN *1992*.

12 NAUMANN *1986*, 11, 101, 105. It is worth noting that the emphasis on thinking and logic did not disappear from the teaching of German even after General Grammar was superseded by the Historicistic School in linguistics (NAUMANN *1986*, 110).

It is doubtful whether that situation held widely outside Prussia,[13] although some of the smaller German states are reported to have followed Prussian developments (cf. MATTHIAS *1907*). In southern Germany, especially Austria, it was not uncommon to find courses in 'philosophical propaedeutics' offered at secondary schools; these courses included formal logic. In Bavaria, the topics of practical and pure logic found their way into the syllabuses, and logic was sometimes also taught in connection with language courses. In fact, the connection between logic and language or grammar is not infrequently found in the writings of mathematical logicians.

Furthermore, logic formed an important element of introductory philosophy courses given at the universities. And at this point it must be taken into account that mathematics and the sciences belonged in the Faculty of Philosophy. It was precisely to the members of the Philosophical Faculty at Göttingen that RIEMANN addressed his *Habilitationsvortrag*. Thus, RIEMANN did not need to make any explicit reference to logic, he could be certain that his words would be properly understood. Generally speaking, it seems safe to assume that in RIEMANN's time all educated persons—in particular university students and professors—were familiar with traditional logic, and therefore with the basic idea of the connection between concepts (general notions) and classes or extensions.

On the other hand, it might seem that his appeal to a relation between manifolds (sets) and logic could be a peculiarity which neatly differentiates RIEMANN's approach from the trend which really led to set theory, that is to say, from CANTOR's work. In fact, by the 1890s CANTOR considered set theory as something completely different from logic. However, the case of DEDEKIND shows already that RIEMANN's position was not totally isolated. The views of several logicians and some testimonies of HILBERT and ZERMELO, considered below, will further show how common the concept/set relation was in the thought of nineteenth-century mathematicians. In this light, it is CANTOR's position which seems to have been rather isolated, however important his work on set theory was.

Since logical doctrine, so familiar to German scholars, was behind RIEMANN's and DEDEKIND's innovations, the reader may wonder why it took a long time until mathematicians accepted them. Partly, this was due to the fact that logic had never been intertwined so intimately with mathematics: after all, in schools and universities it was studied in connection with grammar or philosophy, not with mathematics. This gives a natural explanation why there must have been some delay in the diffusion of RIEMANN's or DEDEKIND's ideas. But the main answer, I believe, is that

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13 At this point I owe important clarifications to Volker PECKHAUS from Erlangen University, where research on logic teaching in German secondary schools has been done (cf. PECKHAUS *1991a*).

the introduction of set language in mathematics was only possible at the prize of abandoning the familiar objects and methods in which mathematicians were being trained. It required the adoption of new methods of proof adjusted to the characteristics of the new objects—manifolds, set-structures, point-sets, etc.—and this went in the direction of unprecedented levels of abstraction. Thus, resistance to those innovations can be seen as resistance to the epistemological and ontological 'revolution' going on in mathematics at the time.[14]

2.3. Logicism and other views.

The issue of classes or sets and their connection with concepts was also related to the rise of logicism, as is particularly clear in the case of DEDEKIND. The fact that significant parts of mathematics could be derived from set theory was understood by some as evidence that mathematics is just a part of logic. But this depends on the acceptance that set theory is indeed pure logic, involving one step further from assuming the concept/set connection. In order to clarify this matter, it will be worthwhile to devote some space to establish a subtler classification of opinions.

First of all, it is important to analyze what the acceptance of the concept/class relation does imply, and what it does not. In my opinion, we must think that the relation was generally accepted, even by authors who were *not* willing to consider the theory of classes as a part of logic. That is, an author could undertake an extensional analysis of concepts while thinking that this belongs properly not to logic but, say, to mathematics. There are reasons to think that this was the position of RIEMANN, and perhaps that of HERBART.[15] In this way, RIEMANN could consider manifolds (or sets, classes, extensions) as basic mathematical objects, and at the same time be far from a logicist position.

Likewise, authors could take it for granted that no set exists unless a determining concept (or property) is stated, without implying that set theory is a part of logic. CANTOR's position seems to have been precisely this one: his first explicit definition of a set—properly speaking, a "manifold"—speaks about elements that can be linked into a whole by some law (CANTOR *1883*, 204); this law entails the conceptual aspect, because it implies explicitly determined properties of

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14 Cf. GRAY *1992*, which discusses events directly related to our subject from a broader perspective. In a nutshell, and using GRAY's terminology, one of my points will be that questions of epistemology determined to an important extent—at least in the cases of DEDEKIND and RIEMANN—the revolution in mathematical ontology.

15 There is no evidence that RIEMANN ever embraced a logicist viewpoint. HERBART thought that extensions (classes) are useful, but not essential in logic (HERBART *1808*, 216, 222), which might imply that he regarded them as an auxiliary tool, the study of which belonged to a different sphere of knowledge. Moreover, HERBART rejected Kantian apriorism and considered logic as a simple propaedeutics of knowledge (ibid. 267); all of this undermines the epistemological thrust of logicism.

the elements.[16] The important point is to observe how the concept/set relation was generally accepted, whatever the epistemological position of the author concerning the foundations of mathematics.

But of course, it is also crucial to note that the contemporary conception of logic allowed for a step further, and in fact some mathematicians went on to consider the theory of classes (or sets) as a part of logic. This was the case of BOOLE, DEDEKIND, PEANO and SCHRÖDER, to give some examples. Towards the end of the century, the growing importance of classes in mathematical theories and foundational work was for some of these authors evidence that mathematics was a part of logic. This seems to have been the process which brought the logicist position into existence.

Among the earliest logicists we find FREGE, who restricted his logicism to arithmetic, and DEDEKIND, for whom all of pure mathematics was just logic. The case of DEDEKIND will be carefully analyzed below, and for the moment it suffices to say that he defended the logicist position in 1888, asserting that arithmetic, algebra, and analysis are only a part of logic (DEDEKIND *1888*, 335). Unlike DEDEKIND, FREGE is always cited as one of the great logicists; he formulated that viewpoint in his major works on the foundations of the number system (FREGE *1884*, *1893/1903*).

The case of FREGE is especially interesting because he is often considered the founder of modern logic, and because he stated most clearly the importance of the set/concept relation. FREGE seems to have arrived at the logicist viewpoint after realizing that arithmetical statements involve assertions about concepts (FREGE *1969*, 273; also FREGE *1884*). This means that during the 1870s he identified concepts as a central element of logical theory, and he kept doing so (see section 6.1). Moreover, FREGE gave the traditional connection between concepts and classes its most precise formulation, in the form of the principle of comprehension involved in "law V" of his *Grundgesetze* (FREGE *1893*, 35-36).

III. RIEMANN'S MANIFOLDS AS A FOUNDATION FOR MATHEMATICS

In his famous 1854 lecture on the foundations of geometry, RIEMANN presented the new notion of manifold as a prerequisite for deeper reflections on geometry and its axioms (hypotheses). The context in which that notion made its appearance was broader, however, since it was established as

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16 This seems to have been CANTOR's position from his first articles in the 1870s through the late 1880s. See the definition of fundamental sequences in 1872 (CANTOR *1932*, 92), his terminology of 'Inbegriffe' for sets (ibid. 115-118; this involves a reference to concepts [Begriffe], explicit in p. 117), and also his explanation of 'wohldefiniert', involving a reference to logical laws (ibid. 150). Nevertheless, in his 1895 *Beiträge* he avoided explicit reference to a law or property, maybe following the example of DEDEKIND (*1888*, see below).

a basis for the general theory of magnitudes (RIEMANN *1854*). Here it is important to consider that up to the 19th century it was customary to define mathematics as the science of magnitudes, since this suggests that manifolds were perhaps proposed as a foundation for mathematics. This is corroborated by both RIEMANN's definition and several passages in his lecture, but historians of mathematics have failed to see these broader implications of RIEMANN's new concept.

In my opinion, this failure is related to difficulties in the interpretation of RIEMANN's general definition, which depends on the rather forgotten ideas of traditional logic. Furthermore, the availability of modern concepts of differential geometry has an important effect in leading readers not to pay attention to RIEMANN's original explanations: since they have in mind current definitions of a differentiable manifold, they do not have to take pains in understanding an obscure definition that contains many incorrections. (It is well known that RIEMANN defined continuity in a nominal way, lacked a satisfactory definition of the dimension of a manifold, and simply presupposed differentiability.) This is what causes present-day readers to be unaware of the broader implications of the notion of manifold.

Meanwhile, for 19th century readers the opposite situation held: they encountered manifolds for the first time, and understanding RIEMANN's new notion was one of the main problems they faced when reading his lecture. Moreover, at a time when the foundations of mathematics were unclear, RIEMANN's comments on manifolds as a foundation should have caught the attention of many mathematicians.

3.1. Origins of the notion of manifold.

The available evidence suggests that the notion of a manifold evolved from the so-called Riemann surfaces, that RIEMANN introduced in the field of function theory.[17] There were mainly two reasons that led him to take this step in his dissertation of 1851. First, RIEMANN was primarily interested in the study of multi-valued functions, which forced him to face phenomena of ramification; his surfaces amounted to a geometrization of the branching properties of those functions. The domain of the function ceased to be a part of the complex plane, to assume the shape of a surface of several sheets which covers the plane; the function, multi-valued over the complex plane, becomes single-valued over the surface (RIEMANN *1851*, 7-9; *1857*, 89-91). Since a Riemann surface cannot be embedded in three-dimensional space, in 1851 he was already beginning to surpass the limits of traditional geometry.

17 RIEMANN presented his ideas on function theory in his dissertation (RIEMANN *1851*), but they were first published in his famous paper on Abelian functions (RIEMANN *1857*). On his function theory, see VERLEY *1978*, SCHOLZ *1980*, BOTTAZZINI *1986*, GRAY *1986*.

But second, the surface captured a good deal of information about its corresponding function, and RIEMANN was interested in ways of determining complex functions by means of a minimal set of properties. He was aware of the global dependence of analytic functions on their local behavior, and noticed how this meant that contemporary determinations of functions by means of global formulas involved redundant information (RIEMANN *1851*, 38-39, *1857*, 97). RIEMANN obtained minimal determinations by using a system of data which were partly analytic and partly geometrical: the geometrical information was contained in the Riemann surface, while the analytic part consisted in discrete facts, including the behavior of the function in singular points.[18]

In order to study the integrals of complex functions, he analyzed the Riemann surfaces from a topological viewpoint, developing new methods that enabled him to define the 'order of connectivity' of surfaces—essentially Euler's characteristic (RIEMANN *1851*, 9-12, *1857*, 91-96). And he showed how the topological invariants of surfaces are intimately connected to the properties of functions, an astounding example being the RIEMANN-ROCH theorem. In this way he opened a promising new area of mathematical research.

Some documents which have been recently published by Erhard SCHOLZ enable us to trace the development of RIEMANN's ideas in the time between his doctoral thesis and his lecture of 1854 (SCHOLZ *1982*). According to SCHOLZ's dating, in the period 1851-53 RIEMANN wrote down some manuscripts in which he dealt with the notion of a manifold and its relation to geometry, with *n*-dimensional continuous manifolds and *n*-dimensional topology. In my opinion, these manuscripts suggest that the notion of a manifold grew out of an attempt to find a satisfactory conceptualization of RIEMANN's new function-theoretical ideas.

The main text that supports this conclusion is SCHOLZ's appendix 4 (SCHOLZ *1982*, 228-229); the purpose of this manuscript was to establish the relations between manifolds, geometry, and intuition. During this early period, RIEMANN explained manifolds in terms of what essentially

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18 RIEMANN's tendency toward geometrical considerations in function theory is related to GAUSS more than any other mathematician. GAUSS's first (1799) and fourth (1849) proofs of the fundamental theorem of algebra were based on the geometrical representation of complex numbers (GAUSS *1863/1929*, vol.3, 74 and 114). He regarded his argument as belonging essentially to "a higher domain of the abstract theory of magnitudes, independent from the spatial, whose object is the combinations of magnitudes linked by continuity", i.e., to topology (GAUSS *1863/1929*, vol.3, 79). So he showed the interest and even necessity of a geometrico-topological approach to complex functions. (From a modern viewpoint, the so-called fundamental theorem of algebra belongs to function theory.)

Moreover, when he defended the geometrical representation of complex numbers, GAUSS spoke about these numbers as representing a "series of series" or "what comes to the same ... a *manifold* of two dimensions" (GAUSS *1863/1929*, vol.2, 176; emphasis added). At the end of this paper, GAUSS mentioned manifolds of more than two dimensions, and in his lectures he came to speak about *(n-k)*-dimensional manifolds (SCHOLZ *1980*, 16-17). All of this anticipates RIEMANN's reflections on manifolds, and thus it is only natural that RIEMANN referred to GAUSS as a source of his ideas (RIEMANN *1854*, 255).

is the space of states of a physical substance or object: given a "variable object" [veränderlicher Gegenstand, Ding], the different "forms of determination" [Bestimmungsweisen] which the object can take form the points of the manifold (SCHOLZ *1982*, 222). In the manuscript in question, RIEMANN illustrates this by taking the example of an experiment in which we measure both a temperature and a weight: the totality of possible results of the experiment forms a two-dimensional manifold. This idea involves no limitation of dimension, since we can also make experiments in which a higher number of variables has to be considered, and then we will find manifolds of more than three dimensions (SCHOLZ *1982*, 228-229).

Thus manifolds are abstract objects, and the notion of a manifold is independent of our geometrical intuitions. Furthermore, manifolds establish a frame within which it is possible to recover traditional geometry in an abstract way, without the least dependence on intuition (o.c., 229). Finally, this is employed to explain the use of manifolds in function theory: although in principle manifolds are abstract objects, in practice it is simpler and clearer to deal with them, not in an abstract way, but leaning on spatial intuition (ibid.). This is what RIEMANN had done with the surfaces, i.e., two-dimensional manifolds, that he considered in the context of function theory.

If we follow this line of reasoning backwards, it offers a plausible reconstruction of the path followed by RIEMANN in his development of the notion of manifold. It would seem that he felt puzzled by his use of geometrical constructs (Riemann surfaces) in complex analysis. Riemann surfaces were not a part of traditional geometry, since they were not objects in three-dimensional space, and so their theoretical basis was unclear. Moreover, he might well have felt that the use of surfaces was not rigorous, since the contemporary tendency was to avoid resorting to geometry and intuition in the development of analysis.[19] So he was led to the questions: What is the true foundation for higher-dimensional geometry and topology? Does that foundation explain the relations between geometry and functions encountered in complex analysis? The notion of manifold gave the answer to these questions, being an abstract concept on which both the theory of magnitudes and (abstract) geometry could be based.[20]

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19 This was not only the influential approach of CAUCHY, but also the direction taken by GAUSS. (Both mathematicians had strongly influenced DIRICHLET, who was the main representative of rigorous analysis during this time; DIRICHLET was RIEMANN's professor in Berlin and advised him while working on his dissertation and *Habilitation*.) RIEMANN's approach to function theory seemed to contradict that tendency, and therefore posed a problem: Did analysis really depend on geometry? In the above mentioned appendix 4, RIEMANN was able to turn this question and show that, on the contrary, geometry depended on the abstract theory of magnitudes, which for him was the basis of analysis (it suffices to remember that he refers to function theory as "the theory of a variable, complex magnitude", cf. RIEMANN *1851*).

20 In his manuscripts of the same period RIEMANN generalized his topological analysis of surfaces to the case of *n*-dimensional manifolds (cf. 'Fragment aus der Analysis Situs', in RIEMANN *1892*, 479-482, and its dating in SCHOLZ *1982*). This fact adds to the plausibility of the above reconstruction: if *n*-dimensional manifolds evolved as a theoretical basis for Riemann surfaces, it is only

Be that as it may, the fact is that RIEMANN was employing a notion of manifold by 1851-53. His first definition of manifolds was essentially equivalent to the idea of the space of states of a physical substance, but in the 1854 lecture we find a different definition of manifolds, whose purport was to attain greater generality. Likewise, although in the lecture we find again a study of the interrelations between magnitudes, manifolds, and geometry, there is an important novelty: by 1854 RIEMANN had realized the richness of different metrics that can be given to one and the same topological substratum (manifold), and how this could be used to produce a clearer and richer picture of the fundamental notions of geometry. Here again, RIEMANN's geometrical ideas were rather abstract; it is well known that many of his contemporaries either found them difficult to accept or failed to see their implications.

3.2. RIEMANN's theory of manifolds.

3.2.1. Magnitudes and manifolds. A difficult point in RIEMANN's conception is the connection between manifolds and the theory of magnitudes. In searching for a foundation on which to base Riemann surfaces and their topological analysis, RIEMANN stuck to the traditional view of mathematics as the theory of magnitudes.[21] Until the middle of the 19th century—and even today in some dictionaries—it was common to define mathematics as the science of magnitudes. This, of course, originated with the Greeks, who also distinguished two kinds of magnitudes: the discrete, and the continuous.[22] This conception was devised so as to include arithmetic and geometry, the two historical roots of mathematics, under a common heading.

RIEMANN preserved this general conception while mixing it with some new, abstract ideas, the main novelty being the notion of manifold, that he took as basic for the theory of magnitudes. The amount of this transformation, and the extent to which it involved a radical departure from the Greeks, is easily seen when we come to the idea of a topological analysis of manifolds. Meanwhile, RIEMANN's adherence to the traditional view also implies that his theory meant to embrace both geometry and arithmetic.

As regards the relationship between magnitudes and manifolds, a careful reading of the

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natural that the topological methods employed for studying the latter were then applied to the former.

21 The early development of set-theoretical notions is intimately related with re-conceptualizations of the traditional theory of magnitudes. Examples of this are Hermann GRASSMANN (1809-77), RIEMANN, and WEIERSTRASS. The case of DEDEKIND, studied below, shows the motives that led mathematicians to divorce their new, abstract ideas from the old word 'magnitude', and to talk about sets properly.

22 ARISTOTLE included number among discrete magnitudes, and counted line, surface, body, time, and space as continuous magnitudes (*Categories*, 4b 20).

1854 lecture shows that RIEMANN was simply using both words as synonymous. For instance, the first section of his lecture, devoted to explaining the idea of an *n*-dimensional manifold, is entitled "Notion of a multiply expanded magnitude" (RIEMANN *1854*, 255). This identification was possible because he—like GAUSS—conceived of magnitudes in a more abstract way than was customary: to him, quantitative relations were only one part of the general theory of magnitudes, the other being the topological study of magnitudes or manifolds (o.c., 256).

Having said this, it is also important to emphasize the fact that RIEMANN conceived of his explanation of the notion of manifold as belonging to philosophy, rather than to mathematics:

> In trying to solve the first of these tasks, the development of the concept of a multiply expanded magnitude, I believe I can ask for an indulgent judgement all the more since I have little practice in such tasks of a philosophical nature, where the difficulties lie more in the concepts than in the constructions; and because, apart from very brief indications given by Gauss in [*1831, 1849*] and some philosophical investigations of Herbart, I have been unable to use any previous work. (RIEMANN *1854*, 255)

Given this context, it should certainly not be surprising that RIEMANN's definition of a manifold, given immediately below the preceding lines, was based on contemporary logical (i.e., philosophical) notions.

The definition is contained in a short sentence:

> Notions of magnitude are only possible where a general concept is found which admits different forms of determination [Bestimmungsweisen]. According as a continuous transition does or not take place among those determinations [Bestimmungsweisen], from one to another, they form a continuous or discrete manifold [Mannigfaltigkeit]; in the first case the single determinations are called points, in the last one elements of that manifold. (RIEMANN *1854*, 255)

With section II in mind, it should be easy to interpret this explanation: we find a clear statement of the traditional concept/class relation. And in view of the evidence presented in that section, I regard it as proven that contemporary readers should have interpreted RIEMANN's manifolds as intending to be the classes of traditional logic.[23]

Nevertheless, the above definition has puzzled modern commentators, possibly because they were formed after the 'revolution of the antinomies', which above all affected logic, more than

23 Concerning the terminology, recall that German did not supply a privileged word for expressing the notion of set, as other languages did. Therefore, each author proposed a terminology of his own, making things complex. RIEMANN's terminology was accepted by CANTOR himself from 1878 to 1890.

mathematics or set theory itself (see below). BOURBAKI, for instance, chooses to simplify his reader's task, translating "element" where RIEMANN wrote "general concept" (BOURBAKI *1969*, 176); in this way, BOURBAKI alters radically the original intention of the author. A greater misunderstanding can be found in SCHOLZ's interpretation that what RIEMANN called a manifold was the general concept itself (SCHOLZ *1980*, 30).

In order to further corroborate my own interpretation, I will mention that several other passages from RIEMANN's lecture can only be understood when we interpret his "manifolds" as being simply sets. This happens with an incidental comment, where he uses the term "manifold" to refer to what we would call the set of possible cases for the result of a given experiment.[24]

It is likely that the idea of connecting his manifolds with concepts, thus relying on logical conceptions, was suggested to RIEMANN by HERBART's work. In HERBART's writings it is easy to find comments on the relation between concepts and continua. An early example is his mention, in the course of a discussion of the right way to teach mathematics, of "the *whole continuum* that is contained under a *general concept*" (HERBART *1964*, vol.1, 174; emphases in the original). In his theory of spatial concepts, HERBART used to give as examples the continua that fall under the concepts of tone and color (cf. SCHOLZ *1982a*). In both cases HERBART's terminology and general conception are very close to RIEMANN's, although RIEMANN did not think that most ordinary concepts involve continuous, but discrete manifolds.[25]

3.2.2. Arithmetic, topology, and geometry. Compared to the approach of the manuscripts written in 1851-53, it seems clear that the 1854 definition was marked by a desire to generalize as much as possible, in order to include the whole of mathematics under the new conception. This is clearly shown by RIEMANN's comments on discrete manifolds, related of course to traditional views on magnitudes as the foundation of mathematics.

Immediately after the definition of a manifold, RIEMANN mentions the fact that, for any given objects, cultivated languages always make it possible to find a concept under which they all fall. In this way, his definition gives us the highest degree of generalization, because *any* objects

24 Regarding the question to what extent are our hypotheses confirmed by experience, he says: "there is an essential difference between mere extensive [topological] relations and metric relations, in that among the former, where the possible cases form a discrete manifold, the dictates of experience, never completely sure, are not imprecise; while for the latter, where the possible cases form a continuous manifold, every determination from experience remains always imprecise—as high as may be its probability of being nearly exact" (RIEMANN *1854*, 266).

25 Cf. SCHOLZ *1982a*, 421-424, where HERBART's theory of the spatial is discussed, together with its similarities and important differences from RIEMANN's theory of manifolds.

whatsoever are intended to be mathematizable. The notion of number thus attains greater clarity, as RIEMANN suggests in a comment within brackets.[26] And the sentence beginning RIEMANN's following paragraph confirms the relation between discrete manifolds and numbers:

Determined parts of a manifold, distinguished by a characteristic or a boundary, are called quanta. Their comparison according to quantity occurs for discrete magnitudes by numbering, for continuous by measuring. (ibid.)

The direct relation between discrete manifolds and numbers is clearly stated, and although there is a great distance between this and a set-theoretical foundation for the number system, it is nevertheless clear that the basic idea—sets as the basic objects of arithmetic—was suggested by RIEMANN.

But, although he included considerations about discrete manifolds and their relation to numbers for the sake of completion, RIEMANN's main interest was in continuous manifolds, which formed the basis for his work on function theory, topology, and geometry.[27] In a footnote at the end of the lecture RIEMANN indicated that the section we are discussing "constitutes also the preliminary work for contributions to analysis situs", i.e., topology (RIEMANN *1854*, 268).[28] According to RIEMANN, even if we cannot compare two continuous magnitudes by measuring, it is still possible to study them in an abstract, mathematical way:

The investigations which can be carried out in this case constitute a general part of the theory of magnitudes, independent of metric determinations, where magnitudes are considered, not as existing independently of position nor as expressible by means of a unit, but as domains in a manifold. Such investigations have become a necessity for many branches of mathematics, particularly for the treatment of multi-valued analytic functions, and the dearth of them is one of the main reasons why the celebrated theorem of Abel and the achievements of Lagrange, Pfaff, and Jacobi in the general theory of differential equations have remained unfruitful for so long. (RIEMANN *1854*, 256)

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26 Since any given objects may constitute a single manifold, "in the theory of discrete magnitudes, mathematicians could therefore depart unhesitatingly from the postulate of considering given things as all of one kind" (RIEMANN *1854*, 256). This is an obscure comment indeed, but in the light of the reference to discreteness and to several things being "all of one kind", I think we should read it as a discussion of the notion of unit. EUCLID had defined number as a 'collection of units', which was still a current definition around 1850. But mathematicians and philosophers of that time were puzzled by the fact that number-units should be simultaneously equal and unequal (cf. FREGE *1884*). RIEMANN suggests that a formulation in terms of sets eliminates the problem: diverse objects can be regarded as equal insofar as they belong to one and the same manifold.

27 RIEMANN's assumption that continuous manifolds are always differentiable turned continuity into the only prerequisite of the differential geometry of manifolds. It is well known how WEIERSTRASS showed the extent to which this was erroneous.

28 On RIEMANN's topological ideas, see PONT *1974*, SCHOLZ *1980*.

Here we find the crucial point in which RIEMANN introduces the new program of topology within the apparently traditional framework of the theory of magnitudes.[29] Furthermore, the relation between topology and function theory is clearly stated.

The core of RIEMANN's lecture was the fact that a single topological basis (a continuous *n*-dimensional manifold) may be given a great number of different metrics, and its consequences for the study of physical space. Therefore, the distinction between a topological and a metrical theory of manifolds was the basis of RIEMANN's geometrical conceptions. In fact, the origins of RIEMANN's ideas on differential geometry and physical space should be traced back to the moment when he realized the richness of different metrics which can apply to one and the same topological substratum (see SCHOLZ *1982*).

3.3. RIEMANN's influence on the history of sets.

RIEMANN's contribution gives us a partial answer to a question stated in the introduction: where does the approach to the foundations of mathematics in terms of sets come from? It seems that RIEMANN arrived at his new view by reflecting on the surfaces that he had introduced in function theory, which seemed to imply an intimate relation between complex analysis and geometry. His explanation of that phenomenon involved a revision of the classical notion of magnitude, and the idea that magnitudes constitute the foundation of mathematics and in particular of geometry. RIEMANN proposed a new view of these most basic mathematical objects: they were to be considered as manifolds, that is, classes associated to concepts. Both quantitative and topological relations emerged from the comparison of manifolds, with discrete manifolds giving rise to arithmetic, and continuous manifolds constituting the foundation of topology and geometry.

As we see, RIEMANN understood the surfaces of his function theory as based on the notion of set, and the same is true for the n-dimensional manifolds that appear in his geometrical work. This amounts to the introduction of set language in both fields. But he also went one step further, since sets or manifolds turned out to be the basic objects of a new approach to magnitudes, and thus a foundation for mathematics. What we do not find in his work, however, is any development of an autonomous theory of sets, but the possibility remains open that his seminal ideas stimulated other authors to work on set theory.

Since RIEMANN's conception was of a general and intuitive nature, we should expect it to have influenced further developments more on a general level than in the customary way of

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29 His terminology is very similar to that of GAUSS, speaking of 'analysis situs' as an autonomous part of the general theory of magnitudes (see footnote 18).

particular mathematical results or techniques. The ideas that we have reviewed offered a new vision of mathematics and its foundations, but not particular methods or results that would be important later in the theory of sets or even of point-sets. Their effectiveness lay in their potential to suggest interesting ways of inquiry, or even a research program, thus leading to particular questions. Although a general influence of this kind is more difficult for the historian to assess, its importance should not be undervalued. As CANTOR wrote in 1868, "in mathematics the art of posing questions is more consequential than that of solving them" (CANTOR *1932*, 31).

3.3.1. Diffusion; CANTOR's manifolds. RIEMANN's general vision of mathematics remained unknown outside Göttingen until 1868, when DEDEKIND published both of his *Habilitation* works in volume **13** of the *Abhandlungen der Königlichen Gesellschaft der Wissenschaften zu Göttingen*. During the 1850s and most of the 1860s, only close friends such as DEDEKIND could have known his speculations, discussing them with the author himself. The other paper that DEDEKIND edited in 1868 is also famous: it is RIEMANN's work on trigonometrical series, which included his definition of the integral. This and the paper on geometry appeared simultaneously, and they immediately caused a sensation in the German mathematical world. Notably, both played some part in the development of set theory.

The impact of the paper on trigonometrical series can be judged from works by HANKEL, HEINE, CANTOR, and DU BOIS REYMOND that appeared two or three years later. RIEMANN's paper did not contain any direct contribution to the theory of point-sets, nor any reference to the notion of manifold. But his new definition of the integral opened the way for a systematic study of discontinuous functions, and therefore constituted a most important background for the study of point sets in connection with questions of real analysis, a study begun by the above mentioned authors (cf. HAWKINS *1975*, DAUBEN *1979*).

As regards the lecture on geometry, its impact can be seen from the papers of HELMHOLTZ and BETTI, and from KLEIN's *Erlanger Programm* of 1872 (cf. SCHOLZ *1980*). But it is not so easy to judge the impact of RIEMANN's reflections on the general notions of magnitude and manifold. HELMHOLTZ and KLEIN adopted the term 'manifold', but interpreted it in a rather restricted way, and they did not develop RIEMANN's considerations on the foundations of arithmetic and topology. Nevertheless, the possibility of an influence of this aspect of RIEMANN's lecture on authors related with the development of point-sets should not be overlooked. This is the more probable, since both papers were published together, and both were widely read.

Among the names mentioned above, HANKEL and CANTOR are the authors for whom the probability of such an influence seems higher.[30] In the case of CANTOR, it is notable that from 1878 to 1890 he termed his field of study 'theory of manifolds' [Mannigfaltigkeitslehre]. And the first time it happened he established a direct relation to RIEMANN's 1854 lecture: it was in a paper of 1878 devoted to show that all continuous 'manifolds' have the same power or cardinal number. Here, CANTOR presented the matter in connection with the Riemannian notion of *n*-dimensional continuous manifold, and showed how RIEMANN's characterization of dimension was insufficient (see below). Afterwards, his decisive series of papers published from 1879 to 1884 was devoted to 'infinite, linear point-manifolds'; it gradually incorporated all the basic ideas of transfinite set theory.

It has been suggested that CANTOR's use of the word 'manifold' could have come from WEIERSTRASS's lectures (Johnson *1979*, 128-129), but an analysis of the use of this term in extant transcriptions of those lectures suggests that WEIERSTRASS never gave it the general meaning of set. WEIERSTRASS seems to have called certain subspaces of $\mathbf{R}^n$ 'manifolds', which is similar to GAUSS's use of the word, and also to what a superficial reading of RIEMANN's lecture might suggest.[31] Such a usage would not allow for calling 'manifold' a set of points scattered in a line or space, which is the sense given to the word by CANTOR in his papers of the period 1879-84. On the other hand, RIEMANN's conception of manifolds as classes allows this kind of use.[32] Moreover, it was RIEMANN, not GAUSS or WEIERSTRASS, who talked about manifolds in a systematic way, and in connection with arithmetic, analysis, and geometry. And it was to RIEMANN that CANTOR referred in the 1878 paper in which he first used the word 'manifold'.

Notably, DEDEKIND himself understood CANTOR's terminology to be related to RIEMANN's work. In a letter of 1879, he proposed to replace the clumsy word 'Mannigfaltigkeit' by the shorter 'Gebiet' [domain], which, he said, is "also Riemannian" (CANTOR & DEDEKIND *1937*, 47). Furthermore, in his 1888 book devoted to the set-theoretical foundations of natural numbers, DEDEKIND mentioned the word 'Mannigfaltigkeit' as a synonym for 'System', that is, set (DEDEKIND *1888*, 344). So it seems clear that both DEDEKIND and CANTOR were aware of the

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30 For Hermann HANKEL (1839-73), see his *1870*. Apart from this early and interesting contribution, he left no other traces in the development of the theory of point-sets because of his early death.

31 Cf. WEIERSTRASS *1986* and *1988*, also WEIERSTRASS *1894/1927*, vol.7, 55-60, and PINCHERLE *1880*, 234-237.

32 Although RIEMANN's association of manifolds with concepts might seem to set narrow limits on acceptable classes, it should be noted that "being a point at which a given function is discontinuous" would be considered as a concept by nineteenth-century logicians. It is in this sense that RIEMANN's definition is ampler than WEIERSTRASS's or GAUSS's use of the word.

A difficulty still exists, though, when we consider that apparently no concept could be associated to an *arbitrary* set of points in the line. This might have been the reason why both DEDEKIND and CANTOR later tended to dilute or even abandon the concept/class connection (see below).

fact that RIEMANN's manifolds intended to be nothing but sets.

A problem with ascribing a Riemannian ancestry to CANTOR's manifolds is that CANTOR failed to understand or to accept RIEMANN's vision of mathematics as based on manifolds. Until 1884, CANTOR seems *not* to have considered manifolds or sets as a foundation for mathematics; rather, he seems to be studying manifolds as the latest refinement of mathematics, making use of any previously developed tools (MEDVEDEV *1984*). An example will clarify what I mean: if we are going to base analysis on the notion of set, we should develop the necessary parts of set theory without recourse to analysis itself; in this spirit, during the early 1880s STOLZ and HARNACK defined the (outer) content of point sets in such a way that the notion of content could be used to define the integral (see HAWKINS *1975*). But when CANTOR presented his version of the same notion in 1884, he defined the outer content of a point-set essentially in terms of the integral of its closure (CANTOR *1932*, 229-231), which of course precludes the use of his definition as a foundation for integration theory. This evidences the fact that he was not interested in or accustomed to consider sets as a foundation for mathematics.[33]

An interesting, if speculative, explanation for this deviation from RIEMANN's viewpoint can be obtained from the fact that CANTOR was trained under WEIERSTRASS. Like RIEMANN's theory of manifolds, WEIERSTRASS's rigorous theory of the number system was presented under the ambiguous language of magnitudes.[34] This might have caused CANTOR to pass over RIEMANN's identification of magnitudes and manifolds, retaining a Weierstrassian view of magnitudes, together with a more Riemannian conception of manifolds. At least, that might be the implication of the following comment, written in 1882:

> Most of the difficulties of principle encountered in mathematics have their origin, it seems to me, in the ignorance of the possibility of a purely arithmetical theory of magnitudes and manifolds. (CANTOR *1882*, 156)

This text might imply that CANTOR considered magnitudes and manifolds as basically *different* kinds of objects, and furthermore as the two fundamental kinds of objects of (pure) mathematics. Such reading is coherent with his general attitude when studying set theory: he could rely on

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33 Earlier examples of the use of analysis for the development of set theory are his first proof of the non-denumerability of the real numbers (1874), his proof of the equipollence of continua that differ in number of dimensions (1878), and some other theorems contained in the 1879-84 series of papers on point-manifolds (see CANTOR *1932*, 153, 161-164).

34 In defining the real numbers, both WEIERSTRASS and CANTOR spoke about 'numerical magnitudes' [Zahlengrössen]; a rigorous theory of magnitudes was to be based upon the notion of natural number (see WEIERSTRASS *1986* and *1988*, CANTOR *1882*, 150).

integration for the definition of the (outer) content of manifolds because integration theory depended on the theory of magnitudes, not on the theory of manifolds.

3.3.2. Topology, continuity, and dimension. Among the questions opened by RIEMANN's vision, one of the most basic was to elaborate further the fundamentals of his theory of manifolds. This theory was based on a distinction between discrete and continuous manifolds, and there were open questions in connection with both. The relation that RIEMANN established between discrete manifolds and numbers was still very rough, but it might be related to the fact that DEDEKIND undertook the development of set-theoretical foundations for the number system. But, above all, it was the matter of continuous manifolds and topology that needed further development.

Notably, DEDEKIND wrote some manuscripts on basic topological notions. In a manuscript of 1863-66,[35] he defined the notions of an open set, of its interior, exterior, and boundary, proving related theorems, and treating those concepts within the context of metric spaces. The existence of this manuscript was mentioned in a letter to CANTOR of 1879, where DEDEKIND comments on the need of a rigorous exposition of the elements of the "theory of manifolds" independently of geometrical intuition, and explains how he came to occupy himself with such questions and find "some definitions which seem to offer a very good foundation" (CANTOR & DEDEKIND *1937*, 48).

But this was not DEDEKIND's first contribution to the foundations of RIEMANN's theory of manifolds; there was a prior one obtained in the year 1858,[36] and published in 1872. Of course, I am referring to his famous paper on real numbers and continuity (DEDEKIND *1872*), which has never been considered as a contribution to Riemannian subjects. The connection is the following. In his lecture, RIEMANN explained the continuity of a manifold by reducing it to the possibility of continuous transitions from any point to any other (which seems similar to path-connectedness). But, since he presupposed this notion of a continuous transition along a path, his explanation was almost purely verbal, and so the basic distinction of his theory (discrete vs. continuous manifolds) lacked a satisfactory foundation.

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35 'Allgemeine Sätze über Räume' (DEDEKIND *1930/32*, vol.2, 353-355). The grounds for my dating of this manuscript are the following. The manuscript was the result of an attempt to establish a satisfactory foundation for the so-called DIRICHLET principle (CANTOR & DEDEKIND *1937*, 48), and this attempt was made in the context of DEDEKIND's projected edition of DIRICHLET's lectures on potential theory. Since this project was tackled after the first publication of DIRICHLET's *Vorlesungen* in 1863 (DEDEKIND *1930/32*, vol.3, 393), and taking into account that DEDEKIND worked hard on RIEMANN's *Nachlass* from 1866, it is clear that 'Allgemeine Sätze über Räume' should have been written during the period 1863-66. DEDEKIND's notable use of the word 'Körper' to refer to open sets (and not to fields) corroborates that the manuscript was written before 1870.

36 See DEDEKIND *1872*, 316. DEDEKIND could give very precise datings due to the fact that he kept an extremely detailed diary, unfortunately lost (in it he even noted correspondence received, and daily temperatures!).

The first abstract definition of continuity was given by DEDEKIND in his paper of 1872, and he himself emphasized that the definition was perfectly general, applying to all continuous domains.[37] In establishing a theory of the real numbers "everything must depend", says DEDEKIND, on the definition of continuity, "and only through it shall we obtain a scientific foundation for the investigation of *all* continuous domains" (DEDEKIND *1872*, 322). The emphasis here is DEDEKIND's, and there is little doubt that he saw his definition of continuity as applying to RIEMANN's manifolds.[38] He must have noticed how an essential distinction in RIEMANN's theory lacked a sound basis, and therefore must have considered his definition as a contribution to the foundations of the theory of manifolds.

It was also in 1872 that CANTOR presented his theory of the real numbers, in which CAUCHY sequences are used to define them; he postulated an axiom of continuity that for **R** is equivalent to Dedekind's definition of continuity (CANTOR *1872*). From a modern standpoint, both are just methods for the *completion* of spaces, and so they do not afford a satisfactory definition of continuity. Nevertheless, they were crucial steps in the way towards an abstract approach to topology, and they fulfilled satisfactorily their purpose within the context of real number-spaces; in fact, only after 1900 did the abstract notion of a topological space begin to emerge. One more step along this line was given by CANTOR in his famous 'Grundlagen einer allgemeinen Mannigfaltigkeitslehre' of 1883, where he offered a new abstract definition of continuity.[39] It basically develops his earlier contribution, generalizing it to a straight definition of the continuity of $\mathbf{R}^n$; this definition showed the way to be followed by subsequent topological approaches to continuity. But the important point here is that, in all of these contributions, both set theorists were working within RIEMANN's tradition.

Coming back to CANTOR's and DEDEKIND's theories of real numbers, the common trait

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37 An objection might arise here, since DEDEKIND's definition of continuity can only be applied directly to linear, densely ordered sets; we cannot use it to explain the continuity of multidimensional domains. But it is possible to give an answer to this objection, since DEDEKIND conceived of 'pure' mathematics as the science of number. Along this line, he tended to think of *n*-dimensional space, not in an abstract way, but in the sense of a "real number-space of *n* dimensions", as he wrote in 1897 (DEDEKIND *1930/32*, vol.2, 114). Any continuum of the kind they were considering may be put in a bicontinuous one-to-one correspondence with an $\mathbf{R}^n$; and the continuity of $\mathbf{R}^n$ can be explained on the basis of that of **R**. Certainly, this indirect solution is worse than a straight analysis of *n*-dimensional continuity, along the lines of CANTOR's later definition (CANTOR *1883*), but it is a possible, rigorous solution.

38 The same emphasis reappeared some years later, when DEDEKIND wrote that his definition of continuity offered "a firm and at the same time simple foundation for infinitesimal analysis and for the investigation of all continuous domains" (DEDEKIND *1930/32*, vol.2, 356). Moreover, we know that the word 'domain' [Gebiet] was for him related to RIEMANN's views (see above).

39 CANTOR *1883*, 190-194. In CANTOR & DEDEKIND *1937*, 52, CANTOR says that he first tried to generalize directly DEDEKIND's definition, but, after this attempt failed, he came to use a new approach directly related with the 'fundamental sequences' used in his own definition of the real numbers.

that they share can arguably be connected with the influence of RIEMANN's lecture. It is well known that CANTOR and DEDEKIND presented their independent theories almost simultaneously in 1872, but that they coincided in one important point: the *axiom of continuity* of the line. Both emphasized that, while it was possible to build genetically a continuous number-domain, the continuity of geometric lines had to be postulated (CANTOR *1872*, 97; DEDEKIND *1872*, 322-323). The coincidence is most notable when we consider that WEIERSTRASS and HEINE, when treating the question of real numbers around the same time, did not touch upon that point. But the idea of considering spatial continuity as a hypothesis would seem natural to anyone influenced by RIEMANN's lecture on geometry. Here, the properties of space were considered as consequences of some hypotheses which are to be experimentally tested; they could be very probable, but never absolutely certain. In particular, RIEMANN never dismissed the possibility that physical space could be discrete, i.e., discontinuous (see RIEMANN *1854*, 268).

I want to mention, finally, a well-known and important contribution of CANTOR to RIEMANN's theory of manifolds: his proof that the notion of dimension of a manifold needed to be refined. In his lecture, RIEMANN analyzed the notion of dimension through a peculiar 'mechanical' generation or reconstruction, and suggested how points in the manifold might be determined by means of coordinates.[40] RIEMANN's conclusion was simple: the essential characteristic of an *n*-dimensional manifold is that *n* independent coordinates are required in order to determine the position of a point (RIEMANN *1854*, 258). This conclusion was accepted by most authors, particularly Helmholtz in his influential papers, until CANTOR showed in 1878 that points in an *n*-dimensional manifold can be determined by means of a single coordinate (CANTOR *1878*, 120-121).[41] Nevertheless, the theorem of the invariance of dimension under bicontinuous mappings was immediately suggested by DEDEKIND (see CANTOR & DEDEKIND *1937*); although a correct proof of this result was only given by BROUWER in 1911, NETTO's and CANTOR's insufficient proofs seem to have satisfied everybody until the end of the century (JOHNSON *1979/81*, DAUBEN *1979*).

3.3.3. On the way to abstraction. There is still one side of RIEMANN's contribution left to discuss, which is more directly related to technical aspects of his work. Although RIEMANN did not contribute directly to the development of abstract set-theoretical techniques and results, his

40 This was a crucial point, for it made possible an analytic treatment of manifolds, and therefore the definition of metrics by means of the fundamental quadratic form and the measure of curvature (see SCHOLZ *1980*, 31-45).

41 This implies that all $\mathbf{R}^n$ are equipollent, i.e., have the same cardinality as $\mathbf{R}$.

approach to mathematics can be characterized as conceptual or abstract, and the question remains whether it paved the way for the development of abstract set theory. I will consider this issue here in connection with RIEMANN's conception of topology.

It is obvious that set theory emerged from the study of the concrete sets suggested by traditional mathematics. In my opinion, the early history of set theory (up to about 1890) should be considered as a process of progressive distinction of different kinds of abstract features or structures that appear in those traditional, concrete sets. But the first such distinction, in connection with questions of geometry and analysis, was that of topological vs. metric aspects, and here the importance of RIEMANN's contribution is undeniable. A second step was DEDEKIND's isolation of algebraic structures in the context of his work on Galois theory and algebraic number theory. A third step was CANTOR's discovery of the transfinite realm, but here it is important to emphasize the fact that in CANTOR's theory of point-sets (and therefore, in his work until the mid-1880s) transfinite and topological aspects of sets are not clearly differentiated.[42]

Of course, topology was not the exclusive brain-child of RIEMANN,[43] but RIEMANN draw the most general consequences regarding the new topological viewpoint, and he did it in connection with the notion of a manifold. The effect of these ideas on the development of set theory may be judged from a comparison of the cases of Bernard BOLZANO (1781-1932) and CANTOR.

BOLZANO is usually named whenever the origins of set theory are discussed. In fact, he proposed to base mathematics on set-like notions, was a partisan of the infinite, and even came very near to such a central notion of set theory as cardinality—or power, in CANTOR's terminology. BOLZANO tried to build up a precise theory of the mathematical infinite, but after being close to the right point of view,[44] he departed from it in quite a strange direction. BOLZANO stated clearly the fact that two infinite sets can be put in a one-to-one correspondence while one of them is a subset of the other (BOLZANO *1851*, 27-28). But from this he did not conclude that they have equal cardinality: he preferred to conceive of equality (with respect to the "multiplicity of parts")

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42 This is particularly clear in the 1879-84 series of papers on point-manifolds: topological and abstract notions are presented together, and most papers are devoted to a joint study of derived sets (a topological notion) and cardinality. It was only the discovery of transfinite numbers that enabled CANTOR to step from the theory of point-sets to transfinite set theory (see FERREIRÓS, forthcoming). This new viewpoint was not incorporated into the 1879-84 series; it first appeared in the 'Principien einer Theorie der Ordnungstypen' (1884-85, published in GRATTAN-GUINNESS *1970*), and came to public knowledge through the 1895/97 'Beiträge zur Begründung der transfiniten Mengenlehre'.

43 The appearance of non-metric geometries—projective geometry in particular—opened the way to topology; important elements of topology appeared in the work of GAUSS and LISTING, in WEIERSTRASS's lectures, and so on.

44 Here, I call 'right' the idea that cardinality is the only meaningful way to compare abstract sets "with respect to the multiplicity of their parts [elements] (that is, if we abstract from all differences between them)" (BOLZANO *1851*, 30; see BUNN *1977*).

only when two sets are identical (o.c. 31). In order to justify this, he discussed two examples of correspondences between segments, that is, cases taken from geometry. When viewed from a modern standpoint, this suggests that his familiarity with Euclidean geometry led him to ascribe too much importance to metric considerations; this led him to that 'erroneous' viewpoint.

Non-metric geometrical ideas seem therefore to have been a prerequisite for the development of abstract set theory.[45] Such ideas began to emerge within the work of projective geometers, and surfaced in the early evolution of topology. Judged from this viewpoint, RIEMANN's contribution, in which we find an explicit differentiation of topological and metric aspects (RIEMANN *1854*), would thus seem to have been crucial in the way to abstraction.

IV. DEDEKIND'S "LOGICAL THEORY OF SYSTEMS"

DEDEKIND is mainly remembered for his work in two areas: algebraic number theory and the foundations of the number system; his set-theoretical ideas can be found in both. This is not coincidental, since DEDEKIND came to conceive of algebraic number theory as part of an edifice that had at its base the notion of number and its set-theoretical prerequisites.[46] His development of algebraic number theory was thus consistent with the basic traits of its assumed arithmetical framework.

In both areas, DEDEKIND's use of sets can be traced back to the late 1850s, but not significantly earlier. This point can be substantiated through an analysis of his *Habilitationsvortrag* (DEDEKIND *1854*), in which DEDEKIND explained his conception of a rigorous development of mathematics, beginning with the elements of the number system. Although some of the views expressed here belong to his life-long convictions—including the idea of a step-by-step definition of the number system—the notion of set is not even suggested in the lecture. Later, sets became the indispensable tool for his genetical definition of numbers. While writing the *Habilitationsvortrag*, DEDEKIND did not see a problem in the introduction of new numbers, but only in the extension of the basic arithmetical operations to broader systems of numbers (DEDEKIND *1854*). His later work, beginning at least by 1858,[47] lay stress on the definition of new numbers by means of sets,

45 The fact that abstract and topological considerations were intertwined in the work of CANTOR until the mid-1880s seems to reinforce this conclusion.

46 That broad edifice, which also included analysis and algebra, was called by him "arithmetic" (DEDEKIND *1888*, 335). This is coherent with the contemporary trend of 'arithmetization', of which DEDEKIND was an outstanding representative. The same extended conception of arithmetic can be found in KRONECKER (*1887*) and in SCHRÖDER (*1895/1905*, vol.1, 441).

47 The year in which he arrived at his typically genetic definition of the real numbers (cf. DEDEKIND *1872*, 316).

while the problem of the operations was regarded as secondary.

The notable step of introducing sets into the picture was hence done during the period 1854-58. Not surprisingly, this coincides with DEDEKIND's closer relation to RIEMANN, that developed from his attendance at the latter's lecture on function theory in the winter of 1855-56. It is thus very plausible that RIEMANN's conception of the foundations of the theory of magnitudes influenced DEDEKIND's step towards set-language. But DEDEKIND was an original mathematician working on fields very different from RIEMANN's, and so he followed his own path: it can be shown that DEDEKIND's mature set theory emerged from algebraic ideas of the late 1850s.

4.1. The algebraic origins of DEDEKIND's theory of sets.

Beginning in 1855, DEDEKIND's research concentrated on algebraic topics,[48] particularly on ABEL's and GALOIS' investigations on the general theory of equations. Result of it were his 1856-58 lectures on cyclotomy and higher algebra, the most notable part of which, his exposition of Galois theory, has been published under the title 'Vorlesung über Algebra' (DEDEKIND *1981*). In DEDEKIND's exposition of group theory, we can see how he tended towards an abstract conception of the subject, based on set-structural language and the consideration of sets as concrete mathematical objects. It was also in this connection that he began to see the crucial role that mappings play in mathematics.[49]

4.1.1. Groups and fields, sets and maps. DEDEKIND himself stressed that, in his 1856-58 lectures, he had presented group theory "in a such way that it could be applied to groups Π of arbitrary elements π" (DEDEKIND *1893*, 484 footnote). In fact DEDEKIND's original explanations are noteworthy. After proving two theorems about the product of 'substitutions', which establish its associativity and the law of simplification, he wrote:

The following investigations are exclusively based on the two fundamental theorems which we have proved, and on the fact

48 See DEDEKIND *1930/32*, vol.3, 403 and 414-415. It was also from 1856 that he began to devote his efforts "fundamentally" to algebraic number theory (DEDEKIND *1930/32*, vol.1, 110 and SCHARLAU *1981*, 37-38), obtaining some interesting results which he did not publish (HAUBRICH *1988* and *1992*).

49 Many of the points indicated in 4.1.1 and 4.1.2 have previously been dealt with by DUGAC (*1976, 1981*), but the reader will find that emphases and details are somewhat different. A major difference is my account of the origins of the notion of map in DEDEKIND's earlier 'substitutions'.

that the number of substitutions is finite:[50] therefore, their results will be equally valid for *any domain* of a finite number of *elements, things, concepts* θ, θ', θ''..., which from θ, θ' admit a composition θθ', defined arbitrarily but in such a way that θθ' is again a member of that domain, and that this kind of composition obeys the laws expressed in both fundamental theorems. In many parts of mathematics, but especially in number theory and algebra, we are continuously finding examples of this theory; the same methods of proof are valid here as there. (DEDEKIND *1981*, 63; emphasis added)

Some pages later, DEDEKIND applied this principle to the particular case given by the law of composition induced in the partition of a group by a normal subgroup (o.c., 68).

In the passage just quoted, DEDEKIND used the word 'domain' to refer to sets, while in some other paragraphs of the lecture he used 'complex' with apparently the same meaning. Moreover, in working with composition laws on the classes which form the partition of a group, he was employing sets as concrete objects, submitting them to operations that are analogous to the traditional ones. The same can be found in other manuscripts of the time.[51]

DEDEKIND's abstract conception of groups is most notable and surprising when we take into account that the mathematical community began to adopt it around 1890. It should be noted that DEDEKIND's view of groups appeared after RIEMANN had formulated his abstract conception of manifolds: as we have seen, RIEMANN's definition ensured that manifolds could be formed by any elements whatsoever. Since DEDEKIND most probably knew about RIEMANN's ideas, the conclusion is that a real influence is acting here.[52] We see how a methodological or even epistemological vision may explain the fact that a researcher anticipates, in some respects, by decades the general evolution of his subject.

DEDEKIND's algebraic studies led him also to the general notion of mapping.[53] This is obscured by the fact that he used a rather strange name for mappings, namely 'substitutions'. But the

50 This is correct, since the existence of neutral and inverse elements is a consequence of the two mentioned laws when we require the group to be finite. Modern axiomatization of groups began with an 1893 paper of H. WEBER, after LIE's work made new axioms for infinite groups necessary (WUSSING *1969*, 223-251).

51 In 'Aus den Gruppen-Studien 1855-1858' (DEDEKIND *1930/32*, vol.3, 439-445), and in a paper written in 1856, 'Abriss einer Theorie der höheren Kongruenzen in Bezug auf einen reellen Primzahl-Modulus' (DEDEKIND *1930/32*, vol.1, 40-66).

52 This is further reinforced by the importance for DEDEKIND of RIEMANN's abstract methodological principles (see FERREIRÓS *1992*). The relevant texts in this connection are a letter to LIPSCHITZ (in LIPSCHITZ *1986*, 59-60), and DEDEKIND *1895*, 54-55.

53 Explicit consideration of mappings was the most characteristic aspect of DEDEKIND's set theory as exposed in DEDEKIND *1888*. CANTOR considered one-to-one correspondences from the beginnings of his set-theoretical work, and he dealt with order-isomorphisms since 1883, but he never considered mappings explicitly as theoretical objects, nor did he use a general notion of mapping, even after having read DEDEKIND's work.

definition presented at the very beginning of his 'Vorlesung über Algebra' makes it clear that 'substitutions' are not simple permutations, but more generally transformations of a 'complex' of elements into any other. And DEDEKIND makes the explicit restriction that, for the rest of the lecture, both complexes will be identical (DEDEKIND *1981*, 60). If we disregard this restriction, it is clear that 'substitutions' are simply mappings. This fact is confirmed by a text written in 1879:

> It happens very frequently, in mathematics and other sciences, that when we find a system Ω of things or elements ω, each definite element ω is replaced by a definite element ω' which is made to correspond to it according to a certain law; we use to call such an act a substitution, and we say that by means of this substitution the element ω is transformed into the element ω', and also the system Ω is transformed into the system Ω' of the elements ω'. Terminology becomes somewhat more convenient if, as we shall do, one conceives of that substitution as a mapping [Abbildung] of the system Ω, and accordingly one calls ω' the image of ω, and also Ω' the image of Ω. (DEDEKIND 1879, 470)[54]

Internal reasons account for the fact that, in his algebraic researches, DEDEKIND only considered morphisms and not general mappings; the paragraph following our quotation introduces that restriction explicitly. More interesting, however, is how he came to consider mappings which were not bijective, and not even injective. This was a natural consequence of his studies of group homomorphisms, which appear in 'Aus den Gruppen-Studien 1855-1858'. Homomorphisms are not generally injective maps, and it is clear that DEDEKIND saw this point since he proved the theorem of isomorphism, considering the kernel of a group homomorphism.[55]

DEDEKIND's investigations of the late 1850s remained unpublished, and so his set-conception of algebra remained unknown until the appearance of his ideal theory in 1871. But his algebraic work of the 1850s shows already a definite tendency towards employing sets as basic, concrete mathematical objects. RIEMANN's ideas may have suggested this, but there is no doubt that it was the real value of such language for particular algebraic problems, that convinced

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54 A note to this text contains the first public announcement of DEDEKIND *1888*. Concerning the notion of mapping [Abbildung], he says: "On this mental faculty of comparing a thing ω with a thing ω', relating ω with ω', or making ω' correspond to ω, without which it is not at all possible to think, rests also, as I shall try to prove in another place, the whole science of numbers."

'Abbildung' is even today the German word for mapping, but it would be tempting to translate it by 'representation', which suggests some connotations of the German word and makes it understandable that DEDEKIND could consider such an operation as a basic mental faculty (see below).

55 DEDEKIND *1930/32*, vol.3, 440-442; here again, homomorphisms are called 'substitutions'. The relevance of general (non-injective) mappings was later reinforced by his discovery that the general theory of chains, including a generalized theorem of induction, does not require injective maps (DEDEKIND *1888*). This can already be found in a manuscript written between 1872 and 1878 (see DUGAC *1976*, 293-309).

DEDEKIND of its usefulness. Sets offered the possibility of dealing with mathematical questions in a way such that DEDEKIND's methodological preferences could be satisfied (on these methodological ideas, see FERREIRÓS *1992* and HAUBRICH *1992*).

DEDEKIND's use of set-language for dealing with groups was accompanied by an important novelty: in the context of Galois theory, he realized the usefulness of talking about fields, which during this period he called "rational domains" (cf. DEDEKIND *1981* and SCHARLAU's comments in his *1981*).[56] Again this amounted to the introduction of set-language, but it also involved an important new aspect: rational domains or fields, in DEDEKIND's sense, are always infinite sets. In this respect they differ from the finite 'complexes' or 'domains' that emerged in group theory (on the question of the infinite, see section V). DEDEKIND's familiarity with fields would later prove crucial in the context of algebraic number theory.

It was in this context that the notion of field was first published, and just one single paragraph of DEDEKIND's 1871 exposition of ideal theory is enough to show the maturity he had attained in operating with set-structures through set-operations and mappings. Thus the following text, with which DEDEKIND's first exposition of ideal theory began, condensed all the achievements of the 1850s that we have reviewed:

While we try to introduce the reader to these new ideas, we will place ourselves at a somewhat higher standpoint, and begin by introducing a notion which seems very appropriate to serve as a foundation for higher algebra and those parts of number theory connected with it.

1. We will understand by a *field* [Körper] every system of infinitely many real or complex numbers, which is so closed and complete in itself, that addition, substraction, multiplication, and division of any two of those numbers yields always a number of the same system. The simplest field is constituted by all rational numbers, the greatest field by all [complex] numbers. We call a field **A** *divisor* of field **M**, and this a *multiple* of that, if all the numbers contained in **A** are also found in **M**; it is easily seen that the field of rational numbers is a divisor of all other fields. The collection of all numbers simultaneously contained in two fields **A**, **B** constitutes again a field **D**, which may be called the *greatest* common divisor of both fields **A**, **B**, for it is evident that any divisor common to **A** and **B** is necessarily a divisor of **D**; similarly, there always exists a field **M** which may be called the *least* common multiple of **A** and **B**, for it is a divisor of all other common multiples of both fields. Moreover, if to any number a in the field **A**, there corresponds a number $b=\varphi(a)$, in such a way that $\varphi(a+a') = \varphi(a)+\varphi(a')$, and $\varphi(aa') = \varphi(a)\varphi(a')$, the numbers b constitute also (if not all of them are zero) a field **B**=φ(**A**), which is

56 It is noteworthy that, many years later, DEDEKIND would recommend to CANTOR the word 'domain' as an "also Riemannian" substitute for 'manifold' (CANTOR & DEDEKIND *1937*, 47).

conjugate to **A** and results from **A** through the substitution φ; inversely, in this case **A**=θ(**B**) is also a conjugate of **B**. Two fields conjugate to a third are also conjugates of each other, and every field is a conjugate of itself. (DEDEKIND *1871*, 223-224)

This passage should be considered as decisive in the history of sets: it is extremely rich, incorporating as it does all crucial ideas related to the notions of set and mapping as used in algebra (cf. DUGAC *1976*, 29). There is no doubt that it had to be difficult to understand for any reader of the time, since they were completely unaccustomed to any use of the notion of set in algebra. But for this very reason, it had to attract strongly the attention of readers to this new vision—whether to accept it or refuse it.

DEDEKIND's terminology might seem strange, but from his definitions it is clear that 'divisor' and 'multiple' mean the two sides of an inclusion; 'greatest common divisor' designs the intersection; and 'least common multiple' refers to the union, in the sense of the smallest structure that contains both. The selection of these expressions is easy to understand if we consider that DEDEKIND was dealing with algebraic number theory. In the case of **Z**, the inclusion of principal ideals corresponds strictly to the divisibility of their generators; this led him to employ the analogy between inclusion and division throughout the supplement on ideal theory. Moreover, his intention was to establish terminology in such a way that theorems could be formulated with exactly the same wording as in elementary number theory.[57]

There exists evidence that DEDEKIND's terminology was understood by at least one outstanding mathematician as related to the general notion of set. CANTOR used DEDEKIND's terminology for inclusion, union and intersection of fields in his decisive series of papers 'Über unendliche, lineare Punktmannigfaltigkeiten', from 1880 to 1884.[58] This is particularly striking because that terminology is rather inappropriate in a general set-theoretical setting such as the one to which CANTOR applied it—DEDEKIND himself replaced it in such a context (see below).

In the text on fields, DEDEKIND's treatment of 'substitutions' or mappings, and particularly those which transfer structure,[59] was equally a model. The fact that he is considering morphisms

57 For instance, the sense of 'divisor' and 'multiple', as defined for modules, are contrary to their counterparts for fields; the reason: to obtain the common wording for theorems. Regarding arbitrariness of terminology, see a letter to LIPSCHITZ from 1876 (LIPSCHITZ *1986*, 79), another to KEFERSTEIN from 1890 (SINACEUR *1974*, 274), and some passages of *Was sind und was sollen die Zahlen?* (DEDEKIND *1888*, 360, 377-378, 348).

58 The terminology is introduced in the second part of the series (1880; CANTOR *1932*, 145-146); the latest instances are found in the sixth part (1884; ibid. 214, 226, 228, etc.).

59 In later versions of ideal theory, DEDEKIND emphasized the need for restricting the general notion of mapping or 'substitution' in a way characteristic of algebra, by considering only mappings which preserve structure (see DEDEKIND *1879*, 470-471; *1893*, 456-

which may not be injective, is made clear by the comment on zero images: when he says that the numbers b constitute a field "if not all of them are zero", it means that his notion of mapping allows for the trivial case in which all elements of the original have 0 as their image. (We have seen that non-injective maps were already present in his 1856-58 'Gruppen-Studien'.) Moreover, the clarity with which the reflexive, symmetric and transitive properties of field 'conjugation' are set forth in the text deserves attention.

The example we have considered here is only that of fields, discussed by DEDEKIND in the first paragraph of his 1871 ideal theory, but the whole exposition insisted on the importance of set-structures for algebraic number theory. The very viewpoint adopted involved a constant exercise in the translation of problems stated in terms of numbers to new and more abstract set-formulations: DEDEKIND replaced KUMMER's ideal numbers with the kind of sets of numbers that he called 'ideals' (cf. section 4.2).

4.1.2. Sets and maps in a general setting. The year after the publication of this first version of ideal theory, DEDEKIND began to write the draft for his later book *Was sind und was sollen die Zahlen?*. The 1872 draft begins with definitions of some set operations (DUGAC *1976*, 293-294); he presented them in a general setting, abstracted from the structural restrictions that are necessary in an algebraic context. The word 'system' was defined in a way that undeniably pointed to the extensional notion of set, and the most characteristic part of DEDEKIND's algebraic terminology changed in accordance with the new abstract framework: the word 'divisor' was replaced by 'part' [Theil], meaning subset. Meanwhile, he maintained the expression 'least common multiple' for the union set, although he also wrote 'compound system', which was to become his final choice.

Nevertheless, set operations were by no means his primary interest. They were completely clear in themselves, and DEDEKIND had long been accustomed to them. The whole draft was primarily devoted to a study of the notions of mapping and chain, the second being based on the first. The term 'mapping' [Abbildung] appears also at the very beginning of the draft (DUGAC *1976*, 294), and is defined in its general sense. We will return to this essential part of *Was sind und was sollen die Zahlen?* later on.

The translation of set-structural operations to a general set-theoretical framework, which DEDEKIND sketched in his 1872 draft, reveals clearly what I have already stated: that DEDEKIND's mature set theory has its roots in his algebraic ideas, that emerged during the late 1850s.

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457).

It would be interesting to know about DEDEKIND's motivation for writing the 1872-78 draft. It probably was one or two of the following factors. In 1872, he published his theory of the real number system after learning for the first time about two other theories, those of WEIERSTRASS and CANTOR. The knowledge that some mathematicians had developed ideas similar to his own might have motivated him to publish his more general reflections on the foundations of the natural numbers, and arithmetic as a whole, since they established the general framework for his theory of the real numbers. CANTOR had also developed the notion of derived set (see CANTOR *1872*), which perhaps was even closer to DEDEKIND's viewpoint, since it used the notion of set. On the other hand, it was during the summer of 1872 that DEDEKIND made CANTOR's acquaintance, although unfortunately there is no record of the content of their conversations. One is left to speculate about the possibility that they discussed such matters as the (set-)foundations of the number system, and the Riemannian theory of manifolds.[60]

4.1.3. Critique of magnitudes. A striking difference between DEDEKIND and RIEMANN is the complete absence of the notion of magnitude in the former's views. While RIEMANN's conception of mathematics and manifolds was still rooted in the traditional idea of magnitude, DEDEKIND's mathematics are based on numbers, sets, and mappings.

DEDEKIND was perhaps the first mathematician who consciously avoided any kind of reliance on the traditional notion of magnitude. His reasons are clearly formulated in passages of his works on numbers: "I demand that arithmetic shall be developed out of itself" (DEDEKIND *1872*, 321), that is, "without any admixture of foreign ideas (such as that of measurable magnitudes)" (DEDEKIND *1888*, 338).[61]

DEDEKIND's thoughts on the matter are spelled out in his correspondence with LIPSCHITZ of 1876 (LIPSCHITZ *1986*; partly in DEDEKIND *1930/32*, vol.3, 469-479). Here it becomes clear that the problem with the theory of magnitudes was a lack of rigor. The notion of magnitude was ambiguous: there had been talk of 'named' and 'pure' magnitudes, depending on whether the magnitudes belonged to the real world of natural science, or were of a purely mathematical kind; it had even been customary to speak of 'variable magnitudes'. But magnitudes had never been rigorously defined, and so the theory was dependent on implicit, unwarranted assumptions. This affected above all the question of the continuity of the real number system. No author, from Euclid to the nineteenth century, had ever established a postulate that implied the

60 Concerning this and other episodes in the relations between CANTOR and DEDEKIND, see FERREIRÓS *1993*.

61 More generally, cf. DEDEKIND *1872*, 321-322; *1876/77*, 269; *1888*, 338-339.

continuity of the set of ratios between homogeneous magnitudes.[62] Instead, they had relied on the assumed *existence* of incommensurable magnitudes (DEDEKIND *1930/32*, vol.3, 476-477), making arithmetic depend on foreign matters.

Previously it was thought that the continuity of the number system is imposed by the characteristics of physical or geometrical magnitudes. In DEDEKIND's eyes, this made arithmetic lose its autonomy, and even its *a priori*, logical character. (The changes in geometrical thought, and above all RIEMANN's contribution, supported this interpretation, since geometry was no longer an *a priori* theory.) Breaking with such tradition, DEDEKIND emphasized the fact that numerical continuity was abstractly definable, and that only such abstract definition allows us to postulate the continuity of a domain of magnitudes, or of geometrical space (cf. also CANTOR *1872*).[63]

An autonomous development of the number system, "without any reference to the rather obscure and complex concept of magnitude" (DEDEKIND *1930/32*, vol.3, 476), became the antidote to those diseases. Apparently the mathematical community agreed that the measure was appropriate: by the beginning of the twentieth century, despite the frequent talk of magnitudes in the writings of influential mathematicians such as WEIERSTRASS and his school, the word 'magnitude' had basically disappeared from mathematical language.

4.2. The heuristic way towards ideals.

One further aspect of DEDEKIND's ideal theory is directly relevant to our subject. As we have seen in section II, during the 19th century sets were primarily conceived as logical classes correlated with concepts. It turns out that DEDEKIND's notion of ideal emerged from considerations in which the traditional concept/set relation was central.

This is at least what the introduction to a paper of 1876/77, 'Sur la théorie des nombres entiers algébriques', suggests. In it DEDEKIND described the path which led him to formulate the whole theory in terms of ideals; it was meant to be a historical description of the process of his own reflections, in which he "wrote each word only after the most careful reflection" (LIPSCHITZ *1986*, 59). We can be certain that any such account involves a measure of rationalization, but DEDEKIND's description is interesting as it shows his implicit assumptions, including underlying conceptions related to the notion of set, that he considered to have been central to the formulation of

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62 Cf. DEDEKIND *1930/32*, vol.3, 472-474, where DEDEKIND emphasizes this problem and outlines a rigorous development of the theory of magnitudes.

63 As we have seen, the idea that continuity was really a postulate, and not a fact, was central to RIEMANN's geometry (RIEMANN *1854*).

the notion of ideal.

Around 1860, DEDEKIND had tried to produce a completely general theory of the factorization of algebraic integers by generalizing KUMMER's approach in a rather direct way, through higher congruences. This approach turned out to be unsatisfactory, because there always appeared exceptions, numbers whose decompositions could not be obtained by means of higher congruences.[64] DEDEKIND let the problem sleep for some years, until around 1869, when he began work on the second edition of *Vorlesungen über Zahlentheorie*. The result of his renewed efforts was a completely new viewpoint: the factorization problem was formulated in set-structural terms. DEDEKIND emphasized the importance of this change:

I have not arrived at a general theory without exceptions [...] until having abandoned completely the old, more formal approach, and having replaced it by another which departs from the simplest basic conception, and fixes the eyes directly on the end. Within this approach, I do not have any more need of new creations, as that of Kummer's *ideal number*, and it is entirely sufficient to consider the *system of really existing numbers* which I call an *ideal*. The power of this notion resting on its extreme simplicity, and being my wish to inspire confidence in this concept, I shall try to develop the series of ideas which led me to this notion. (DEDEKIND *1876/77*, 268)

KUMMER had not defined ideal numbers in themselves; he just defined divisibility by ideal numbers. That is, in KUMMER's restricted theory, and in DEDEKIND's first approach, ideal numbers were introduced through one or more congruence relations, which determined when an algebraic integer was to be considered as divisible by the ideal number (cf. EDWARDS *1980*). It is a small step to consider the congruences associated with each ideal number as its defining concept or property, which may or may not be satisfied by an integer; DEDEKIND took this step.

DEDEKIND's aim was to produce a general definition of ideals, making a precise definition of their multiplication possible. To this end, it would be necessary and sufficient to determine what is common to all those properties *A, B, C*... which are used to define the different ideal numbers, and to establish how properties *A, B* associated with two ideal numbers determine the property *C* which defines their product (DEDEKIND *1876/77*, 268-269). At this point, the relation between concept and class came into play, since the stated task is "essentially simplified" when we consider, not the property *A* itself, but the class, set, or system which it determines:

As such a characteristic property *A* serves to define, not the ideal number in itself, but only the divisibility of numbers

64 See DEDEKIND *1930/32*, 202-230, and HAUBRICH *1988* and *1992*.

contained in [the ring of integers] **o** by an ideal number, one is naturally led to consider the system of *all* those numbers α in domain **o** which are divisible by a certain ideal number; from now on I shall call such a system **a**, for short, an *ideal*, and so to each particular ideal number corresponds a certain *ideal* **a**. But as, reciprocally, the property *A* [...] consists only in α's belonging to the corresponding ideal **a**, one may, instead of properties *A, B, C*... [...], consider the corresponding ideals **a**, **b**, **c**... in order to establish their common and exclusive character. (DEDEKIND *1876/77*, 270)

This step towards set language, which DEDEKIND regarded as 'natural', was difficult and strange for his contemporaries.[65] What made it natural for DEDEKIND were, undoubtedly, two factors: his familiarity with the traditional logical conceptions, that established the concept/set relation; but above all his confidence in the set-theoretical approach to arithmetic and mathematics generally. Without the latter, the connection between those traditional conceptions and mathematics would not be clear at all; this, and the changes in method implied by DEDEKIND's step, explain why the innovation was difficult to accept.

The next task was to establish a definition of ideals, i.e., to determine necessary and sufficient conditions for a set of integers to be an ideal. Through a consideration of principal ideals (sets of integers divisible by a given, 'existing' integer), DEDEKIND found two conditions apt for a general definition. An ideal was defined to be a *system* or set of algebraic integers in a field **K**, such that (DEDEKIND *1871*, 251; *1876/77*, 271):

I. the sums and differences of any two numbers in the ideal are again numbers belonging to it, and
II. any product of a number in the ideal by an integer of the field is again a number belonging to the ideal.

Finally, DEDEKIND took pains to prove that any so-defined ideal was either the set of multiples of an integer, or of an ideal number in the old sense.

This train of thought led him "naturally" to base the whole theory of algebraic integers on "this simple definition, entirely freed from any obscurity and the admission of ideal numbers" (DEDEKIND *1876/77*, 271-272). DEDEKIND had a rather strict conception of arithmetic and how to treat it, including the requirement that definitions should be appropriate to base upon them a rigorously deductive theory. It is clear that the definition of ideals satisfied his preferences completely, and this seems to have been the reason why he adhered to it, despite its novelty and the

65 This is one of the reasons why ideal theory was not generally accepted until the 1890s (see HAUBRICH *1992*).

difficulties it posed to the understanding of contemporary readers.

4.3. The logical theory of systems and mappings.

The foregoing may suffice for the reader to obtain a grasp of the sense in which sets were at the basis of ideal theory. Ideal theory was genetical or "constructive" in DEDEKIND's sense: taking the set of algebraic numbers **A** as given, it employed set-constructions on it, i.e., subsets of **A** (the ideals), as its basic objects; and this made it possible to define rigorously the operations on ideals, especially multiplication. In this way, it was perfectly coherent with DEDEKIND's conception of the number system, to which we now have to turn.

4.3.1. DEDEKIND's program for the foundations of mathematics. In his works on the foundations of the number system, DEDEKIND expressed repeatedly the idea that this system was obtained from the natural numbers through step-by-step definitions or "constructions" (DEDEKIND *1854*, 430-431; *1872*, 317-318; *1888*, 338). In manuscripts that are still preserved, he developed the well-known idea of defining the integers and their operations on the basis of equivalence classes of pairs of natural numbers; and similarly for the rationals on the basis of equivalence classes of pairs of integers. In 1872 he published the much more sophisticated idea of employing so-called 'DEDEKIND cuts' on the set of rational numbers for defining the real numbers and the operations on them. In all of these cases, the procedure is genetical in the above sense: taking a set of numbers as given, the next higher set is defined by means of set-constructions on the former, i.e., it is defined as (isomorphic to) the set of some specified subsets of the former. This, and the operations on the 'lower' number-set, suffice to define the operations on the new numbers. Since, according to DEDEKIND's reliable dating, his theory of real numbers was formulated in 1858, it is a mild assumption that the much simpler theories of the integers and rationals should be traced back, at least, to that same year.[66]

According to the foregoing, the arithmetical framework that embraces both the number system and ideal theory is that of a series of set-constructions which have the natural numbers as their basis. Natural numbers with their operations on the one hand, and sets and mappings on the other, are the basic elements of arithmetic. At some point in time, however, DEDEKIND was able to simplify this conception essentially; he discovered that the natural numbers and their basic operations could be defined by means of sets and maps alone. Thus sets and mappings alone are a sufficient foundation for arithmetic. This is the main thought that finds its detailed expression in the

66 Here I skip over the details of the evolution of DEDEKIND's views on the matter, which I plan to discuss in a new article.

draft of *Was sind und was sollen die Zahlen?*, which was written sporadically from 1872 to 1878. By 1872, sets and mappings had come to be, in DEDEKIND's mind, the only indispensable tools for the whole of arithmetic (see especially DUGAC *1976*, 293).

As I said earlier, by 'arithmetic' DEDEKIND understood a very broad discipline. It can be described as the most general theory of numbers and their operations and functions, based on the theory of sets and mappings. Beginning with the natural numbers, the edifice of 'arithmetic' raised to embrace number theory, algebra, and analysis (DEDEKIND *1888*, 335). As regards the latter two branches of arithmetic, it is not difficult to imagine how they fit into DEDEKIND's ambitious scheme. Set-constructions on the systems of rational and complex numbers (and intermediate systems, like those of algebraic numbers) allow us to define the algebraic structures that DEDEKIND introduced; and mappings make it possible to consider morphisms between those structures.[67]

A reconstruction of DEDEKIND's thoughts on analysis is necessarily more tentative, because to the best of my knowledge there is no document recording them. But since the real and complex numbers can be defined within DEDEKIND's framework, and since we have the general notion of mapping at our disposal, real and complex functions are easily obtained. It might well have been along this line that DEDEKIND saw analysis integrated within the general picture of 'arithmetic'—meaning his conception of classical mathematics as based on numbers, and ultimately on sets and maps.

DEDEKIND's foundational masterpiece *Was sind und was sollen die Zahlen?* (1888) was devoted to a detailed presentation of the elements of the whole edifice of his 'arithmetic'. It developed the "construction" of the natural numbers on the basis of a careful presentation of the theory of sets and mappings. But above all, it has to be read as presenting all the necessary ingredients for a detailed derivation of arithmetic, algebra, and analysis.

4.3.2. Set and mapping as logical notions. In the preface to his 1888 book, DEDEKIND clearly adopted a logicist viewpoint. The work begins with an statement of the author's epistemological and methodological views:

In science nothing capable of proof ought to be accepted without proof. As evident as this requirement might seem yet I cannot regard it as having been met even in the simplest science, that *part of logic* which deals with the theory of numbers [...]

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67 This was enough to recover the relatively concrete conception of algebra prevalent in DEDEKIND's time. In particular, DEDEKIND presented Galois theory as dealing with groups of automorphisms in DEDEKIND *1893*.

> In speaking of arithmetic (algebra, analysis) as a part of logic I mean to imply that I consider the number-concept entirely independent of the ideas or intuitions of space and time, that I consider it an *immediate result from the pure laws of thought*. [...] It is only through the *purely logical construction* of the science of numbers and by thus acquiring the continuous number-domain that we are prepared accurately to investigate our notions of space and time by relating them to this number-domain created in our mind. (DEDEKIND *1888*, 335; emphases added)

The motivation for this confident affirmation of logicism lay in the fact that, according to DEDEKIND's foundational program, all of (pure) mathematics can be reduced to the notions of set and map, which in their turn are simply logical notions.

We do not have any detailed argument by DEDEKIND on the logical character of sets, which is understandable since this could be considered as supported by tradition—see section II. But we do know that he accepted this tradition. In a second draft of the book, written in 1887, its section 1 carried the title: "Systems of elements (Logic)".[68] This clearly means that sets belong to logic, and even as late as 1897 DEDEKIND still referred to set theory as the "logical theory of systems [Systemlehre der Logik]" (DEDEKIND *1930/32*, vol.2, 113). Incidentally, DEDEKIND never claimed it possible to reduce logic to the theory of sets and maps alone.

Traditionally, logic had been considered to deal with the most general laws of thought, that apply to any subject or discipline. In accordance with this, DEDEKIND's book begins establishing the most general scope for set theory: he defines a "thing" to be "any object of our thought", and states that any "thing" may belong to a "system" or set. In his definition of systems, DEDEKIND emphasizes what we call the axiom of extensionality: two systems are identical if and only if they contain the same elements. Moreover, systems are again things, so that the process of forming sets of sets is indefinitely open (DEDEKIND *1888*, 344-345).[69] All of this shows that the logicist position advanced in the book's preface was perfectly coherent with the theory itself.

DEDEKIND's viewpoint opened the way to the appearance of antinomies, since the whole book considers, sometimes implicitly, a universal set conceived—in the same general, 'logical' spirit of the explanations about things and systems—as "my mental universe [Gedankenwelt], i.e., the totality of all things which can be objects of my thought" (DEDEKIND *1888*, 357). Nevertheless, it is notable that DEDEKIND did not establish any axiom of comprehension, as was done by FREGE (*1893*, 35-36). FREGE's axiom of comprehension should be regarded as the highest expression of

68 *Nachlass DEDEKIND* III, 1, III, p. 2. In the book, the title is the same, except for the part in parentheses.

69 It is noteworthy that similar definitions—including that of 'thing'—can be found in the 1872-78 draft (DUGAC *1976*, 293), which seems to imply that by 1872 DEDEKIND was already a logicist.

the traditional relation between concepts and sets, which played an important role in RIEMANN's view and also in DEDEKIND's way towards ideals. That relation is still clearly visible in DEDEKIND's 1872-78 draft, but in the 1888 book it only appeared in vestiges. This step led him to present a more modern theory, one for which the effect of the antinomies was not so obvious as for FREGE's.[70]

Unlike the statement that sets are just a part of logic, based on traditional conceptions, DEDEKIND's idea that mappings are also purely logical was novel. It is natural, therefore, that he showed an interest in convincing the reader that this was true. The relevant passage can be found in the preface to his book, immediately after the above-quoted text in which the theory of numbers is called a "part of logic":

> If we scrutinize closely what is done in counting a quantity or number of things, we are led to consider the faculty of the mind to relate things to things, to let a thing correspond to a thing, or to represent [abbilden: map] a thing by a thing, a faculty without which no thinking at all is possible. Upon this unique foundation, which is absolutely indispensable in any case, must in my opinion, as I have already affirmed in an announcement of this paper,[71] the whole science of numbers be established. (DEDEKIND *1888*, 335-336)

Since maps are the foundation of the theory of numbers, and this is only a part of logic, DEDEKIND clearly implies that the notion of map is purely logical. To show it, he resorts to the conception of logic as the science of the laws of thought: he notes that the ability to correlate things to things is an indispensable prerequisite of thinking─he may have had in mind the role of words, or some other form of representation of external objects, in thought.

The reader should recall here that DEDEKIND's term for 'mapping' was 'Abbildung', the word still used in German for that purpose. It is a compound from 'Bild', which means image or figure, the result of copying something, but also a mental idea: the 'Abbildung' of an object can be not only its pictorial representation, but also its idea in somebody's mind. The term, therefore, has a broad meaning that is coherent with the presumed logical nature of the notion of mapping. If it did not run against a well-established tradition, it might be better to translate 'Abbildung' into 'representation', which would make DEDEKIND's logicist position easier to understand.

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70 In my opinion, the reason for this change was DEDEKIND's adherence to a methodological principle related to RIEMANN: he always tried to avoid particular forms of representation that are arbitrary to some extent, i.e., that are not intrinsically determined (see FERREIRÓS *1992*). Since two different concepts may define what extensionally is the same set, it was preferable to refer to sets in an abstract way, avoiding explicit reference to defining concepts.

71 This refers to an 1879 text quoted above (footnote 54).

The theory of sets and mappings developed by DEDEKIND should be considered as an elementary general set theory, as opposed to CANTOR's transfinite set theory. It was a notable contribution to the study of abstract sets, which established basic results on both finite and infinite sets, based upon the first deductively useful definition of the infinite (see section V). DEDEKIND studied finite sets on the basis of infinite sets, overturning what had been considered the 'natural' relation between both. DEDEKIND's was the first deductive presentation of set theory, and—as has been repeatedly mentioned—it played a role in the birth of axiomatic set theory through its influence on ZERMELO. On the other hand, DEDEKIND did not present any transfinite result, not even general results that he had obtained and were relevant to CANTOR's theory (see Ferreirós *1993*); nor did he indicate how his exposition connected with CANTOR's preceding papers.[72]

DEDEKIND's treatment of sets is notable for its deductive character, and for being more succinct and systematic than anything written until then, but otherwise it is not particularly interesting. He presented the inclusion relation (introducing a symbol for it), and the operations of union and intersection of a possibly infinite family of sets; CANTOR had presented this extension of the basic operations to an infinite family of sets in a paper of 1880. Methodologically characteristic is the fact that he derived all basic results needed, including laws of associativity, and analyzed systematically the interrelations between the diverse notions introduced. But he did not study more interesting operations, such as power set formation, or Cartesian products.

DEDEKIND's terminology and above all his notation were somewhat inconvenient, as FREGE later showed, but the latter's criticisms regarding DEDEKIND's notions were for the most part unfair. DEDEKIND was perfectly aware of the distinction between inclusion and the belonging relation. He did not distinguish them in his notation because he admitted the formation of a unitary set from any element *a*, denoting it with the same letter *a*. This does not mean that he confounded a single element with its unitary set, quite the contrary: he was very aware of the 'dangers' that his notation involved.[73] Another feature criticized by FREGE is the fact that in his book DEDEKIND

72 In fact, DEDEKIND eliminated intentionally one reference to CANTOR that appeared in his drafted preface. It seems that the strains between both mathematicians were the reason why he did not clarify the relation between their theories (see FERREIRÓS *1993*). Nevertheless, DEDEKIND's book would seem to have established what he himself considered as a more rigorous foundation for CANTOR's contributions, and there are places in which a reference to CANTOR would have been only natural. For instance, at some point DEDEKIND considers set equipollence, and after showing its reflexive, symmetric, and transitive properties, he indicates the possibility of forming equipollence classes. Since he had established no cardinality restriction, he might well have referred to CANTOR's notion of power (DEDEKIND *1888*, 351). The same might have happened when he spoke about the definition of cardinality itself, which only for the sake of simplicity was restricted to finite cardinals (ibid., 387 footnote).

73 See DEDEKIND *1888*, 345, an 1888 letter to H. WEBER (DUGAC *1976*, 273), and the manuscript 'Gefahren der Systemlehre'—written probably around 1900—in SINACEUR *1971*. In all of these places he makes clear that such a confusion leads to plain contradictions.

excluded the empty set, saying that for other researches it could be "comfortable" to "imagine" it. FREGE was troubled by this negligent way of talking, but this is above all a peculiarity of FREGE. In fact, DEDEKIND had already introduced the empty set in his 1887 draft of the book, and he did it just as FREGE would have wanted, through a reference to contradictory concepts: "A system may consist of *one* element (that is, of a *single* one, one and *only* one), may also (contradiction) be *empty* (contain no element)" (*Nachlass DEDEKIND* III, 1, III, p. 2).[74]

The main peculiarity of DEDEKIND's theory, as compared with earlier work, including CANTOR's, is the notion of mapping.[75] DEDEKIND defines a mapping as an arbitrary "law" according to which every element of a set (original) is correlated with some other thing (image) (DEDEKIND *1888*, 348). His treatment of mappings was perfectly general, including the identity map, images of subsets of the original, restriction of maps to subsets of the original, and mapping composition. The terminology was well-chosen and became universally accepted, except for the term used by DEDEKIND for injective maps. Here, the terminology was again chosen in accordance with his logicist standpoint: as his mappings were 'representations' [Abbildungen], injective maps were called "similar or clear representations" (DEDEKIND *1888*, 350). When speaking about 'similarity', DEDEKIND sometimes implies bijectivity; apparently he regarded the restriction of the final set to the image as trivial. Thus, DEDEKIND's "similar systems" (ibid. 351) are a set and its image under an injective map, which are equipollent; we would have talked about bijective maps here. Similar representations, or injective maps, were the basis for his crucial definition of the infinite.

To DEDEKIND, the notions of set and mapping were indispensable for the development of arithmetic, regardless of the foundational viewpoint that we may adopt (see DUGAC *1976*, 293). We can assume that this seemed to him supported by both his development of the number system and his ideal theory. Moreover, sets and maps turned out to suffice for establishing a rigorous and general foundation for mathematics. And they were basic logical notions. (Below I consider the good reception that this proposal found.) They served to formulate the basic properties of numbers in such a way that they became subordinate to more general concepts—to the most general indeed:

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74 Likewise, DEDEKIND used the empty set in a manuscript written in 1889 (DEDEKIND *1930/32*, vol.3, 450-460).

75 Of course, CANTOR had considered several kinds of mappings: bijective mappings for cardinality theorems; order isomorphisms beginning in 1883; and even—in 1895—'coverings', which in fact are general mappings. But he used different names for these notions (resp. 'Äquivalenz', 'Abbildung', 'Belegung'), and never considered them under a common general notion, even after having read DEDEKIND. In fact, CANTOR did not consider the notion of mapping explicitly: most of his work deals with it, but never as a thematic object of his theories.

to logical notions.[76] The resulting conception gave a neat epistemological account of the nature of mathematics, and implied the need to follow a strictly deductive methodology, that had appealed to DEDEKIND since the beginning of his career. All of this helps, in my opinion, to clarify the way in which the logicist viewpoint could appeal to 19th and early-20th century mathematicians.

4.3.3. DEDEKIND's deductive method. As I have mentioned, DEDEKIND's theory of sets is notable for its deductive structure. In keeping with his motto "in science nothing capable of proof ought to be accepted without proof", DEDEKIND took pains to demonstrate all kinds of results that his colleagues regarded as evident. This brought him criticisms from many of them. During a visit to Berlin in 1888, HILBERT noted down the negative reaction to DEDEKIND's book "in all mathematical circles, among the young and the older" (DUGAC *1976*, 93); for instance, Paul DU BOIS-REYMOND considered the book to be "horrendous" (DUGAC *1976*, 203 note).

In the long run, DEDEKIND's contribution found a more positive response. It is well known that his theory was important in ZERMELO's search of a set-theoretical axiom system during the 1900s.[77] To some extent, indeed, DEDEKIND's work is a natural precedent for the modern axiomatic method. His theories intended to satisfy the following requirement:

in replacing all technical expressions by newly invented arbitrary words (that until then lacked any meaning), the edifice, if well constructed, should not collapse, and for instance I affirm that my theory of the real numbers would bear that test (DEDEKIND *1930/32*, vol.3, 479; also LIPSCHITZ *1986*, 79)

If DEDEKIND said this about his theory of the real numbers, it must be true all the more of his 1888 book, since this exhibits a rigid deductive structure, and establishes the general framework in which the definition of the real numbers can be set up. In 1890 he stated that his theory of sets and the natural numbers seemed to him an "edifice constructed according to the canons of the art, perfectly compact in all its parts, unshakable" (SINACEUR *1974*, 259-260).

But in other respects DEDEKIND's deductive method seems rather strange, and could even be called anti-axiomatic. Against EUCLID's classical model, later followed by HILBERT and his school, in DEDEKIND's book we find no postulate or axiom, only definitions and theorems. The whole theory is derived exclusively from definitions of set, union, intersection, map, etc. In my

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76 See DEDEKIND's 1890 letter to KEFERSTEIN (SINACEUR *1974*, 272, or its English translation in VAN HEIJENOORT *1967*, 99-103).

77 Cf. Emmy NOETHER's comments in DEDEKIND *1930/32*, vol.3, 390-391, also MOORE *1978* and *1982*.

opinion, this peculiarity is intimately related to DEDEKIND's logicist convictions, and to his conception of logic.

The traditional conception of axioms regarded them as true propositions that do not admit of a proof. To the influential Kantian epistemology, axioms were "synthetic *a priori* principles, insofar as they are immediately true" (KANT *1787*, 760). Since logic was a purely analytical science, in which no synthetic principle plays a role, it was radically foreign to the use of axioms. This Kantian conception can still be found in the work of the German mathematical logicians SCHRÖDER (cf. *1890/1905*, vol.1, 441) and FREGE. It was for that reason that FREGE did not talk about arithmetical 'axioms', but about the 'fundamental laws' [Grundgesetze] of arithmetic (FREGE *1893/1903*). Similarly, DEDEKIND seems to have thought that the logicist program demanded the development of arithmetic in a rigorously deductive way, without any recourse to axioms.

The idea that a deductive theory can be based on definitions alone was not completely unknown in this period. In SCHRÖDER's first logical work, devoted to the Boolean calculus, we can read the following:

> All theorems of our discipline are intuitive; as soon as they are brought to conscience, they appear as immediately evident. Therefore, the statements that we introduce here as axioms could also justifiably be presented as consequences that are immediately given with the definitions. (SCHRÖDER *1877*, 4)

SCHRÖDER preferred to base the logical calculus on axioms, perhaps due to the influence of Hermann GRASSMANN, while DEDEKIND's was the opposite choice. DEDEKIND made a conscious effort to derive all immediate consequences of his definitions that would be needed later on.

In a word, it seems that for DEDEKIND a theory can only be judged strictly logical when its propositions follow from basic logical *notions*, like those of set and map, without the use of any axioms. In this way, DEDEKIND's theory can be considered as strictly deductive but non-axiomatic—definitional instead. From a modern viewpoint, this has to be considered a drawback, since the axiomatic treatment allows better control of the theory.

V. INTERLUDE: THE QUESTION OF THE INFINITE

It has frequently been pointed out that, until the time when CANTOR championed the actual infinite, it had been traditionally rejected by all mathematicians—a tradition that is traced back to ARISTOTLE, and which counts GAUSS and CAUCHY as 19th century examples. In this section, however, we will see that this generalization does not hold water against the views held by RIEMANN and DEDEKIND from the 1850s.

In fact, when we consider more closely the situation in 19th century Germany, there are good reasons to reject that generalization. At the beginning of the century, in the time of idealism, the potential infinite of mathematics was called the "bad infinite" by HEGEL and his followers (see BOLZANO *1851*, 7). The implication was clear: there is a good infinite, that is actual in the highest sense: the Absolute. It is well-known that idealism affected an important sector of German scientists, and among them we find some mathematicians; the best example I know is that of the Berlin professor Georg STEINER (see his *1832*).

But idealism was not all in German philosophy, nor the only trend that affected scientists. An important characteristic of German philosophy and science during the 19th century was the influence of LEIBNIZ, and LEIBNIZ was an outstanding defender of the actual infinite.[78] It is not by chance that both BOLZANO (*1851* frontispiece) and CANTOR (*1883*, 179) quoted the following text by LEIBNIZ:

> I am so much for the actual infinite, that instead of admitting that Nature abhors it, as is vulgarly said, I defend that it affects her everywhere, in order to better mark the perfections of her Author. And so I believe there is no part of matter which is not, I do not say divisible, but actually divided; and consequently the least particle must be considered as a world full of an infinity of different creatures.

Even HERBART, who in his later works argued against the actual infinite (cf. CANTOR *1932*, 392-393), talked early in the century about the service that metaphysics had made to mathematics by eliminating the aversion of the notion of infinity.[79]

In the group of Leibnizians we can also count the Königsberg professor of mathematics Johann SCHULZ, who was a friend of KANT's and developed a mathematical theory of the infinitely great (see SCHUBRING *1982*). And we should also consider here scientists (not mathematicians) that under LEIBNIZ's influence presented revised versions of the theory of monads, defending the idea that the number of monads was actually infinite. The most important examples are the scientist-philosophers Gustav Theodor FECHNER (see his *1864*) and Hermann

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78 At least in some of his writings, notably in the influential *Monadologie* (LEIBNIZ *1714*), though not in some other works, such as the *Nouveaux Essais*. On this question, cf. KÖNIG *1990*.

79 HERBART *1964*, vol.1, 174. Such aversion had impulsed mathematicians to teach "in strange ways, *without* that fundamental concept, that which *only* through it was accessible to the *discoverer* himself" (probably meaning LEIBNIZ and the calculus).

LOTZE (cf. *1856/64*).

These examples taken from 19th century mathematicians and scientists suffice to show that the customary generalization on the rejection of the actual infinite is far from true. The existence in the 1870s of a group of theologians (GUTBERLET, FRANZELIN) that accepted the actual infinite, and that were important for CANTOR, points to the same (cf. MESCHKOWSKI *1967*, PURKERT & ILGAUDS *1987*). But for our purposes, the most important cases are those of RIEMANN and DEDEKIND. Since the acceptance of the actual infinite could be proposed as a criterion for a mature, mathematically interesting set theory, the question treated here might seem crucial for the purposes of this article.

5.1. A reconstruction of RIEMANN's viewpoint.

Like LOTZE and FECHNER later, and like his philosophical master HERBART, RIEMANN accepted a version of LEIBNIZ's monadology: the ontology of the 'Realen' that HERBART had elaborated (see SCHOLZ *1982*). This is strong evidence for a positive position towards the infinite on his side. On this point, however, RIEMANN's mathematical texts are not so clear as those of DEDEKIND that we will see below.

In speaking of continuous manifolds as sets of points, RIEMANN was implicitly accepting completed infinities, but this does not go beyond what was traditionally common in geometry. More interesting information on his views is to be found in one of his philosophical texts, called 'Antinomien', and in a passage of his 1854 lecture.

Beginning with the latter, in a surprising passage RIEMANN assumes the existence of infinite-dimensional spaces, giving two examples: the different possible functions on a given domain, and the possible configurations of a spatial figure. This might imply an acceptance of the actual infinite, although the evidence is not unequivocal. Incidentally, the text seems to show that RIEMANN did not identify the discrete and the finite: he distinguishes "an infinite series" from "a continuous manifold" (RIEMANN *1854*, 258), probably because infinite series are discrete but infinite manifolds.

RIEMANN's acceptance of the infinite is proven, I think, by his philosophical text 'Antinomien'. In it, he presents several pairs of contradictory ideas as thesis and antithesis, no doubt following the famous example of KANT's *Kritik der reinen Vernunft*. RIEMANN's theses are presented under the common heading "finite, representable", and the antitheses under "infinite, conceptual systems which lie on the borders of the representable" (RIEMANN *1892*, 518-519). And a general comment on the relation between thesis and antithesis enables us to reconstruct his position. RIEMANN indicates that under the latter we find "concepts which are well determined by means of negative predicates, but are not positively representable" (ibid.). Thus the notion of the infinite is well defined, and therefore seems to be completely acceptable.[80]

5.2. DEDEKIND and the infinite.

DEDEKIND's position was much more explicit, since he formulated a definition of the infinite, and based on it his whole theory of sets and natural numbers. This definition was first published in 1888, after CANTOR's pathbreaking contributions, but it can be found at the very beginning of DEDEKIND's 1872-78 draft of his later book (see DUGAC *1976*, 294). According to this, it has to be dated to 1872, and therefore it precedes CANTOR's papers on set theory, including his crucial non-denumerability result obtained late in 1873. Unfortunately, as far as I know there exist no other indications regarding the origins of DEDEKIND's definition.[81]

There is much independent evidence for DEDEKIND's acceptance of the actual infinite at this time: both his 1871 ideal theory, and his 1872 theory of the real numbers, explicitly employ infinite sets. But since the theory of real numbers was first formulated in 1858, it might seem that DEDEKIND was accepting the actual infinite already in the 1850s. This is corroborated by some others texts, and again it is coherent with the plausible influence of RIEMANN's views on him.

5.2.1. The roots of DEDEKIND's infinitism. The dependence of the theory of real numbers on infinite sets has, in fact, been proposed as the reason why DEDEKIND took the side of the actual infinite, and why he formulated his ideal theory in an infinitistic version.[82] This, however, does not seem to be very plausible. First, DEDEKIND's acceptance of the infinite does not have the appearance of a more or less *ad hoc* position, adopted to safeguard his theory of the real numbers. It rather looks like a deep-rooted conviction: infinite sets are perfectly acceptable objects of thought, that involve no contradiction, and that play a crucial role in mathematics; this is why DEDEKIND took the side of the actual infinite.

Second, DEDEKIND placed no special value on his theory of real numbers: he regarded it as a rather straightforward contribution that simply came to fill a gap in the elements of the number

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80 It was precisely RIEMANN's friend, DEDEKIND, who showed how we can define directly the infinite, leaving the finite as the non-infinite. Judging from the sentence above, this should have greatly surprised RIEMANN.

81 Since in the 1872-78 draft it is formulated from the start in its final form, the possibility remains open that the definition might have been obtained some years earlier.

82 EDWARDS *1983*, 16-18. It has to be said that EDWARDS is a convinced constructivist, and this viewpoint, which he has never hidden, undoubtedly influences his perception of the historical issues at stake.

system; other mathematicians would have formulated something similar had they devoted some effort to it (DEDEKIND *1930/32*, vol.3, 470, 475). Therefore, it is highly implausible that he might have conditioned his cherished ideal theory on a trait of the much less valuable theory of real numbers.

Third, it is even possible to find texts written by DEDEKIND prior to 1858, i.e., prior to the formulation of his theory of the real numbers, in which he employs infinite sets. This is what happens in a paper written in 1856 and published the following year, where DEDEKIND presented a theory of higher congruences.[83] We saw above that around this time DEDEKIND was talking about finite sets in the context of group theory, and he also introduced (infinite) fields under the name of 'rational domains'. The paper on higher congruences gives independent corroboration of his use of sets, but above all it is interesting for it shows that he accepted *infinite* sets and equated them with the most concrete objects of traditional mathematics. The relevant text says:

The preceding theorems correspond exactly to those of number divisibility, in the sense that the whole system of infinitely many functions of a variable, congruent with each other modulo *p*, behaves here as a single concrete number in number theory, for each function of that system substitutes completely any other in any respect; such a function is the representative of the whole class; each class possesses a definite degree, certain divisors, etc. and all those characteristics correspond in the same manner to each particular member of the class. The system of infinitely many incongruent classes—infinitely many, since the degree may grow indefinitely—corresponds to the series of whole numbers in number theory. To number congruence corresponds here the congruence of classes of functions with respect to a double modulo, [...] (DEDEKIND *1930/32*, vol.1, 47)

It should be noticed how DEDEKIND stresses the fact that he is considering infinitely many classes, *each of which contains infinitely many elements*. Despite this character of actual infinity, he compares those classes with the natural numbers, most concrete objects for a traditional mathematician.

Fourth and finally, in the only text that I have been able to find where DEDEKIND tried to justify his use of infinite sets in ideal theory, he establishes no relation between this and the real numbers. Instead, he traces an interesting parallel between his work and GAUSS's theory of the composition of quadratic forms in his famous *Disquisitiones Arithmeticae* (1801). In a letter to LIPSCHITZ of June 1876, DEDEKIND wrote:

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83 'Abriss einer Theorie der höheren Kongruenzen in Bezug auf einen reellen Primzahl-Modulus' (1857), in DEDEKIND *1930/32*, vol.1, 40-66.

As we can conceive of a collection of *infinitely many* functions—which are still dependent on variables—as *one* whole, for instance when we collect in a form-class all equivalent forms, denote this by a single letter, and submit it to composition, I can with the same right conceive of a system A of infinitely many, completely determined numbers of **O**, which satisfies two extremely simple conditions I. and II., as *one* whole, and name it an ideal [...] (LIPSCHITZ *1986*, 62)

While speaking about classes of quadratic forms, GAUSS had been very careful to express himself in a way that did *not* imply the existence of actual infinities. But it is clear that his reader DEDEKIND was not worried by philosophical subtleties; from the 1850s on, he accepted actual infinities as natural mathematical objects, perfectly sound and consistent in themselves.

5.2.2. The definition and the theorem of the infinite. DEDEKIND's definition of the infinite was made possible by the consideration of mappings, and particularly of injective mappings or 'similar representations'.

A system *S* is called *infinite* when it is similar [equipollent] to a proper part of itself (32); in the opposite case *S* is said to be a *finite* system. (DEDEKIND *1888*, 356)

In a footnote to this definition, DEDEKIND affirmed that it "forms the core of my whole investigation", and that he had communicated it to several mathematicians (SCHWARZ and H. WEBER in the 1870s, CANTOR in 1882).

The definition should have been quite surprising for DEDEKIND's contemporaries: far from defining the infinite as that which cannot be counted, that is, through a negative property (as RIEMANN and everybody else considered necessary), DEDEKIND defines it positively; the finite, instead, emerges as that which is not infinite. The reason for this lay in direct connection with DEDEKIND's aim of establishing set theory as the *basis* for mathematics, and in particular as the basis for a definition of the natural numbers.[84] From a modern viewpoint, to choose one definition of the infinite or another is only of technical interest, in relation to what axioms we need for developing the theory, or for connecting one definition with the other. But this should not lead us to underestimate the historical and conceptual difficulties implied in DEDEKIND's step. He took what GALILEO and others—CAUCHY among them—had considered a paradoxical property, and used it

84 Defining the infinite as that which is not countable implies that we take the natural numbers as given, and use them to establish a crucial set-theoretical notion. This was precisely CANTOR's position until he knew about DEDEKIND's ideas (see DEDEKIND's letter to Weber in DEDEKIND *1930/32*, vol.3, 488).

for a simple, deductively useful definition. Still more paradoxical must have seemed the fact that his theory of the finite subsets of N was based on the theory of chains, which are infinite subsets. DEDEKIND's theory of natural numbers and finite sets was decidedly an infinitistic one.

We thus see that the notion of infinite set was really crucial for DEDEKIND's project. Along this line, he came to notice that the theory would be incomplete unless he could establish a proposition securing the *existence* of infinite sets. This is how his famous theorem of the infinite came to light. The theorem can not be found in the 1872-78 draft, but only in a comment to the second draft written in 1887 (*Nachlass DEDEKIND* III, 1, II, p. 32). It is well known that, in the meantime, DEDEKIND read BOLZANO's *Paradoxien des Unendlichen*,[85] where a similar proof is presented (BOLZANO *1851*, §13). BOLZANO's proof seems to have motivated DEDEKIND to include this theorem, although it is almost certain that it was not the only reason behind it. During the 1880s, CANTOR's papers on set theory encountered criticism, especially from KRONECKER, who tried to eliminate the infinite altogether from mathematics (cf. EDWARDS *1989*). KRONECKER considered CANTOR's ideas as nonsense and even perverted, proposed to find ways to do without the real numbers, and criticized DEDEKIND's ideal theory for being infinitistic (KRONECKER *1882*, *1887*). The polemical reception of infinite sets during the 1880s was certainly behind DEDEKIND's theorem of the infinite.

BOLZANO had tried to show that "the set of *propositions and truths in themselves*" was infinite. His method was to select a particular truth—the proposition 'that there are truths'—and construct from it another proposition, and another one from this, and so on.[86] Thus he obtained an unlimited series of propositions, producing a set similar to N, and therefore infinite. This proof was dissatisfactory to the extent that it lacked a formal definition of the infinite. Therefore DEDEKIND refined it through his definition, but he also reformulated it in accordance with his peculiar logicist epistemology.

Unlike BOLZANO, DEDEKIND had a strong 'nominalist' bias, in the sense that he avoided any reference to seemingly self-existing abstract objects, such as BOLZANO's world of truths in themselves. For him, mathematical objects were mental creations of the human mind, not self-existing objects. Therefore he tried to show that the "mental universe" **S**, or totality of all possible thoughts, is infinite (DEDEKIND *1888*, 357).

According to his definition of the infinite, what he needed was to establish an injective mapping of **S** such that the image set is a proper part of **S**. Given $s \varepsilon \mathbf{S}$, he selected as its image $\varphi(s)$

85 See DUGAC *1976*, 81, 88, 256.

86 'It is true that there are truths', 'It is true that it is true that there are truths', etc.

the proposition '*s* can be an object of my thought', which is a new possible thought. All of the images thus obtained are propositions according to traditional logical theories, that is, they are composed from a subject and a predicate. Moreover, φ(*a*) and φ(*b*) are different, since they have different subjects, and so the mapping is an injective one. Now, he only needed to exhibit an element of **S** not belonging to φ(**S**), and he selected 'my own self', which undoubtedly—at least for Occidental people—is a possible object of thought. Since 'my own self' has no predicate, it is not a proposition and does not belong to φ(**S**). Therefore, **S** is infinite.[87]

Nobody today would accept this as a mathematical theorem. DUGAC has even written that it is "the only theorem of Dedekind whose «proof» is not coherent with Dedekind's mathematical thought" (DUGAC *1976*, 88). This statement, however, seems to me quite misleading and anachronistic. A basic characteristic of DEDEKIND's mature mathematical thought was to consider mathematics as the science of number, and this as a part of logic. But the logicist project would have fallen down if he had simply postulated an axiom of infinity (see section 4.3.3 above). DEDEKIND tried to show that the existence of infinite sets was a logical truth. It is for this reason that I consider his theorem as completely coherent with his mathematical thought, that is, with his conception of mathematics.

Moreover, the requirement of a proof for the existence of infinite sets was not only a question of elegance and theoretical completeness, but also a matter of consistency. In 1890, DEDEKIND wrote to KEFERSTEIN:

> does such a[n infinite] system *exist* at all in our mental universe? Without a logical proof of existence it would always remain doubtful whether the notion of such a system might not perhaps contain internal contradictions. Hence the need for such proofs [...] (VAN HEIJENOORT *1967*, 101; original in SINACEUR *1974*, 275)

This is still an unsolved—and mathematically unsolvable—question, since no consistency proof for set theory is possible without relying on stronger theories. In spite of the shortcomings of DEDEKIND's proof, later undermined by the antinomies, he shares with BOLZANO the merit of having seen the necessity of a proposition securing the existence of infinite sets within the deductive structure of set theory. For this reason, ZERMELO used to call the axiom of infinity 'DEDEKIND's axiom' (ZERMELO *1908a*, 204; *1909*, 186). The attempts of BOLZANO and DEDEKIND must be counted among the rare serious ones aiming to show the acceptability and validity of the actual

87 See also 'Über den Begriff des Unendlichen' (published in SINACEUR *1974*), where DEDEKIND stresses the relation between his proof and traditional logic.

infinite in mathematics and in human thought generally.

VI. FROM MARRIAGE TO PARTNERSHIP: IMPACT OF THE ANTINOMIES

6.1. Pre-Russellian logicism.

Logicism can be considered to have been in the air since modern mathematics in general, and set theory in particular began to develop. But it was especially during the 1880s, when FREGE and DEDEKIND published their independently developed views, that a growing logicist trend emerged within the mathematical community. In the 1890s and 1900s, followers of logicism appeared in all the main European countries: DEDEKIND, FREGE, and SCHRÖDER in Germany, JOURDAIN and RUSSELL in Great Britain, and COUTURAT in France are the most outstanding names. The early form of logicism, however, has been mostly forgotten since, from the 1900s on, RUSSELL's conceptions became the most influential. RUSSELL and the *Principia Mathematica* (1910/13) represent a second phase in the history of logicism, forced by the effect of the antinomies.

Earlier than the works of FREGE and DEDEKIND, we also have RIEMANN's seminal contribution, that could easily be understood in a logicist sense. But as we have seen its reception is difficult to ascertain. The impact of RIEMANN's notion of manifold on DEDEKIND and CANTOR is almost certain, but the same notion seems to have been understood in a much narrower sense by most mathematicians (see above). It is important to consider, however, that FREGE began his career working on geometry, and so it might well be that RIEMANN's work influenced his evolution.[88] Be that as it may, by the time when logicism began to have a considerable group of followers, set theory was mainly represented by CANTOR and DEDEKIND, and so we do not encounter the name of RIEMANN any more.

DEDEKIND's pioneering contribution of 1888 is surprising when viewed in the context of contemporary logic. He was a 'pure' mathematician who had never published anything on logic, but in 1888 he dared to present a presumed logical theory as a foundation of arithmetic, and even the whole of mathematics. Judged from twentieth-century standards, it was indeed a strange 'logic': nothing but a general theory of sets and mappings, and no mention even of propositional logic! Of course, DEDEKIND's 'logic' is not so strange when compared with the then-reigning algebra of logic, but it is still interesting to examine the response to this proposal within the logical community. Among the diverse contemporary conceptions of logic, we are interested above all in

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88 This is just a conjecture for which I have no proof, but it might be fruitful to review FREGE's early writings in order to corroborate or disprove it. FREGE's "royal road from geometry" is described in WILSON *1992*.

mathematical logic, the principal names being FREGE, SCHRÖDER, and PEIRCE.

And in fact, there is evidence that DEDEKIND's logic was taken as a part of logic by his contemporaries. Even if the logical ideas of FREGE, PEANO, PEIRCE, or SCHRÖDER were more advanced, it was not so for mathematical conceptions, and DEDEKIND's contribution seems to have played an important role in the diffusion of logicism.

In section III we observed how the notion of set was traditionally ascribed to logic, and we have seen that DEDEKIND did not even feel the necessity to argue for the logical character of sets. But his 1888 preface showed a clear desire to convince the reader that the notion of mapping was a purely logical one. According to DEDEKIND, mappings express the mental faculty of relating things to things, making things correspond, or using one thing as a representation of the other; this faculty is an indispensable ingredient of thought, and mappings are also a necessary basis for arithmetic (DEDEKIND *1888*, 336).

The reaction to this proposal was surprisingly good. This was the time of the development of a general theory of relations in the hands of PEIRCE and SCHRÖDER, and therefore a particularly appropriate situation for the reception of DEDEKIND's notion of mapping. Although Charles S. PEIRCE (1839-1931) did not undertake a logicist position, in 1901, writing on 'Logic' for BALDWIN's *Dictionary of Philosophy and Psychology*, he confessed that DEDEKIND's work showed how the borderline between logic and pure mathematics is almost evanescent (PEIRCE *1931/60*, vol.2, 124-125). In 1911 he would say that DEDEKIND's conception of mappings was "an early and significant acknowledgement that the so-called «logic of relatives» is an integral part of logic" (PEIRCE *1931/60*, vol.3, 389).

By and large, however, it was Ernst SCHRÖDER (1841-1902) who received DEDEKIND's foundational work more positively, becoming a partisan of it. His work on the algebra of relatives, and on the subsumption of DEDEKIND's theories under it, finally brought SCHRÖDER's conversion to logicism. Incidentally, it is significant that in SCHRÖDER's and PEIRCE's work, the logicist position is always associated with DEDEKIND, and not with FREGE.

In 1898, SCHRÖDER expressed openly his logicist conviction, saying that "pure mathematics seems to me just a *branch* of *general logic*" (SCHRÖDER *1898*, 149).[89] But he had not always been so positive about it. In 1890, while emphasizing that arithmetical truths are purely analytical, he was careful to leave open the question whether arithmetic can be equated with logic; thus he distanced himself from "those who, with DEDEKIND, consider arithmetic as a branch of

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89 This and related texts of the late 1890s have been analyzed in PECKHAUS *1991*, especially 177 and 191-194. But PECKHAUS has not observed the Dedekindian origins of SCHRÖDER's logicism (cf. also his *1993*).

logic" (SCHRÖDER *1890/1905*, vol.1, 441).[90] Apparently, it was in 1891 that he endorsed for the first time the logicist viewpoint.[91]

Over the years, SCHRÖDER's admiration for DEDEKIND's work grew. Its apex can be found in the third volume of SCHRÖDER's major work, the *Vorlesungen über die Algebra der Logik* (1895). Here, two out of 12 lectures were devoted to an examination of DEDEKIND's theories: chapter 9 dealt with chain theory, and chapter 12 with the theory of mappings, which of course SCHRÖDER conceived as relations of a particular kind (see SCHRÖDER *1890/1905*, vol.3). Most notable is the emphasis with which SCHRÖDER underlined the value of DEDEKIND's "epoch-making" contributions:

> It is therefore to the *filling of a* great and important *gap, that* until now *could be found in all manuals of arithmetic and algebra* (not excluding that of the present author), that Mr. Dedekind has devoted himself with success! [...] When I consider, on the one hand, how much the calculus of logic had to advance in its development, in order that it were just possible to establish the lost connection in a really *conclusive* way, and on the other hand, how much sharpness the filling of that gap has required from Dedekind [...], then I cannot reproach myself or any other exposer of arithmetic for the existence of the gap. On the contrary, one must admire all the more the contribution that created the missing connection. (SCHRÖDER *1890/1905*, vol.3, 349).

SCHRÖDER devoted several pages of his book to review *Was sind und was sollen die Zahlen?* (ibid., 346-352), and stated that one of the "most important objectives" of his work was to incorporate all the essential parts of DEDEKIND's book into the edifice of general logic (ibid. 346).

Nevertheless, SCHRÖDER's efforts were not directed towards developing the logicist program, but towards showing the capabilities of his algebra of relatives. For instance, SCHRÖDER generalized the theory of chains, which as he observed does not strictly require mappings, but can be applied generally to binary relations. In any case, there is little doubt that SCHRÖDER's treatise was an instrument for the diffusion of DEDEKIND's ideas around the turn of the century, especially outside Germany (DEDEKIND's own work was probably more widely read in Germany itself).

As regards FREGE, he acknowledged DEDEKIND's book as "the most complete work on

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90 The sense given to "arithmetic" by SCHRÖDER is exactly the same general sense in which DEDEKIND used the word (SCHRÖDER *1890/1905*, vol.1, 441 footnote).

91 "Arithmetic [...] has developed itself into a branch of logic, especially thanks to the works of H. GRASSMANN, G. CANTOR, WEIERSTRASS, and DEDEKIND" (ibid., vol.2, 54). Those four mathematicians are mentioned because of their important contributions to the foundations of arithmetic, but, needless to say, only DEDEKIND was openly a logicist——CANTOR, in particular, opposed to logicism.

the foundations of mathematics which has come to my knowledge lately" (FREGE *1893*, vii). Nevertheless, FREGE was the most critical of all contemporary logicians, and expressed openly his dissatisfaction with the work of the great set theorists. He thought that contemporary notions of set were not really abstract notions, nor of course logical notions: they were just generalizations of the naive idea of a grouping of things.[92] In his view, it was necessary to give primacy to the intensional viewpoint: sets or classes are no satisfactory foundation for logic; instead, everything should be stated in terms of concepts.[93]

This does not mean that FREGE wanted to dispense with set-theoretical ideas; on the contrary, he thought it possible to restate CANTOR's results in a more rigorous way. In fact, for his logicist deduction of arithmetic FREGE needed sets or classes, which he introduced as "extensions of concepts" (FREGE *1884* and *1893/1903*); thus he was led to use exactly the old terminology of German logicians. In fact, he formulated a logical "law" that became the most precise expression of the traditional relation between concepts and classes. This was the basic law V of the *Grundgesetze* (FREGE *1893*, 35-36, 240), which involved the so-called principle of comprehension responsible for the sinking of FREGE's ship due to RUSSELL's antinomy.

DEDEKIND's mappings were also subject to FREGE's objections, which are similar to his objections to sets. Mappings are not purely logical tools; instead one should speak in the intensional tongue, namely about relations. To end his discussion of DEDEKIND's ideas, FREGE wrote: "Concept and relation are the basic stones on which I erect my building", i.e., the *Grundgesetze der Arithmetik* (FREGE *1893*, 3). FREGE's definition of natural numbers, for instance, cannot be stated without making use of concepts, extensions of concepts, and relations.

In spite of FREGE's criticism, the parallelism between his pair of basic notions, concept and relation, and DEDEKIND's basic ideas of system and mapping, is remarkable. If we ignore the choices made by FREGE on the basis of his preference for the intensional, this is just a confirmation that DEDEKIND's was indeed a logical theory. Most important, it shows that *DEDEKIND's and FREGE's logicisms rested essentially on the same basis*.

The parallelism between FREGE's and DEDEKIND's theories of arithmetic shows how the logicist program needed set theory, or an equivalent device, in order to subsume arithmetic—or classical mathematics generally—under pure logic. Logicism depended the notions of set and relation, conceived extensionally or otherwise. Thus set theory was an indispensable ingredient of

92 In the introduction to FREGE *1893* (1-3) he criticizes DEDEKIND's notion of set, as he had done with CANTOR's theory in a review; in FREGE *1895* he criticizes SCHRÖDER.

93 A lengthy and sophisticated defense of the extensional viewpoint can be found in SCHRÖDER *1890/1905*, vol.1, 83-101.

the logicist's logic, which is why this first logicist program was shaken by the antinomies. For as we will see, the antinomies undermined the traditional justification of the logical character of sets: the connection between concept and set, the principle of comprehension.

6.2. Effect of the antinomies.

This is not the place to detail the complex 'discovery' of the antinomies, from CANTOR's clear perception of the issue during the 1890s,[94] through the changing and sometimes obscure reflections of Cesare BURALI-FORTI (1861-1931) and RUSSELL, to FREGE's despair, which finally led to a general recognition of the problem.[95] The important point for us is that the antinomies showed the unrestricted transition from concept to set, or alternatively the notion of a universal set, to entail contradictions. Thus they forced changes that affected core elements of traditional logic, and that altered the whole relation of set theory and logic.

The traditional transition from a concept to its corresponding extension, class or set had led to a naive acceptance that any (apparently) well-defined concept determined an acceptable—i.e., non-antinomical—set. In terms of ZERMELO's axiom of subsets or 'separation', it was as if a universal set existed, to which any property could be applied in order to get a subset. (In fact, most 19th century logicians and mathematicians seem to have accepted the notion of a universal set—with the notable exceptions of CANTOR and SCHRÖDER.)

In 1897, as an editor of *Mathematische Annalen*, HILBERT corresponded with CANTOR on the latter's project of adding a third part to the 'Beiträge zur Begründung der transfiniten Mengenlehre'. In this third part, the well-ordering theorem would be proven on the basis of the antinomical character of the set of all alephs.[96] CANTOR communicated to HILBERT this antinomy, but at first HILBERT could not accept the contradictory character of the set of alephs. His reply, textually quoted by CANTOR, is a perfect example of the traditional mentality:

The collection [Inbegriff] of alephs may be conceived of as a concrete well-defined set, for certainly, when any thing is given,

94 Certainly CANTOR did not see any antinomies, since the arguments did not appear paradoxical from his viewpoint; but he understood very clearly the fact that assuming some sets leads to contradictions, and the way in which this affected DEDEKIND's work (cf. their correspondence in CANTOR *1932* and the letter to HILBERT in PURKERT & ILGAUDS *1987*, 154).

95 Cf. GARCIADIEGO *1992*, MOORE *1993*. The general recognition began in 1903, with RUSSELL *1903*, and the appendix to FREGE *1903*. Already in 1897-99, CANTOR perceived the matter correctly, although he saw no contradiction since he did not share the logicist's beliefs (PURKERT & ILGAUDS *1987*); since he did not publish, only a few (DEDEKIND, HILBERT and his circle) knew of the antinomies through him.

96 This correspondence was first published in PURKERT & ILGAUDS *1987*, 224-231; it is discussed in ibid. 150-159.

it must always be possible to decide whether it is an aleph or not; and nothing else corresponds to a well-defined set. (PURKERT & ILGAUDS *1987*, 226)

Here, the property of being an aleph is considered as sufficient for the determination of a consistent set; this step is justified by implicitly resorting to the universal set, with the words "when any thing is given".

Eventually, HILBERT came to accept the surprising conclusion that neither the concept/set relation, nor the universal set, were reliable. From this, he extracted a lesson that confirms my main point:

the set theoretic paradoxes [...] show, it seems to me, that the conceptions and means of investigation prevalent in logic, taken in the traditional sense, do not measure up to the rigorous demands that set theory imposes. (HILBERT *1904*, 130, or *1930*, 249).

In HILBERT's view it was logic, not mathematics or set theory, that was threatened by the antinomies: set theory had pressed traditional logic to the point of showing its inadequacy. This, in my opinion, is basically correct: more important than the need to find a new basis on which to establish set theory was the fact that it had become necessary to alter radically the traditional picture of logic and its demarcation.

There is little doubt that HILBERT passed on his advanced information on set theory and the antinomies to his students, among which Ernst ZERMELO (1871-1953) occupied an outstanding position during the 1900s (see MOORE *1980*, 130). ZERMELO discovered independently the RUSSELL antinomy, that gave a new and even more direct stroke to the traditional confidence in the relation concept/set. RUSSELL's antinomy did not depend on sophisticated notions such as CANTOR's alephs or ordinals, but used only elementary notions of logic and set theory: negation and the belonging relation. Thus, there was no doubt that the property invoked by RUSSELL: 'to be a set which does not belong to itself', was purely logical. The failure of the traditional concept/set relation, encoded in FREGE's axiom of comprehension, was plainly shown.

In his famous paper of 1908 presenting an axiom-system for set theory, ZERMELO summarized the impact of the antinomies as follows:

At present, however, the very existence of this discipline [set theory] seems to be threatened by certain contradictions, or "antinomies", that can be derived from its principles—principles necessarily governing our thinking, it seems—and to which no entirely satisfactory solution has yet been found. In particular, in view of the "Russell antinomy" of the set of all sets that do not contain themselves as elements, it no longer seems admissible today to assign to an arbitrary logically definable notion [Begriff] a set, or class, as its extension. (ZERMELO *1908a*, 200)

Clearly, therefore, what the antinomies had shaken was the naive transition from a concept to a set, commonly accepted by ZERMELO's predecessors.[97] Under these circumstances, ZERMELO did not try to find a new definition of set, but proceeded inversely: starting from the existing set theory, he derived an axiom-system sufficient for its rigorous foundation (ibid.).

Incidentally, it is appropriate in the context of this paper to mention that ZERMELO considered both DEDEKIND and CANTOR as the founders of set theory. The introduction to his fundamental paper on the axiomatization of set theory reads:

In the present paper I intend to show how the entire theory created by *Cantor and Dedekind* can be reduced to a few definitions and seven principles, or axioms (ZERMELO *1908a*, 200; emphasis added).

ZERMELO studied carefully the works of his predecessors in order to discover the basic postulates involved in set theory, and it has been mentioned several times that some of his axioms were based on DEDEKIND's book. In particular, he called the axiom of infinity "Dedekind's axiom" (ZERMELO *1908a*, 204; *1909*, 186), and stressed that the failure of DEDEKIND's theorem of the infinite did not affect the rest of his theory.[98] Moreover, ZERMELO used chain theory for some proofs presented in his papers (ZERMELO *1908a*, 209)[99] and he employed a transfinite generalization of DEDEKIND's chains for his second proof of the well-ordering theorem (ZERMELO *1908*, 190). The latter is, in my view, the best example of how ZERMELO's work harmonized and intertwined the ideas of DEDEKIND and CANTOR.

Coming back to the antinomies, as we see several important mathematicians interpreted them as showing the failure of traditional logic, or of FREGE's axiom of comprehension. HILBERT and ZERMELO seem to have come to the conclusion that set theory was strictly speaking a mathematical theory: it was not a part of logic, and therefore it required an autonomous, axiomatic foundation. Thus ZERMELO established a series of set-theoretical axioms, like those of infinity, of

97 Though notably not by CANTOR, who nevertheless expressed himself with so little clarity that only recently has his opinion been properly understood (see PURKERT & ILGAUDS *1987*).

98 LANDAU *1917*, 56, where LANDAU attributes the passage to ZERMELO.

99 See his comments in CANTOR *1932*, 451 note 2.

power sets and union sets, or of choice, which capture essential traits of the cumulative hierarchy (cf. ZERMELO *1908a*). All of these axioms, and the idea of the cumulative hierarchy itself, have nothing to do with logic. ZERMELO included an axiom of 'separation' or subsets which is just a revised version of FREGE's axiom of comprehension; but this axiom, indispensable for all practical purposes, can be regarded as secondary to the basic foundation of set theory constituted by the rest of his axioms.

The divorce between logic and set theory was thus consummated, breaking a centuries-old tradition. The resulting panorama was especially disturbed by the axioms of infinity and choice, which being purely existential postulates seemed completely foreign to logic. And the situation was further worsened due to later changes in logical theory which culminated in its restriction to the first-order calculus.[100] With this, logic ceased to be the source of set theory, but there was an ever-stronger interaction between logic and set theory, responsible for many of the most interesting results obtained in this century. From the destroyed marriage, a fruitful partnership emerged.

Nevertheless, not everybody took ZERMELO's path: there was a logicist resistance, a group of logicians that tried to recover the lost splendor of FREGE's and DEDEKIND's ideal. The leader of this movement, that inaugurated the second phase in the history of logicism, was RUSSELL. Far from ZERMELO's recourse to the cumulative hierarchy, RUSSELL interpreted the antinomies as showing the need to *restrict* FREGE's axiom, but not to abandon it, nor to alter the core of traditional logical theory. His theory of types constitutes an attempt to secure the concept/set connection through precise specifications about the kind of objects admissible as belonging to the extension of the concept. The restrictions are based on the assumption of a series of types, such that a set of the *n*th type can only have elements belonging to the *n-1* previous types (see RUSSELL *1908* and RUSSELL & WHITEHEAD *1910/13*). In RUSSELL's theory, the concepts—now called 'propositional functions'—are still the core logical element, which gives rise to classes or sets through the concept/set relation. It is in this sense that RUSSELL's reaction to the antinomies was much closer to the traditional viewpoint than ZERMELO's.[101]

This revised version of logicism, elaborated in the post-antinomies era, is the one that presently prevails in the minds of historians, logicians and mathematicians. The present paper, however, has tried to show that second-phase logicism is not the right place to start if one is looking

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100 I can only point to these developments here; see GOLDFARB *1979*, MOORE *1980* and *1987*.

101 I have oversimplified RUSSELL's complex thoughts on the theory of types and its connections with the no-classes and substitutional theories, partly because this is not the place to enter into them, and partly to show more clearly the way in which the theory of types can be seen as a development of the traditional view. Interested readers should see the relevant essays in PEARS *1972*, SAVAGE & ANDERSON *1989*, WINCHESTER & BLACKWELL *1989*, and also the paper RODRIGUEZ CONSUEGRA *1989*.

for historical understanding of the emergence of the logicist movement.

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University of California at Berkeley