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The Emergence of Some of the Nonlogical Paradoxes of the Theory of Sets, 1903–1908

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The goal of this paper is to discuss why, how, and when nonlogical set-theoretic paradoxes were discovered. No one factor dominated the whole outlook; instead, it is argued that these paradoxes were not a simple and direct consequence of the ones discovered by Bertrand Russell and published for the first time in his *The Principles of Mathematics* (1903). © 1985 Academic Press, Inc.

La finalidad de este artículo es discutir el porqué, cómo y cuándo fueron descubiertas las paradojas no lógicas de la teoría de conjuntos. No hubo un factor que dominara la perspectiva por completo; en su lugar, se argumenta que estas paradojas no fueron una consecuencia simple y directa de las descubiertas por Bertrand Russell y publicadas por primera vez en *The Principles of Mathematics* (1903). © 1985 Academic Press, Inc.

Le but de cet article est de discuter pourquoi, comment et quand ont été découvertes les paradoxes non-logiques de la théorie des ensembles. On essayera de montrer qu'il n'y en a pas un facteur qui domine complètement la perspective; en particulier, ces paradoxes n'ont pas été une conséquence simple et directe de celles découvertes par Bertrand Russell qui ont été publiées pour la première fois dans *The Principles of Mathematics* (1903). © 1985 Academic Press, Inc.

1. INTRODUCTION

The traditional approach to studying the origin of the nonlogical paradoxes [1] treats them as immediate consequences of the logical paradoxes discovered by Bertrand Russell (see [Garciadiego 1983, 5–29]). Such interpretations imply that Russell's role was simply that of a guide. However, subsequent research has shown that his participation was much more complicated, and that he was in fact directly involved with the origin of the nonlogical paradoxes as well.

Throughout, the following points will be assumed: first, that the diffusion and metamorphosis of the Burali-Forti paradox paralleled the origin and development of other paradoxes [Moore & Garciadiego 1981, 331–342]; second, that Zermelo's proof of the Well-Ordering Theorem [1904]—the assertion that any set can be well-ordered—provoked immediate polemics (these controversies involved, at least, French, German, British, and Italian mathematicians); third, that the atmosphere surrounding the origin of the paradoxes was one of confusion and misunderstanding among mathematicians and philosophers. This state of confusion caused some researchers to treat some arguments of others as paradoxes, even though they were not originally considered paradoxical. In fact, such confusion persists; one finds historians describing and discussing "paradoxes" that were not considered as such by their creators.

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In what follows, only the origin of those nonlogical paradoxes arising directly from Cantor's theory of sets will be discussed. Others (e.g., the paradox of Epimenides), although sometimes discussed in connection with Cantor's ideas, were originally concerned—at least according to their creators—with other questions. Specifically, the boundaries observed here are the years from 1903 to 1908. In 1903 Russell published *The Principles of Mathematics* in which he described the paradox of the greatest cardinal number (Cantor's paradox), and elements that would lead somewhat later to the greatest ordinal number (Burali-Forti's paradox). According to earlier studies (van Heijenoort 1967a, 199], two separate attempts made in 1908—Zermelo's and Russell's—seemed to solve the paradoxes. Consequently, 1908 is taken here as the closing boundary for this account. Nevertheless, Moore [1978, 307] argues that Zermelo published his axiomatization of set theory [1908b] in an endeavor to respond to the objections of other mathematicians to his Axiom of Choice and his proof of the Well-Ordering Theorem. At the same time, Moore has also shown that Zermelo was not trying to resolve the paradoxes. On the contrary, it was Russell who attempted this. According to Russell, he had already proved that all paradoxes could be generated from a single general one [Russell 1907, 35], which implied that it was possible to formulate any number of them. In the article of 1908, where Russell presented his final attempt—the ramified theory of types—to resolve the paradoxes, he described some of the contradictions (as he called them) which he intended to lay to rest. Among those discussed by Russell were the Epimenides paradox, equivalent to his own in terms of relations, as well as the paradoxes due to Berry, König, Richard, and Burali-Forti. Russell did not discuss the paradox of Grelling and Nelson [1908]—formulated in terms of two classes of "autological" and "heterological" adjectives—doubtless because it was unknown to him at the time he published his paper on the theory of types. Nor is there any discussion of this paradox in *Principia Mathematica* [Whitehead & Russell 1912], where the authors proposed the same ramified theory of types once again. Therefore, supposing that Russell was completely unaware of the existence of this paradox, no attempt will be made here to analyze its roots. On the other hand the König–Zermelo paradox—which does not come up in Russell's [1908]—is considered because it was mentioned during his debate with Henri Poincaré.

2. BERRY'S PARADOX

The first nonlogical paradox was the one discovered by G. G. Berry about 1904. He communicated his paradox to Russell by letter (Berry to Russell, December 21, 1904; letter in the Bertrand Russell Archives, McMaster University, Hamilton, Ontario, Canada), but Russell published a modified version of it only in 1906 [Russell 1906, 645]. Berry was working at the Bodleian Library at the time and, according to Russell, he was the only person at Oxford who knew any mathematical logic [Feinberg 1967, 140]. As far as can be determined, Berry did not hold an academic position when he met Russell (holograph note written by Russell, undated; Bertrand Russell Archives).

In his letter to Russell, Berry said that he was disappointed with *The Principles* and the articles Russell had published in Peano's journal *Rivista di Matematica* because he did not provide a proof that the ordinal numbers could be well-ordered. This point is quite important for the origins of these arguments, in particular of Burali-Forti's paradox, because the absence of a proof allowed Russell to avoid the contradiction [Russell 1903, 323]. Berry maintained that Georg Cantor had virtually proved the existence of such a well-ordering when Cantor had shown that ordinals of the second class are well ordered [2]. Berry insisted that "it is very easy to prove that the series [of finite ordinal numbers] is well-ordered which has none but well-ordered segments" (Berry to Russell, December 21, 1904; Bertrand Russell Archives). For this same reason, namely Russell's lack of a proof, Berry wrote, "You did not involve yourself in a contradiction which I expected to find you had" (Berry to Russell, December 21, 1904). This implies that by the time he had read Russell's book, Berry had already explored the possible consequences of denying such a principle. In other words, Berry's discovery of the paradox seems to have been independent of Russell's writings.

It has not been possible to discover the reasons for Berry's interest in these problems. There seems to be nothing mathematical published by him, and there is no way of telling whether or not he was one of those "who wished to be known only by their paradoxes" [Russell 1899, 120, 1900, 72]—a sentence Russell paraphrased from Leibniz. It is also important to keep in mind that Berry is equally well known for the way in which he introduced himself to Bertrand Russell. According to Russell (and confirmed by recent studies; see [Grattan-Guinness 1977, 50]), Berry presented Russell with a note saying, "The statement on the other side of this paper is true"; on the other side was written, "The statement on the other side of this paper is false." Consequently, Berry has been credited with the "visiting card paradox."

It is not surprising to find Berry searching for contradictions or paradoxes in set theory. Zermelo had already discovered independently and prior to Russell the contradiction of the set of all sets that are not members of themselves [3]. David Hilbert claimed to have discovered "even more convincing contradictions [than Zermelo's or Russell's]" [Frege 1980, 51] [4]. Moreover, it has been shown that Russell discovered at least two of the most famous paradoxes (Cantor's and his own) as a direct consequence of his complete acceptance of Cantor's theory [Garciadiego 1983, 151–167, 247]. In fact, it was only after Russell's mathematical and logical formulations of these arguments that they became so well known.

The original formulation of Berry's paradox was the following:

> Some ordinals, e.g., ω, ω^ω, ω^{ω^ω} are definable in a finite number of words. Let us suppose that there is any ordinal which is not so definable. The ordinals less than this particular one are a well-ordered series. Hence, if among them there are any which are not finitely definable, there is one of these less than all the others. This least number of the class is then the least ordinal which is not definable in a finite number of words. But this is absurd, for I have just defined it in thirteen words. Going back a little I infer that one not finitely definable ordinal cannot exceed another such, therefore there is at least one such; and this again is absurd [Berry to Russell, December 21, 1904, Bertrand Russell Archives].

Russell published a modified version of this paradox in 1906. Prior to this, there is no indication that Berry had communicated his argument to anyone else. In other words, the paradox was not known to the mathematical community at large, nor did it play a role in the origin of other paradoxes developed before this date.

Summarizing, Berry's paradox seems to have originated independently of Russell's paradoxes; that is to say, it was not a direct consequence of the logical ones. At the same time, Berry's argument does not appear to have influenced other researchers in the formulation of their paradoxes.

3. THE KÖNIG–ZERMELO PARADOX

In 1883, Cantor affirmed that the principle asserting that every "well-defined" set could be reduced to a "well-ordered" set was a "fundamental logical law of great consequence, being noteworthy by its universal validity" [Cantor 1932, 169]. By 1895, however, Cantor realized that it was necessary to *prove* this principle [Moore 1979, 138]. The theorem became well known in 1900 when Hilbert posed it—along with the Continuum Hypothesis, asserting that $2^{\aleph_0} = \aleph_1$—as the first of his famous twenty-three problems.

Several attempts to prove the well-ordering theorem were published but none was valid (see among others [Hardy 1904; Jourdain 1904a, b]. It was not until August 10, 1904, at the Third International Congress of Mathematicians at Heidelberg, that Jules König presented his contribution in which he "proved" the falseness of the theorem [König 1905a, 144–147]. In particular, he argued that not all transfinite sets could be well-ordered. He based this upon a supposed demonstration that the set of all real numbers could not be well-ordered, and, therefore, the Continuum Hypothesis itself must be false [Moore 1979, 143–148]. Zermelo, however, found an error in König's proof the day after its presentation at the congress [Kowalewski 1950, 202; Moore 1979, 145]: König had used one of Bernstein's results on the exponentiation of alephs [5]. On September 24, 1904, Zermelo sent a letter to Hilbert, who was then editor of *Mathematische Annalen*, in which part of Zermelo's letter appeared two months later (see [Zermelo 1904, 514–516]; translated into English in [van Heijenoort 1967a, 139–141]). But in the portion of the letter published by Hilbert, no mention was made of any mistake in König's proof. For mathematicians who attended the congress but who were not in close contact with the circle of friends associated with Zermelo and Hilbert, there must have seemed a contradiction. On the one hand, König had supposedly proved that the set of all real numbers was not well-ordered; on the other, Zermelo had just proved that *any* set could be well-ordered [6].

Soon the French mathematician Jacques Hadamard published a brief report of the Heidelberg congress (see [Hadamard 1904, 961–962]; see also [Borel 1912, 157]). Although he was unable to give a detailed description of the papers presented, he noted one that was of particular interest: König's paper devoted to the question of whether the continuum could be arranged as a well-ordered set. Hadamard went on to say that the question formulated at the congress of 1900 had apparently found its answer at the congress of 1904, and that the answer was negative. Nevertheless, Hadamard said, it should not be forgotten that Zermelo had recently arrived at the exact opposite conclusion [Hadamard 1904, note 4]. A few months later, Hadamard returned to the same question [1905, 241]. Again, he stressed that the two conclusions were contradictory. He noted that König's contribution was still unpublished, and that consequently it was impossible to discuss, even though mathematicians were already debating Zermelo's Axiom of Choice [7].

There are several interrelated points to be made about Hadamard's note. First, he did not resolve the apparent contradiction between König's and Zermelo's contributions. Instead, he mentioned that this was not the first time contradictions had arisen in set theory. Although Cantor had asserted the existence of a series of transfinite ordinal numbers, Burali-Forti had shown that the existence of certain transfinite numbers, with the properties discovered by Cantor, implies a contradiction. In fact, Hadamard was not surprised by the appearance of these contradictions—not because he was opposed to Cantor's theory, but, on the contrary, because contradictions seemed, historically, a natural occurrence in mathematics. After all, as Hadamard noted, mathematicians had encountered contradictions when they first introduced incommensurable, negative, and imaginary numbers [Hadamard 1905, 241].

Ironically, Zermelo and König already knew their results were not contradictory, because Zermelo had found a mistake in König's proof rendering it invalid. In fact, when König's contribution to the congress was published in 1905, he had already modified his position, and only claimed that "if" Bernstein's result were true in general, "then" the set of all real numbers could not be well-ordered [König 1905a, 147; Moore 1979, 146]. This left open the possibility of future paradoxes, but in fact, there was none present in König's work of 1905.

4. RICHARD'S CONTRADICTION

Jules Richard seems to have discovered the contradiction associated with his name strictly from Hadamard's notes. Apparently he did not know of the dispute among mathematicians over Zermelo's proof of the Well-Ordering Theorem, nor was he aware of the concrete formulation of Burali-Forti's paradox also described by Bernstein and Schoenflies in the pages of *Mathematische Annalen*. Their description was based on Jourdain's comments and a paraphrasing of Russell's version [Moore & Garciadiego 1981, 334].

In a letter to Louis Olivier, editor of the *Revue générale des sciences pures et appliquées*, Richard described another contradiction (see [Richard 1905, 541]; English translation in [van Heijenoort 1967a, 143–144]). He mentioned that he had read the note published by Hadamard which contained certain contradictions found in the theory of sets. Richard did not specify the contradictions to which he was referring, but it is reasonable to suppose that he was taking into consideration the Zermelo–König and Cantor–Burali-Forti contradictions, the two mentioned by Hadamard. Richard did not seem to know the detailed formulation of the latter, which supports the hypothesis that he came upon the paradoxes through Hadanard's note, where this argument was only cited. It is important to keep in mind that Zermelo, König, Cantor, and Burali-Forti never claimed to have discovered *these* paradoxes.

In his letter, Richard mentioned that it was not necessary to go so far as the theory of ordinal numbers to find contradictions:

> I am going to define a certain set of numbers, which I shall call the set E, through the following considerations. Let us write all permutations of the twenty-six letters of the French alphabet taken two at a time, putting these permutations in alphabetical order; then, after them, all permutations taken three at a time, in alphabetical order; then, after them, all permutations taken four at a time, and so forth. These permutations may contain the same letter repeated several times; they are permutations with repetitions.
>
> For any integer p, any permutation of the twenty-six letters taken p at a time will be in the table; and, since everything that can be written with finitely many words is a permutation of letters, everything that can be written will be in the table formed as we have just indicated.
>
> The definition of a number being made up of words, and these words of letters, some of these permutations will be definitions of numbers. Let us cross out from our permutations all those that are not definitions of numbers. Let u_1 be the first number defined by a permutation, u_2 the second, u_3 the third, and so on.
>
> We thus have, written in a definite order, *all numbers that are defined by finitely many words*.
>
> Therefore, the numbers that can be defined by finitely many words form a denumerable infinite set.
>
> Now, here comes the contradiction. We can form a number not belonging to this set. "Let p be the digit in the nth decimal place of the nth number of the set E; let us form a number having 0 for its integral part and, as its nth decimal place, $p + 1$ if p is not 8 or 9, and 1 otherwise." This number N does not belong to the set E. If it were the nth number of the set E, the digit in its nth place would be the same as the one in the nth decimal place of that number, which is not the case.
>
> I denote by G the collection of letters between quotation marks.
>
> The number N is defined by the words of the collection G, that is, by finitely many words; whence N should belong to the set E. But we have seen that it does not.
>
> Such is the contradiction. [Richard 1905, 541] (quoted from [van Heijenoort 1967a, 143])

A remarkable point of this argument is that Richard explicitly argued that he had discovered a *contradiction*, not a paradox. He even suggested a solution for it. According to him, the collection G had meaning only if the set E were totally defined; this could not be done except with infinitely many words [Richard 1905, 541; van Heijenoort 1967a, 143]. Henri Poincaré agreed with Richard's solution and called Richard "sagacious" [Poincaré 1906, 305]. Perhaps Poincaré thought that Richard was identifying the idea of *any* infinite set as the source of the contradiction, and therefore that the infinite should be expelled from mathematics. It should not be forgotten that this rejection of the actual infinite was the main consequence of Poincaré's criticism of Cantor's theory of transfinite numbers, as well as Russell's logicism—the thesis that mathematics is completely deducible from logic. Later on, Richard responded to certain objections (in particular, ones raised by Peano [1973, 218]), and at the same time explicitly referred to the actual infinite as the cause of his contradiction.

5. POLEMICS OVER THE WELL-ORDERING THEOREM AND INITIAL RESPONSES TO THE BURALI-FORTI PARADOX

In contrast to Burali-Forti's article of 1897 [Garciadiego 1983, 5–15], Zermelo's paper of 1904 did provoke an immediate and strong reaction which consisted of several publications opposed to certain features of his proof. Perhaps the most interesting aspect of these criticisms lies in the fact that different steps of the proof came under attack from different authors [Moore 1978, 312].

In addition to controversies in the German and French mathematical communities (see [Moore 1978, 312–315]), there was also dispute among British mathematicians. E. B. Hobson initiated an exchange of ideas carried on in the pages of the *Proceedings of the London Mathematical Society* when he published a long article reacting to [Hardy 1904; Jourdain 1904b]. Hobson criticized their attempts to prove that every transfinite cardinal number was necessarily an aleph. As far as can be determined, Hobson was the first mathematician to suggest that—because of the contradictions involved in conceiving of the set of *all* ordinal and cardinal numbers—it was necessary to carry out "a further scrutiny of the foundations of . . . [set] theory" [Hobson 1905, 171]. Hobson argued that Jourdain's explanations of the contradictions were not satisfactory, because they were based on a distinction between consistent and inconsistent sets—a distinction that Jourdain claimed to have introduced independently of Cantor [Hobson 1905, 172]. Hobson insisted that one needed a criterion to decide when the set was consistent or inconsistent [Garciadiego 1983, 20–23], something Jourdain had failed to provide [8]. Hobson proposed a new definition of "set" in terms of objects satisfying a prescribed "norm." This meant that a set was defined by conditions sufficient to determine whether an object belonged to an aggregate or not [Hobson 1905, 173]. Unfortunately, this kind of definition cannot assure that a transfinite set could be ordered. For this it would be necessary to include a principle implicitly or explicitly in the norm (Hobson's term) defining the aggregate by which the aggregate could be ordered [Hobson 1905, 174].

There were replies to Hobson's article from Hardy [1907], Dixon [1907], Russell [1907], and others. Hardy announced that Russell was preparing a lengthy reply on general aspects of Hobson's criticisms, including Hobson's doubts that any nondenumerable aggregates could be well-ordered. Hardy therefore limited his comments to criticisms Hobson had made of Hardy [1904], although he also discussed the acceptability of the Multiplicative Axiom and Zermelo's axiom [9]. Hardy accepted, at least provisionally, the Multiplicative Axiom, although he left open the question of Zermelo's Well-Ordering Theorem [Hardy 1907, 17].

Russell's reply to Hobson's paper was read before the London Mathematical Society on December 14, 1905. Russell insisted that Hobson had confused difficulties relating to inconsistent aggregates with other difficulties relating to Zermelo's axiom [Russell 1907, 29]. Russell identified his own concept of "propositional function" with Hobson's concept of the "norm" of a set, and discussed the contradictions described by Hobson. In particular, he showed that they were all particular cases of a more general contradiction [Russell 1907, 35]. Russell also claimed to have shown that the contradictions were not essentially arithmetical, but were actually logical. Therefore, in order to resolve them, changes would have to be made in the logical assumptions used [Russell 1907, 37]. In addition, he proposed three different logical "theories"—Russell's terms—to eliminate the contradictions, all of which had their roots in *The Principles of Mathematics*. The "Zig-Zag Theory" would eliminate propositional functions that were too complex, but would permit others, such as the one asserting that the complement of a class is a class. It would allow a largest cardinal number but not an ordinal. The axioms Russell introduced, however, were quite complicated and "lacked intrinsic plausibility" [Moore & Garciadiego 1981, 337].

Russell's second proposal advanced the limitation of size of classes. A similar solution had already been proposed by Jourdain [1904b, 67], and by Cantor (in his correspondence with Dedekind, which remained unpublished until 1932). Within this theory classes like "the class of all entities" would be prohibited, as well as classes like "the class of all ordinal numbers." However, it was necessary to admit all of Cantor's ordinals. Thus there remained a difficulty: how to state a limitation of class size precisely. To cope with this problem Russell proposed a third solution, the "no-class" theory. In order to avoid the contradictions entirely, this theory banished classes and relations altogether. In this theory a propositional function would be treated—by substitutions—simply as an abbreviation of an argument about one or several of its values [Moore & Garciadiego 1981, 338]. Russell anticipated a number of possible objections to the theory: that classes seem too obvious to be denied; that one would have to deny a great part of Cantor's theory of transfinite numbers; that working with the theory was very complicated.

The "no-class" theory was clearly the most radical of the three. Although Russell at first doubted that it was the final solution to the contradictions, two months later he added a note to the article saying that he had no doubts that the "no-class" theory afforded the complete solution [Russell 1907, 53, note added February 5, 1906] [10].

Russell devoted the rest of his article to Zermelo's Axiom of Choice and his own Multiplicative Axiom. Russell exemplified the use of Zermelo's axiom by his now famous example of the man who owns $\aleph_0$ pairs of boots [11]. He also stated that his own axiom could be deduced from Zermelo's, but Russell not only doubted the validity of the general case—Zermelo's Axiom of Choice—but also of his own [Russell 1907, 47, 52].

6. KÖNIG'S AND DIXON'S PARADOXES

In the meantime, König kept working on his attempt to disprove Cantor's Continuum Hypothesis. In order to do so, König was trying to find a proof that the continuum could not be well-ordered. His second attempt following the Heidelberg congress of 1904 was made in a lecture originally delivered before the Hungarian Academy of Sciences on June 30, 1905 [van Heijenoort 1967a]. This happened almost simultaneously with Richard's letter to the *Revue générale des sciences pures et appliquées*. König's aim, however, was quite different from Richard's. The latter was very clear in describing his argument as a "contradiction" and he explicitly stated that other contradictions discovered by other mathematicians had motivated him to write his note. König, on the other hand, was not so much concerned with paradoxes. In the first place, he argued that the indiscriminate use of the word "set" was the real source of the apparent paradoxes. In fact, he proposed solutions to the paradoxes in two different places without going into a detailed analysis, which suggests that he did not consider them of major importance. His first solution consisted in denying that the second number class (see [2]) could be considered as a completed set, that is, "a totality of well-distinguished elements that are altogether conceptually distinct" (see [König 1905b, 160]; English translation in [van Heijenoort 1967a, 148]). This would solve the Burali-Forti paradox, because Ω, representing the totality of all finite ordinal types as a completed set, was effectively excluded [König 1905b, 160, note 1]. His second solution, the more general one, consisted in distinguishing between the concepts of "set (completed sets) and "classes" (sets in the "act of becoming") [König 1905b, 159].

The main goal, it should be stressed, of König's article was not to solve the paradoxes, but to show that the continuum could not be well-ordered. König's proof was formulated in terms of "finitely defined" elements of the continuum and the complements of sets. First of all he gave a definition of the "finitely defined" element:

> An element of the continuum will be said to be "finitely defined" if, by means of a language capable of giving a definite form to our scientific thinking, we can in a finite span of time specify a procedure (law) that conceptually distinguishes that element of the continuum from any other one. [König 1905b, 159] (English translation in [van Heijenoort 1967a, 146])

König's initial hypothesis in his proof was to suppose that the continuum was a well-ordered set, which he took to be a false assumption. Considering the continuum to be a well-ordered set, the elements that could not be finitely defined formed a subset M of the supposedly well-ordered set. Because the cardinality of the set of all the finitely defined elements of the continuum is $\aleph_0$, and because the continuum is not denumerable, there exist elements of the continuum that cannot be finitely defined. The subset M has to be well-ordered, and therefore contains a first element. But if the continuum is a well-ordered set, then there is a one-to-one correspondence between the continuum and a set of specific ordinals. Because of the one-to-one correspondence, there would have to be a first ordinal that was not finitely defined. But this is impossible, because "there exists a definite (well-ordered) set of finitely defined ordinals that follow each other in an unbroken sequence beginning with the first" (see [König 1905b, 159]; English translation in [van Heijenoort 1967a, 147–148]). König then obtained a contradiction to the assumption that the ordinal in question could not be finitely defined by observing that this ordinal was defined by the phrase "the first ordinal that exceeds all of these in magnitude" (see [König 1905b, 159], English translation in [van Heijenoort 1967a, 148]). This provided the contradiction, following from the supposition that the continuum was well-orderable. Once again, it is important to keep in mind that König—as well as Burali-Forti, Cantor, and Dixon (who is discussed below)—did not claim to have encountered a paradox. He was only trying, unsuccessfully, to prove that the continuum could not be well-ordered. König's subsequent interests evolved in a different direction, but that is part of another story [12].

According to some historians, both Richard and König discovered a paradox, the most remarkable point being that the two contributions "were written during the same weeks, perhaps even the same days, and independently of each other" [van Heijenoort 1967a, 142]. In addition, Copilowish claims that A. C. Dixon published another paradox in an article read before the London Mathematical Society on December 14, 1905 [Copilowish 1948, 85]. However, according to Dixon himself, "the idea on which this article [1907] is founded was mooted by one in a letter to Dr. Hobson in June of this year [1905]. . . . " [Dixon 1907, 20, footnote]. If, in fact, Dixon had discovered another paradox, then the coincidence of independent discovery is all the more remarkable, involving not two but *three* cases. It is true that Richard, König, and Dixon each devised their arguments independently of the others in June of 1905. But König, as explained earlier, considered his argument as part of his denial of the well-ordering of the continuum. Similarly, Dixon did not consider his argument as a paradox, but rather as a justification of the proposition that no transfinite aggregate could be well-ordered by a finite set of rules [Dixon 1907, 19].

As already mentioned, Dixon's ideas were originally motivated by the same article of Hobson's that provoked the polemic among British mathematicians [Dixon 1907, 18]. Dixon thought that is was possible to find the cardinal number of the set of all objects that could be defined in finite terms. What did he mean by "define"? An object was defined, he held, when the properties necessary to distinguish it from all other objects of mental activity were stated. Since the number of elements available for use in any definition is finite, say p (including the symbols of all known alphabets, signs of punctuation, etc.), the cardinal number (or power) of the set of objects described by such finite specifications could not be greater nor less than $\aleph_0$. It was not greater because a nondenumerable set could not be formed with only a denumerable number of elements; it could not be less because the sequence of the natural numbers alone already provided a set of objects finitely defined with cardinality $\aleph_0$. Because the set of all real numbers had a cardinality greater than $\aleph_0$, this meant there had to be elements of the real number system which could not be given a finite definition or specification. However, because of Zermelo's theorem that any transfinite set could be well ordered, each number could be finitely defined. If this were not the case, then

> there must be a first [element] which cannot [be finitely defined], and it may be described in finite terms as "the first in the series determined by the rules R which cannot be described in finite terms." This is absurd. [Dixon 1907, 19]

This argument was immediately applied to Cantor's ordinal numbers and the second number class. Now the absurdity lay in the phrase "the least number of the second class that cannot be specified in finite terms" [Dixon 1907, 19]. Instead of concluding that he found a paradox, Dixon inferred that it was not possible to say that a transfinite aggregate (or set) could be well-ordered by a "finite set of rules." This did not contradict Zermelo's theorem, because Zermelo had simply shown that it was *possible* to provide a well-ordering of a transfinite set without showing *how* to produce a specific arrangement of the set as Hilbert had hoped [Hilbert 1902, 447].

Russell, in his now classic formulation of the theory of types [1908], discussed Dixon's and König's arguments as if they involved contradictions. This is the first such treatment of this sort that we have been able to find, implying that Russell not only created some of the now famous "logical" paradoxes, but also one of the "semantical" ones as well. Russell transformed König's and Dixon's arguments into a simple and direct contradiction: not all ordinal numbers can be finitely defined, because the total number of possible definitions is $\aleph_0$ and there are more than $\aleph_0$ transfinite ordinal numbers. Of these there must be a least, and this is defined by the phrase "the least indefinable ordinal," an absurdity [Russell 1908, 223]. In a footnote Russell indicated König [1905b] and Dixon [1907] as sources, but he did not mention whether either of them was thinking in terms of paradoxes or not.

There is—and has been—confusion in the status of these paradoxes, in particular the so-called König–Zermelo paradox. The terms in which some mathematicians, in particular Jacques Hadamard and Jules Richard, chose to discuss the contradictions between Zermelo's proof of the Well-Ordering Theorem and König's alleged proof of the impossibility of the well-ordering of the continuum, has already been covered. Another French mathematician, Henri Poincaré, also mentioned the Zermelo–König "antinomy" as one of the already numerous contradictions arising from set theory [Poincaré 1906, 333; 1912, 530]. Poincaré was hunting antinomies because he was trying to discredit both Cantor's theory of sets and Russell's logicism. Unfortunately, Poincaré only mentioned the Zermelo–König paradox but did not expand upon or discuss it in detail. It is therefore impossible to know in what context he thought there was a contradiction, or to what extent he thought of these as contradictions—or "antinomies" as he preferred to call them. This seems but one more element in the long list of confusions and misunderstandings related to the origin and development of the set-theoretic paradoxes.

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NOTES

1. Throughout this article, the term *paradox* will be used to indicate an argument that derives self-contradictory conclusions by valid deductions from apparently acceptable premises. The word *contradiction* will be taken to mean "a statement containing propositions one of which denies or is logically at variance with the other" [*Oxford English Dictionary* 1971, Vol. I, p. 539]. Nevertheless, the particular choice of the usage of the words made by mathematicians and philosophers at the beginning of the century will be respected in each particular case. In retrospect, the arguments described in textbooks of the history of mathematics as requiring a reexamination of the foundations of set theory might be called paradoxes. We term "logical" those paradoxes described for the first time in Bertrand Russell's *The Principles of Mathematics* [1903]—i.e., Burali-Forti's paradox, Cantor's paradox, and Russell's contradiction. We will label "semantical" or "nonlogical" paradoxes those arguments not involving logical or mathematical terms but containing some reference to thought, language, or symbolism [Ramsey 1926, 352–354].

2. Cantor defined the second number class as "the totality of all order types α of well-ordered sets of cardinality $\aleph_0$" [Dauben 1979, 202]. Berry claimed that Cantor had proved the well-ordering of the second number class in 1895, but in the first part of the "Beiträge zur Begründung der transfiniten Mengenlehre," Cantor simply indicated that:

> [w]e shall show that the transfinite cardinal numbers can be arranged according to their magnitude, and, in this order, [they] form, like the finite numbers, a "well-ordered aggregate" in an extended sense of the words. [Jourdain 1915, 109]

Russell did recognize the validity of Berry's proof of the well-ordering of the ordinals a year later [Russell 1907, 36, second footnote].

3. Zermelo claimed to have discovered, not only independently but also before Russell did, the contradiction of the set of all sets which are not members of themselves [Zermelo 1908a, 118–119, second footnote]. For a detailed explanation of how Zermelo discovered the contradiction see [Thomas & Rang 1981].

4. Probably Hilbert came upon the topic of the paradoxes as a consequence of his correspondence with Cantor in 1895 or 1896 (Hilbert to Frege, November 7, 1903, printed in [Frege 1980, 51–52]).

5. The proposition in question is

$$\aleph_\alpha^{\aleph_0} = \aleph_\alpha \cdot 2^{\aleph_0} \quad \text{for every ordinal } \alpha.$$

Later on, Felix Bernstein claimed that this result was applicable only in the case of finite α [Bernstein 1905, 463–464], but König did not specify such a restriction.

6. Irving Copi has argued that both articles were already contradictory at the time of the congress. This cannot be, however, because Zermelo's contribution was not developed until after the congress had finished (see [Copi 1971, 10]; see also [van Heijenoort 1967a, 47]).

7. Hadamard probably had in mind the dispute among French mathematicians. Later on, this polemic was published in the pages of the *Bulletin de la Société Mathématique de France* (see [Borel 1912, 150–160]).

8. For Jourdain's claim of the independence of the use of the words, see [Jourdain 1904b, 67, first footnote]. Cantor's similar distinction between consistent and inconsistent multiplicities (aggregates) had been rejected by Hilbert in 1904 because of the lack of "a precise criterion for this distinction" [Hilbert 1904, 131].

9. "Given any class of mutually exclusive classes of which none is null, there is at least one class which has exactly one term in common with each of the given classes" [Russell 1919, 122]. This Multiplicative Axiom—as Russell called it—is needed to prove that the infinite product of (finite or transfinite) numbers is zero when one of the factors is zero.

10. Nevertheless, Russell published another "final" attempt to resolve the contradictions in 1908. This one was based on the theory of types, and proved to be his definitive solution [Russell 1908].

11. There is a millionaire who owns $\aleph_0$ pairs of boots and socks. It would be easy to imagine how many boots he had in total because $\aleph_0 + \aleph_0 = \aleph_0$. This is easy to prove for the case of boots because one can distinguish between a right and a left boot. Therefore, one would have no trouble selecting a boot from each pair, say the left one in each case. Unfortunately, manufacturers of socks do not make any difference between right and left socks. Thus there is no criterion for picking one sock from each pair [Russell 1907, 47–48; 1919, 125–126].

12. In later papers König argued that he had discovered a paradox, reflecting increasingly the influence of Poincaré. Finally he came to accept Zermelo's Well-Ordering Theorem. See [König 1907, 1914; Moore 1979, 211–216].

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