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Labyrinth of Thought:
A History of Set Theory and its Role in Modern Mathematics
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REVIEW

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The language of set theory is so all-pervasive in modern mathematics that it is difficult to imagine how mathematicians ever talked and wrote without it. Yet they did, for thousands of years. People wrote about geometric *figures* rather than sets, or they phrased propositions in terms of points having a particular property, focusing on the individual points rather than their totality and thus ignoring what we would now refer to as the *set* of points having a property. The absence of this useful concept and the concept of membership in a set led to some unfortunate lapses in reasoning. In philosophical writing, for example, an object was sometimes conflated with what we now call the singleton set whose only element is that object, leading to confusion about the meaning of the word *unique*. On the other hand, the notions of set and membership in a set are so primitive that it is easy to find prefigurations of them very far in the past. The story of set theory is therefore best told as the gradual coming into focus of a common intuitive notion. The many independent trends that brought about this focusing and thus created modern set theory form the subject of the book under review.

The author's title comes from his epigram, which in turn is a quotation from Jorge Luis Borges's 1981 book *La cifra*. Discussing his reading in the subject of set theory, Borges says, "It was not given to me to enter that delicate labyrinth." And what a labyrinth it is! In studying it, the reader is constantly confronting topics that could be said to belong to logic, topology, real analysis, algebra, geometry, and so on. If you take any convenient turning, you will soon encounter another, and there simply is no systematic way to explore the entire place. (If I might be allowed to mix classical literary references, I would refer to this labyrinth rather as an Aladdin's Cave for the treasures of thought it contains.) The classical allusion to the labyrinth is immediately followed by another, in the introduction. The author begins by explaining that set theory is too often made to appear as if it sprang fully formed from the head of Cantor, like Athena from the head of Zeus. He might have continued these pleasant images by telling us that he proposed to play the role of Ariadne to the reader's Theseus, with this book as the thread to guide the reader out of the labyrinth. In the classical myth Ariadne also gave Theseus a sword to slay the minotaur in the labyrinth. But here it might be wise to stop and let the readers of this review supply their own parallels.

Most of the events described in this book occurred in the century between 1850 and 1950. The five chapters in Part 1 discuss the early period, up to the early 1870s, in which a number of classical areas of mathematics — algebra, analysis, and geometry — came under the scrutiny of mathematicians determined to organize them coherently and make them logically secure. Thus, even in its origins, set theory (the organizing part) was entangled with mathematical logic. Part 2 consists of three chapters discussing the crystallization of set-theoretic ideas, especially the notion of cardinal number, over the next two decades. The final part, also containing three chapters, discusses the spread of set theory in the work mostly of Continental mathematicians, the problems surrounding its logical basis, especially the Axiom of Choice, and attempts to systematize the systematization by formulating a reasonable set of axioms for set theory.

The reader should know immediately that this is going to be a laudatory review. The author paints on a grand scale. He sees clearly, and he sees whole. The result is a spacious canvas full of intriguing scenes and portraits from the history of set theory, seamlessly juxtaposed to form a fascinating and accurate picture of a vast area of modern mathematics. It is a must-have book for anyone who wishes to gain a balanced picture of this history. It is written in clear and elegant language for the learner, while experts in the area will enjoy seeing this beautiful presentation of what they already know, perhaps arguing about some of the author's conclusions and choices of material. The review that follows is an attempt to explain the structure of the book and some of the author's main points, together with an indication of what is necessarily left out since it could not be included without writing a much longer book.

In relating any history of mathematics it is necessary to take account of the currently popular view of the subject. As mentioned above, in the case of set theory, there is a widespread view that credits Cantor with the primary rôle as its creator. While not minimizing Cantor's contributions, the author takes pains to present the earlier and contemporary work of a large number of other authors who made significant contributions to set theory, thus presenting a richer, multi-dimensional picture to replace the predominant one-dimensional version.

One thread in the tapestry of set theory is easy to trace: the connection with the theory of uniqueness of multiple trigonometric series. This thread has one end in an 1854 paper by Riemann that was not published until 1867, after Riemann's death. This thread breaks off in 1916, but resumes in the 1980s.

In the 1854 paper Riemann considered the question whether there could be a trigonometric series with some of its coefficients different from zero that converged to zero at every point. Riemann assumed what Cantor was later to show could be proved, namely that the coefficients of the series necessarily tend to zero if the series converges. (Kronecker pointed out that the general case could be deduced from the case in which the coefficients tend to zero, so that it was in fact not necessary to prove that they tend to zero.) By performing formal term-wise integration on the supposed series twice, Riemann produced a series that was a Fourier series, that is, one whose coefficients were obtained by integrating the product of a continuous function $F(x)$ with the corresponding trigonometric functions. If one could show that all the coefficients of this Fourier series were zero, it would immediately follow that all the coefficients of the original series were also. Riemann's technique for doing so was to introduce a generalized second derivative $D_2F(x)$ which is zero wherever the original series converges to zero. This generalized second derivative has in common with the ordinary second derivative that, if it vanishes on an interval, then $F(x)$ is linear on that interval. Thus, if $D_2F(x)$ vanishes identically, as followed under Riemann's assumption that the original series converges to zero everywhere, then $F(x)$ had to be a linear function on the entire line. That, in turn, meant that a polynomial had to be equal to the sum of the trigonometric functions, which was a function of period 2π. Since the only periodic polynomial is a constant, it followed that the periodic function was constant and then easily that all the coefficients had to be zero.

An important piece of set theory arose from the attempt to strengthen this result by allowing the series to diverge or converge to a non-zero limit for a finite number of points. Riemann's reasoning is still valid. The function $F(x)$ is still continuous and linear in each interval whose endpoints are the finite number of exceptional points. But then, Riemann's *second theorem* comes into play. This theorem asserts that the function $F(x)$ cannot have a corner, that is, if it has a one-sided derivative at any point, then it has a two-sided derivative. As a result, if this function is linear on $[a, c]$ and also on $[c, b]$, then it is in fact linear on $[a, b]$. This result made it possible to allow isolated exceptional points in the hypothesis of convergence to zero, since they do not invalidate the argument that $F(x)$ is globally linear.

It is the step from a finite number of exceptional points to an infinite number (undertaken by Cantor in 1872) that connected with set theory. Allowing an infinite number of exceptional points brings us up against the Bolzano–Weierstrass theorem, producing at least one point of accumulation for the exceptional points. Cantor knew this fact, having attended Weierstrass' course on analytic function theory in 1864. The isolated exceptional points cause no problems at all, so that the function $F(x)$ is still continuous, and still piecewise linear on intervals whose endpoints are now the points of accumulation of the exceptional points. If these intervals are finite in number, once again, the no-corners principle applies, and the uniqueness theorem continues to hold. Hence when the exceptional set has only finitely many points of accumulation, the proof continues to hold, and one is led to consider the case when the exceptional set has infinitely many points of accumulation. It was in connection with this train of ideas that Cantor began to speak of a *Wertmenge* (set of values) and a *Punktmenge* (set of points).

It was Cantor who first gave a name to the concept of a point of accumulation. Weierstrass had simply said that when a quantity can assume infinitely many values within finite limits, there would be a point every neighborhood of which contains infinitely many of the values. Observe how neatly this phrase talks around the idea of an infinite set without ever mentioning sets. Very soon after he first looked at the problem of allowing infinitely many exceptional points for convergence of a trigonometric series, Cantor had introduced the concept of a point set and its derived sets of all finite orders. The uniqueness theorem is obviously valid if the exceptional set is, as Cantor described it, *von der ersten Gattung* (of first kind), that is, some derived set of finite order is empty. Looking beyond this point, Cantor saw that, since all the derived sets after the first were nested, it would be possible to talk about sets of second kind, that is, sets of infinite order. One could define the derived set of infinite order to be the intersection of all the sets of finite order, then start over again (at "infinity plus one") with a new derived set. In other words, the germ of the later idea of a transfinite ordinal number was contained in the notion of derived sets of sets of real numbers.

Looked at in isolation, this story seems to justify the large amount of credit traditionally assigned to Cantor for originating the basic concepts of set theory. All this history is duly given in Chapter V of the present book. Two points, however, show how wrong such a conclusion would be. First, Cantor abandoned the problem of uniqueness of trigonometric series in the early 1870s, yet he continued to develop set theory for the rest of his life. Second, Cantor did not create the notion of set *ex nihilo*. In the first four chapters we learn just how much Cantor was indebted to earlier and contemporary mathematicians.

Dedekind, for example, came to set theory via algebra. He seems to have been the first (in the years 1855–1858) to realize that groups could be studied as completely abstract objects. All one needed was a set — Dedekind called it variously a *domain* (*Gebiet*) or a *complex* — made up of a finite number of *elements*, *things* (*Dinge*), or *concepts* (*Begriffe*), on which some binary composition was defined. He regarded number fields in the same way, and thus met the author's major criterion for serious involvement with set theory, the consideration of actual infinities. Dedekind's set theory included a theory of mappings (which he called *substitutions*). Although the details are harder to trace, since Dedekind was a perfectionist in relation to publication, it is known that Dedekind also reworked much of Riemann's material on manifolds, especially those of nonconstant curvature, and even attempted to prove the Dirichlet Principle, seven years before Weierstrass published his critique of this principle. The connection of this material with Dedekind's set theory is not completely established, but the author refers to some papers in the Dedekind *Nachlass* bearing the title "Ideale Geometrie" which may shed further light on the matter. Thus, although he used such words as *domain* or *system* rather than *set*, Dedekind took the crucial step of regarding a collection of objects as a single object, and he allowed the collective object to have infinitely many members.

The author finds prefigurations of set theory even further back than Dedekind, in the work of Gauss and Bolzano. Bolzano is quoted (p. 75) as having used the word *set* in a sense that seems modern in a posthumously published (1851) paper in which he showed that intervals of different lengths could be in one-to-one correspondence. (Of course, Galileo had shown this two centuries earlier.) However, he argued that this correspondence did not establish the equality of the two sets.

The author emphasizes the influence of Riemann on both Dedekind and Cantor. In the case of Dedekind, who edited Riemann's *Nachlass*, there is no doubt about this influence. In the case of Cantor, besides the obvious work that Cantor did on the theory of uniqueness of trigonometric series, there is a second, less obvious influence, which the author finds in Riemann's study of manifolds of various dimensions. He notes particularly Riemann's consideration of discrete manifolds. This alleged connection with set theory would not quite have rung true to the reviewer before reading this book, despite Riemann's well-known background in philosophy. Riemann's work is so rich in classical geometric and analytic content that it is difficult to see it as having even a psychological connection with later abstractions. Even the works on geometry and complex function theory, which explicitly contain the words "foundations" in their titles, always seemed to me to aim at getting a clear and intuitive way of thinking about very concrete and classical mathematical objects and, even more importantly, breaking open new areas for study in these areas. They do not have any "point-set" aspect that I could discern. Two telling points of the author have caused me to revise this opinion.

First, the author points out that Riemann regarded a manifold as a single object made up of points, based on the notion of concept-extension. I confess that I have never been able to associate any clear idea with the phrase *concept-extension*, and for that reason would be inclined to look elsewhere for the roots of set theory. However, I am impressed by the fact that Riemann adumbrated the abstract notion of a geometric manifold. By focusing on the parametrization of such a manifold, one might be led to leave the actual nature of the object being parametrized vague, effectively regarding it as an abstract point set, since all the important information about it is encoded in the parameters. Second, and most convincingly, the discussion (in Chapter VI) of Cantor's work on dimensionality and cardinality shows clearly that Cantor was looking at Riemann's discussion of manifolds with a critical eye and had laid his finger on the crucial point, namely the question of invariance of domain. In the 1870s Cantor tried to do what was impossible at that time: prove that dimension is invariant under continuous invertible mappings. His theorems asserting that the real line is not of the same cardinality as the integers, while all continua have the same cardinality, represented a large leap forward in the understanding of what was and was not implied by dimension. Actually, the author's discussion of the nondenumerability of the reals (pp. 176–183) left me with the impression that this particular question did not arise in the course of Cantor's development of set theory, but had been on his mind since his student days, independently of his other interests.

The author notes that the publication of Riemann's paper on trigonometric series, which contained the definition of the Riemann integral and its explicit validity for some discontinuous functions, was simultaneous with the publication of his work on manifolds. Thus, in two areas, Riemann's work led to the consideration of objects that were significantly more abstract than those considered previously. The reviewer has always regarded the paper on trigonometric series representations as being anomalous in the canon of Riemann's works. Compared with his work on abelian functions, geometry, Riemann surfaces, and so forth, it has a peculiar "real-variable" flavor. I have always attributed this quality to the fact that Riemann was assigned this topic; it was not one that he chose himself. In contrast to all his other work, in this one paper he gives copious citations of earlier literature (provided to him, we now know, by Dirichlet, who had set him the problem in the first place).

However that may be, the author makes the point that "Riemann's abstract-conceptual approach to mathematics may have paved the way for the development of abstract set theory." After all, those abstract objects have to be thought of *somehow*. Given that Riemann was willing to consider even infinite-dimensional manifolds, it is unlikely that he regarded them as embedded in some Euclidean space. However Riemann himself thought of them (as objects of pure intuition, perhaps), later mathematicians, as we know, did eventually turn them into pure abstractions, for which set theory was the necessary basis. When all is said and done, however, it is very clear that Riemann's work focuses on the specific, whereas Dedekind and Cantor were systematizers. The tone and general trend of Riemann's thought is very different from that of Dedekind and Cantor. The latter two were inspired by Riemann's thought, and that is as it should be: the specific should inspire systematization. There is creativity in both, and both are needed for the healthy growth of mathematical knowledge.

It has always seemed to the reviewer that the biggest problem set theory faced at the outset was not attaining philosophical respectability, but rather showing that it made possible the solution of some interesting problems. In one of her letters to Mittag-Leffler, Sonya Kovalevskaya takes pains to point out that Hurwitz had made elegant use of Cantor's research to prove that a meromorphic function of several variables is necessarily a rational function. Toward that same end, the use of set theory by the French mathematicians Baire, Borel, Lebesgue, Fatou, and others to give a systematic treatment of the theory of measure and integration was an epoch-making event. This work was still in progress at the time of the famous confrontation between Cantor and

König at the 1904 International Congress, which in turn was followed by the rather acrimonious discussion of the Axiom of Choice.

The author does a very good job of giving the details of all this history, and these details lead to a tiny mystery that may interest the reader. In 1878 Cantor had used a zigzag curve to show that a closed interval can be placed in one-to-one correspondence with a half-open interval, from which he deduced that a closed interval and an open interval can be in one-to-one correspondence. This result, of course, follows easily from what is commonly called the Schröder–Bernstein theorem. However, as the author relates on p. 239, Schröder's alleged proof of this theorem did not appear until 1896, and it contained a mistake, so that the theorem is more properly the Cantor–Bernstein theorem. Thus, Cantor was obliged to construct an explicit one-to-one correspondence between the two, and he took the time to be very analytic. That is, he didn't do what any undergraduate would do nowadays: extract a countable subset from the interval, shift it by one index, and map one endpoint onto the now-vacant first term of the sequence. Had he done so, there would have been no hope of getting the result accepted by the mathematicians of the time. The author reports (p. 312) that at the third International Congress in 1904, Julius König announced a proof that the cardinality of the continuum is not any aleph (which would, of course, refute the Continuum Hypothesis). As Cantor had been able to prove that every cardinal number is an aleph by assuming that every set can be well ordered, König's result was, as Gerhard Kowalewski wrote [2, p. 202], "in conflict with two of Cantor's basic beliefs." Naturally Cantor was shaken by this revelation. Kowalewski reports that "there was also gratitude to God for allowing him to live long enough to see his mistakes revealed."

So much is told by the author. As it happens, there is another account of this confrontation in the Russian literature. A letter from Nikolai Luzin to his friend Pavel Florenskii [1] reports second-hand what Luzin had heard about this conference from A. A. Volkov. As Luzin wrote,

> . . . Cantor announced that he had succeeded in mapping an interval without endpoints onto an interval with endpoints, but he hasn't given the proof yet. Then some other mathematician, a young man, rose and said that the possibility of this mapping entails paradoxes and gave arguments in support of this. Cantor, who had listened to him in great agitation, declared that these were the most critical moments of his life, and that he would think further . . .

It seems unlikely that Cantor would be badly shaken about two entirely different matters at the same conference, even though he was, as is well known, a very nervous man. Most likely there was confusion in the transfer of information from Volkov's memory to Luzin to Florenskii. The assertion that Cantor "hadn't given the proof yet" is certainly wrong. Probably Volkov was not current on the subject and so didn't understand all that was being said. He obviously did not recognize König or remember his name, if the discussion he is reporting is the same one reported by Kowalewski. It would be interesting to know more details, but they are not likely to be available now, a century later. It seems probable that the correspondence between an interval without endpoints and an interval with endpoints, mentioned above, was advanced by Cantor as an established fact, intended to refute König's argument, and that König (if it was he) replied that such a mapping involved paradoxes, and therefore must be erroneous. The two humble replies attributed to Cantor by the two witnesses both ring true. One suspects that he always half-feared that the edifice of set theory might come crashing down.

According to Kowalewski, the flaw in König's argument, traceable to an error of Felix Bernstein, was pinpointed the very next day by none other than Zermelo, who immediately gave two proofs of the well-ordering principle. The author, however, points out how unlikely that scenario is and conjectures that Kowalewski's memory probably deceived him.

It is a revealing fact that the controversy over the Axiom of Choice, and the enormous amount of literature it generated arose at just this point. There is no logical reason why the axiom could not have been questioned earlier, since proofs involving arbitrary choices were certainly used earlier. It must have been the abstraction of the concepts involved that inevitably led to the explicit formulation and dispute over this axiom. The fact that such proofs were and are so common is well illustrated by the fact that Gregory Moore's encyclopedic study of the history of this axiom ([4]) contains huge amounts of the history of general set theory and real analysis.

In a famous paper published in the *Comptes Rendus* in 1916 and enlarged in the *Bulletin* of the Cracow Academy of Sciences in 1918, Sierpiński revealed the ubiquitous nature of this axiom. The reactions to his survey were vehement. The reviewer read one such reaction, by Luzin, in the Archives of the Soviet Academy of Sciences in 1989.[1]

[1] *Fond* 606, *opis'* 1, *edinitsa khraneniya* 57. The notes bore the date January 1917. Sierpiński had been caught in Moscow at the outbreak of World War I and

To Luzin the central issue in clarifying the Axiom of Choice was the meaning of the German word *Existenz.* He wrote:

> We see that in the absence of an *analytic*[2] (non-*auswahlic*)[3] rule (which is the only thing that could give us confidence in the existence of the required class), the existence of a class becomes mysterious and the problem is actually the question of the validity of this existence and the very *meaning* of this existence. An analysis of the word "existence" would be interesting! Philosophically it denotes absolute being. Only I don't know whether that is equivalent to objective being. To exist does not at all mean "to be an object of our thought." It is something more, since even a contradiction can be an object of our thought, and it is deprived of existence. Indeed, we speak of objective existence of the same degree of certainty as the existence of any mathematical object (in the earlier sense), such as a straight line or a circle.
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> There are two types of *existenz*: First, a thing exists because it is *analytically* defined for everyone; here we do not care what particular analytic procedure is used for the definition; all that matters is that the definitions be *analytic*; the actual procedure is a matter of indifference to us, and hence so are the logical functions and procedures by means of which this analytic definition is accomplished. We require only operations with arbitrariness eliminated. Second, a thing exists by virtue of the Axiom of Zermelo, that is, it exists, although it cannot be analytically defined. That is the true meaning of Zermelo's Axiom. It contains the concept of "existenz," and therefore everything reduces to uncovering the content of that concept.

At this point it will be well to confess a naïve philosophical prejudice. To the reviewer much of this passage seems to be based on bad philosophy. The meanings of words are determined by their usage, and the phrases "absolute being" and "objective being" have no usage outside

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had participated in Luzin's seminar. Luzin admitted that he found Sierpiński's survey "horrifying."

[2]The Russian word is *obshcheobyazatel'nyi*, which could be literally translated "by general obligation," that is, *a priori*, but Luzin used this word as a translation of Lebesgue's *analytique*.

[3]A word coined by Luzin — *ausvalicheskii* in Russian — to denote the use or non-use of the Axiom of Choice (*Auswahlprinzip*) in the definition.

the ethereal word-spinning of philosophers like Hegel. Because of that, they are simply nonsense. Luzin very wisely confined these speculations to his own notebooks, not only because it was personally dangerous to speculate on such inexact matters in his day, when deviance from the state-prescribed metaphysics could carry the death penalty, but also because he realized that they were not well enough formed to be real mathematics or even mathematical philosophy.

Unfortunately, along with this material, Luzin also suppressed some very acute insight into the Continuum Hypothesis. For example, everybody knows the famous diagonal proof that the interval $[0, 1]$ is uncountable.[4] One imagines any sequence in $[0, 1]$. Then by imagining a change in the nth digit of the decimal expansion of the nth number in the sequence, one "constructs" a number not in the sequence. Luzin pointed out that this proof does not show that no enumeration of $[0, 1]$ exists, since the construction of the new number depends on more than the mere existence of the sequence: it requires that the sequence be *explicitly enumerated*. Thus, as far as the proof shows, a one-to-one correspondence between $[0, 1]$ and the positive integers might possibly "exist" in some abstract sense. It just couldn't be explicitly written out.

This same idea, distinguishing carefully between what *exists* and what can be *named*, apparently occurred to Lebesgue as well, and was used by him to deny that some of the uses of the Axiom of Choice were essential (see [4, p. 288]). In particular, Lebesgue argued that as long as explicit enumerations were required, the theorem that a countable union of countable sets is countable remained valid. The usual argument that this proof requires the Axiom of Choice is that one must *choose* an enumeration of each set in the collection in order to enumerate the whole. Lebesgue (1921) argued that the choice would be unique (and hence not a choice) if each set was explicitly enumerated. Luzin later (1926) distinguished between two cases that he called *Lebesgue choice* and *Zermelo choice*, and he said explicitly that "applying free choice amounts, in my opinion, to juggling combinations of empty words, whose meaning does not correspond to any intuitively accessible fact."([3, p. 279]) The author (p. 316) cites Hadamard's comment that Lebesgue's insistence on a *rule* (explicit enumeration) reminded him of the earlier discussion of what was required in order to define a function. The reviewer is strongly reminded of that earlier controversy, in which Weierstrass insisted that analytic functions must

[4]This proof is not Cantor's original proof, which relied on the nested set principle. However, Luzin's argument would apply equally well to that argument.

be defined by power series. To any modern mathematician, this seems to be starting "too far into" the subject. We are all used to considering abstract unspecified functions possessing only the property that they have a complex derivative at every point of an open set. From that point, via the Cauchy integral, we *prove* that they have convergent Taylor series. But there is more to be said for Weierstrass than at first appears. In any application of Cauchy's theory to a specific function we have to be told, somehow, what the function is. How is that to be done, without using some elementary or standard expressions? That seems to be Lebesgue's position on the Countable Union theorem: any *specific* countable union that could be defined to his satisfaction would surely be explicitly enumerable. If one is allowed to assign a double meaning to the word *analytic*, Weierstrass' dictum is appropriate here: "No matter how you twist and turn, you cannot avoid using specific analytic expressions."[5]

To return to the philosophical aspects of the author's work, however, the reviewer was especially pleased to see that the discussion of Russell's paradox on pp. 307–308 includes the very simple train of thought by which Russell was led to discover it, trying to refute Cantor's proof that the power set of any set is of larger cardinality than the set itself. The development of this thought is very clearly marked in Russell's publications, especially his article "Recent work in the philosophy of mathematics," published in the *International Monthly* in 1901, the very year in which the paradox was discovered. In this article he challenged Cantor's proof with the set of all sets and asserted that "the master has been guilty of a subtle fallacy," which Russell proposed to discuss in more detail elsewhere. When this article was reprinted in *Mysticism and Logic* in 1921, Russell added a footnote explaining how he had been wrong. This interesting and easy material gets ignored in the accounts of Russell's paradox in all the standard textbooks of set theory, which present it as a mere fact devoid of all historical context.

The reviewer can find very little to criticize in the book under review. It is not to be expected that every sentence in a book of this size will be perfectly accurate. The present book, however, comes very close, as far as I am able to judge. One might pedantically point to the statement (p. 157) that "Weierstrass realized that uniform convergence was a necessary condition for term-by-term integrability of a series." The author, I am sure, knows better; and this statement was merely

[5] "Wie man sich drehen und wenden mag, man kommt nicht darüber hinweg, bestimmte analytische Formen... zu benutzen." He is said to have made this remark in 1885.

a matter of careless drafting. What I suspect the author meant was that Weierstrass realized that some additional hypothesis was necessary to guarantee term-by-term integrability, and that uniform convergence was a sufficient condition.

The author has chosen his topics well and presented an integrated history of a vast amount of important material. The following comments are not intended as criticism, but only as a reminder that, even on a canvas as large as the present book, it is impossible to paint every aspect of the history of set theory. In selecting the material for the book the author chose to exclude most of the history of descriptive set theory. (Had he not done so, the book would have been at least one-third longer.) This topic is relegated to a section of Chapter IX, under the heading "other developments in set theory," and is rather quickly summarized as follows (p. 333):

> Cantor proved for closed sets a decomposition $P=R\cup S$ into a denumerable set and a perfect subset, which implies that the Continuum Hypothesis (CH) holds in this case... In his dissertation of 1901, Bernstein was among the first to work on generalizing that result. This kind of work studied sets of reals that are definable in different ways, and led to descriptive set theory. The contributions of the French analysts Borel, Baire, and Lebesgue, in their study of real functions and integration, merged with that line of development ... Descriptive set theory emerged from about 1915 with the work of the Moscow school headed by Luzin, to which Aleksandrov and Suslin belonged.

This rapid summary does not make clear why Suslin is mentioned. Aleksandrov had invented an operation called the A-operation (A for analytic, although Aleksandrov later claimed it was for Aleksandrov), which made it possible to label each Borel set with a sequence of pairs of integers. By use of this operation, he was able to show that an uncountable Borel set of real numbers contains a non-empty perfect set, and hence must have cardinality of the continuum. Suslin realized that this same proof actually applied to a possibly larger class of sets, which came to be called *analytic sets*, the word reflecting the belief that they could be defined analytically in the sense of Lebesgue, that is, without using the Axiom of Choice. Luzin proved that a set is a Borel set if and only if both the set and its complement are analytic. The question whether the complement of an analytic set (called a *co-analytic* set) is analytic immediately became important. If so, then analytic sets would merely be Borel sets. It is this work that the author refers to as the emergence of descriptive set theory. In his discussion of Gödel's relative consistency results of 1938 (p. 382) the author (again, quite properly for his purposes) mentions that the Axiom of Choice and the Generalized Continuum Hypothesis were proved to be consistent relative to the Zermelo–Fraenkel system. However, Gödel actually showed that, without introducing any new inconsistency, one could adjoin *four* new assumptions to the ZF system, one of which was the existence of a co-analytic set for which the continuum hypothesis is false (see [4, p. 280]). Thus Gödel, in addition to putting an end to attempts to disprove the Axiom of Choice or the Continuum Hypothesis, also put an end to attempts to prove that co-analytic sets are analytic. In contrast to the date of 1915 assigned by the author, the reviewer is more inclined to date descriptive set theory from the turn of the century, in the work of the French analysts that he mentions. It was Baire (1898) who began classifying functions as continuous, pointwise limits of continuous functions, pointwise limits of pointwise limits of continuous functions, and so on. In his 1902 dissertation and lectures, Lebesgue used Baire's classification of functions to prove that what he called a Borel set (that is, a set formed by finitely many repetitions of countable unions and intersections, starting from the open sets) would have a characteristic function belonging to a finite Baire class. Later Lebesgue was to allow Borel sets to be formed by countably many countable operations of union and intersection; as a result, he obtained an even more elegant correspondence between Borel sets and Baire functions. This work was absorbed by the Moscow mathematicians just as Luzin entered the University of Moscow and was expounded in a 1907 monograph by V. L. Nekrasov bearing the title *Structure and Measure*. All this research involved the classification and study of sets according to their complexity, which is the essence of descriptive set theory, and hence gave that topic an existence in its own right, separate from the general problem of measure. In support of the author's choice of epoch, however, it should be said that the explicit construction of examples of sets at a given rank in the Borel hierarchy was one of the preoccupations of Luzin and his students, especially Nina Karlovna Bari and Lyudmila Vsevolodvna Keldysh in the 1920s and 1930s.

Similarly, the process of proving the Continuum Hypothesis for a class of sets by showing that an uncountable set of the class contains a perfect subset had a more gradual development than one would surmise from the author's statement (p. 333) that "Hausdorff [1914] defined a now classic hierarchy of Borel sets and in [1916], simultaneously with Aleksandrov [1916], established the pathbreaking result that any uncountable Borel set of reals has a perfect subset." Actually, the result cited had been proved for G_δ-sets by W. C. Young in 1906, and the 1914 classification by Hausdorff mentioned by the author contained a second proof of this fact. It was Hausdorff who invented the term "G_δ-set." Again, to be fair, the author does note that Bernstein had begun the generalization of this theorem in 1901.

The author's discussion of trigonometric series in connection with set theory is confined mostly to the discussion of Riemann's work and the early work of Cantor and Heine. This is a sensible choice, given that the later connections between the two are less fundamental than these earlier ones. There were, however, important connections between set theory and the problem of uniqueness of trigonometric series representations at both ends of the twentieth century. For example, in 1908 Felix Bernstein proved that the important lemma quoted above for the proof of Riemann's uniqueness theorem, namely that a continuous function F for which $D_2F(x) = 0$ on an interval must be linear, remains true when exceptional points are allowed, provided these exceptional points form what Bernstein called a *totally imperfect* set; that is, a set containing no non-empty perfect subset. (Obviously, any countable set is totally imperfect.) Simultaneously with the Aleksandrov–Hausdorff proof of the Continuum Hypothesis for Borel sets, D. E. Men'shov, a participant in Luzin's seminar in 1915–1916, exhibited a trigonometric series with non-zero coefficients tending to zero that converged to zero almost everywhere, thereby answering an important question left open in Luzin's famous dissertation "Integration and the trigonometric series." The attempt to draw a clear line between closed sets of uniqueness and closed sets of multiplicity continued for a very long time. Essentially the only uncountable closed sets of uniqueness that can be exhibited are countable unions of sets of *Hardy–Littlewood–Steinhaus* type, called $H^{(n)}$-sets, and the only non-trivial sets of multiplicity exhibited have been constructed as the sets on which a Fourier–Stieltjes series with coefficients tending to zero either diverges or converges to a nonzero limit. The gap between these two classes is enormous. In the 1980s descriptive set theory shed important light on this question by producing what amounts to a metatheorem asserting that no simple criterion can be both necessary and sufficient for a closed set to be a set of uniqueness. Specifically, Solovay and Kaufman proved that in the metric space of closed subsets of the circle with the fractal metric, the sets of uniqueness form a co-analytic set that is not analytic.

The physical layout of the book is excellent. Birkhäuser has done, as usual, a beautiful job with typesetting and graphics. The only flaw the reviewer detected was an apparent font problem that caused the accented ń in Sierpiński's bibliography entry to be replaced with ellipsis marks. The book *feels* good: solid and substantial, like its contents, and it is a pleasure to look at.

In summary, the author has written a brilliant book, carefully analyzing all the sources and meanderings of the stream of set theory. Although the work of Russian and nineteenth-century British authors is treated rather briefly, the overall picture is both accurate and fascinating. The book is a true pleasure to read, and the bottom line is that everyone who wishes to be well-informed on this important subject should read it.

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