Appendix 1

Five Letters on Set Theory

These letters are our translation of Baire *et alii* 1905, printed here with the kind permission of the *Société Mathématique de France*. They are discussed in 2.3. The original pagination is indicated in the margin and by two oblique lines in the text.

I. *Letter from Hadamard to Borel* 261

I have read with interest the arguments that you put forward (second issue of volume LX of *Mathematische Annalen*) against Zermelo's proof, found in the previous volume. However, I do not share your opinion on this matter. I do not agree, first of all, with the comparison that you make between the fact which Zermelo uses as his starting point and an argument which would enumerate the elements of the set one after another *transfinitely*. Indeed, there is a fundamental difference between the two cases: The latter argument requires a sequence of successive choices, *each of which depends on those made previously*; this is the reason why it is inadmissible to apply it transfinitely. I do not see how any analogy can be drawn, from the point of view which concerns us, between the choices in question and those used by Zermelo, which are *independent of each other*.

Moreover, you take exception to his procedure for a *non-denumerable* infinity of choices. But, for my part, I see no difference in this regard between the case of a non-denumerable infinity and that of a denumerable infinity. The difference//would be evident if the choices in question depended on 262 each other in some way, because then it would be necessary to consider the order in which one made them. To me the difference appears, once again, to vanish completely in the case of independent choices.

What is certain is that Zermelo provides no method to carry out *effectively* the operation which he mentions, and it remains doubtful that anyone will be able to supply such a method in the future. Undoubtedly, it would have been more interesting to resolve the problem in this manner. But the question posed in this way (the effective determination of the desired correspondence) is nonetheless completely distinct from the one that we are examining (does such a correspondence exist?). Between them lies all the difference, and it is fundamental, separating what Tannery[1] calls a *correspondence* that can be *defined* from a correspondence that can be *described*. Several important mathematical questions would completely change their meaning, and their solutions, if the first word were replaced by the second. You use correspondences, whose *existence* you establish without being able to *describe* them, in your important argument about [complex] series which can be continued along an arc across their circle of convergence. If only those entire series were considered whose law of formation can be described, the earlier view (*i.e.*, that entire series which can be continued along an arc across their circle of convergence are the exception) ought, in my opinion, to be regarded as the true one. Furthermore, this is purely a matter of taste since the notion of a correspondence "which can be described" is, to borrow your expression, "outside mathematics." It belongs to the field of psychology and concerns a property of our minds. To discover whether the correspondence used by Zermelo can be specified *in fact* is a question of this sort.

To render the existence of this correspondence possible, it appears sufficient to take *one* element from any given set, just as the following proposition A suffices for B:

263 A. *Given a number x, there exists a number y which is not//a value obtained from x in any algebraic equation with integer coefficients.*

B. *There exists a function y of x such that, for every x, y is not an algebraic number and is not a value obtained from x in any algebraic equation with integer coefficients.*

Undoubtedly, one can form such functions. But what I claim is that this fact is in no way necessary in order to assert the correctness of theorem B. I believe that many mathematicians would not take any more trouble than I do to verify this fact before using the theorem in question.

J. HADAMARD

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[1] *Revue générale des Sciences*, vol. VIII, 1897, p. 133ff.

II. *Letter from Baire to Hadamard*

Borel has communicated to me the letter in which you express your viewpoint in the great debate resulting from Zermelo's note. I beg your indulgence in presenting some thoughts that it suggested to me.

As you know, I share Borel's opinion in general, and if I depart from it, it is to go further than he does.

Let us suppose that one tries to apply Zermelo's method to the set M of sequences of positive integers. One takes from M a distinguished element m_1; there remains the set $M-\{m_1\}$, from which one takes a distinguished element m_2; and so on. Each of these successive choices, indeed, depends on those that precede it. But, so you say along with Zermelo, the choices are independent of each other because he permits as a starting point *some choice of a distinguished element in EVERY subset of M*. I do not find this satisfactory. To me it conceals the difficulty *by immersing it in a still greater difficulty.*

The expression *a given set* is used continually. Does it make sense? Not always, in my opinion. As soon as one speaks of the infinite (even the denumerable, and it is here that I am tempted to be more radical than Borel), the comparison, *conscious or unconscious*, with a bag of marbles passed from hand to hand must disappear completely. We are then, I believe, in the realm of *potentiality* [dans le *virtuel*].//That is to say, we establish conven- 264
tions that ultimately permit us, when an object is defined *by a new convention*, to assert certain properties of this object. But to hold that one can go farther than this does not seem legitimate to me. In particular, when a set is given (we agree to say, for example, that we are given the set of sequences of positive integers), *I consider it false to regard the subsets of this set as given.* I refuse, *a fortiori*, to attach any meaning to the act of supposing that a choice has been made in every subset of a set.

Zermelo says: "Let us suppose that to each subset of M there corresponds one of its elements." This supposition is, I grant, in no way contradictory. Hence all that it proves, as far as I am concerned, is that we do not perceive a contradiction in supposing that, in each set which is defined for us, the elements are positionally related to each other in exactly the same way as the elements of a well-ordered set. In order to say, then, that one has established that every set can be put in the form of a well-ordered set, the meaning of these words must be extended in an extraordinary way and, I would add, a fallacious one.

In the preceding paragraphs I have only managed to express my thinking very incompletely. I stated my viewpoint in the letter that Borel cited in his note. For me, progress in this matter would consist in delimiting the domain of the definable. And, despite appearances, in the last analysis everything must be reduced to the finite.

R. Baire

III. *Letter from Lebesgue to Borel*

You ask for my opinion about Zermelo's note (*Math. Annalen*, vol. LIX), about your objections to it (*Math. Annalen*, vol. LX), and about the letter from Hadamard that you communicated to me. Here is my reply. Forgive me for being so lengthy; I have tried to be clear.

First of all, I agree with you on the following point: Zermelo has very cleverly shown that we know how to resolve problem A:

265 // A. *To put a set M in the form of a well-ordered set,*

whenever we know how to resolve problem B:

B. *To assign to each set M′, formed from elements of M, a particular element m′ of M′.*

Unfortunately, problem B is not easy to resolve, so it seems, except for the sets that we know how to well-order. As a result, we do not have a general solution to problem A.

I strongly doubt that a general solution can be given to this problem, at least if one accepts (as Cantor does) that to define a set *M* is to name a property *P* which is possessed by certain elements of a previously defined set *N* and which characterizes, by definition, the elements of *M*. In fact, with this definition, we know nothing about the elements of *M* other than this: They possess all the *unknown* properties of the elements of *N* and they are the only ones that possess the *unknown* property *P*. Nothing about this permits two elements of *M* to be distinguished from each other, still less to be arranged as they would need to be in order to resolve A.

This objection, made *a priori* to any attempt to resolve A, obviously disappears if we particularize *N* or *P*. The objection disappears, for example, if *N* is the set of numbers. In general, all that one can hope to do is to indicate certain problems, such as B, whose resolution would entail that of A and which are possible in certain particular cases that are rarely encountered. In my opinion, this is why Zermelo's argument is interesting.

I believe that Hadamard is more faithful than you are to Zermelo's thought when he interprets this author's note as an attempt, not to resolve A effectively, but to demonstrate the existence of a solution. The question comes down to this, which is hardly new: *Can one prove the existence of a mathematical object without defining it?*

This is obviously a matter of convention. Nevertheless, I believe that we can only build solidly *by granting that it is impossible to demonstrate the existence of an object without defining it.* From this perspective, closely related to Kronecker's and Drach's, there is nothing to distinguish between A and problem C:

C. *Can every set be well-ordered?*

266 //I would have nothing more to say if the convention that I mentioned were universally adopted. But I must admit that one often uses, and that I

myself have often used, the word *existence* in other senses. For example, when Cantor's well-known argument is interpreted as saying that *there exists a non-denumerable infinity of numbers,* no means is given to name such an infinity. It is only shown, as you have said before me, that whenever one has a denumerable infinity of numbers, one can define a number not belonging to this infinity. (Here the word *define* always means *to name a property characterizing what is defined.*) This sort of existence can be used in an argument in the following fashion: A property is true if negating it leads to the assertion that all numbers can be arranged in a denumerable sequence. I believe that this kind of existence can enter an argument only in such a fashion.

Zermelo utilizes the *existence* of a *correspondence* between the subsets of M and certain of their elements. You see, even if the existence of these correspondences were not questionable, due to the way in which their existence had been proved, it would not be self-evident that one had the right to use their existence in the way that Zermelo did.

I come to the argument that you state in the following way: "It is possible to choose *ad libitum* a distinguished element m' from a particular set M; since this choice can be made for each of the sets M', it can be made for the set of these sets." From this argument the existence of the correspondences seems to follow.

First of all, when M' is given, is it self-evident that one can choose m'? It would be self-evident if M' existed in the almost Kroneckerian sense that I mentioned earlier, since to say that M' exists would then be to assert that one knew how to name certain of its elements. But let us extend the meaning of the word *exist.* The set Γ of correspondences between the subsets M' and the distinguished elements m' certainly *exists* for Hadamard and Zermelo; the latter even represents the number of its elements by a transfinite product. However, do we know how to choose an element from Γ? Obviously not, since this would give a determinate solution to problem B in the case of M.

It is true that I use the word *to choose* in the sense of *to name* and that
perhaps it suffices for Zermelo's argument that//*to choose* mean *to think of.* 267
Yet it must be noted all the same that what one is thinking of is not stated and that Zermelo's argument still requires one to think *always of the same determinate correspondence.* Hadamard believes, it seems to me, that it is not necessary to prove that one can *determine* a unique element. In my opinion, this is the source of our differences of judgment.

So as to convey more clearly the difficulty that I see, I remind you that in my thesis I proved the existence (in a sense that is not Kroneckerian and is perhaps difficult to make precise) of sets that were measurable but were not Borel-measurable. Nevertheless, I continued to doubt that any such set could ever be named. Under these conditions, would I have the right to base an argument on this hypothesis—*I assume as chosen a set that is measurable but not Borel-measurable*—even though I doubt that anyone could ever name one?

Thus I already see a difficulty with the assertion that "in a determinate M' I can choose a determinate m'," since there exist sets (the set C for example, which can be regarded as a set M' coming from a more general set) in which it is perhaps impossible to choose an element. Then there is the difficulty that you pointed out concerning the infinity of choices. As a result, if we wish to regard Zermelo's argument as completely general, it must be granted that we are speaking about an infinity of choices whose power may be very large. Moreover, no law is given either for this infinity or for the choices. We do not know if it is possible to name a rule defining a set of choices having the power of the set of the M'. We do not know if it is possible, given an M', to name an m'.

In sum, when I scrutinize Zermelo's argument, I find it, like a number of other general arguments about sets, too little Kroneckerian to have meaning (only as an existence theorem giving a solution to C, of course).

You allude to the following argument: "To well-order a set, it suffices to choose one element from it, then another, and so on." Certainly this argument presents enormous difficulties which are even greater, at least in appearance, than Zermelo's. And I am tempted to believe, as Hadamard does, that pro-
268 gress//has been made by replacing an infinity of successive choices, which depend on each other, with an unordered infinity of independent choices. Perhaps this is only an illusion. Perhaps the apparent simplification resides only in the fact that an ordered infinity of choices must be replaced by an unordered infinity, but one of higher power. Consequently, the fact that one can reduce to the single difficulty, placed at the beginning of Zermelo's argument, all the difficulties of the simplistic argument that you cited merely shows that this single difficulty is very great. In any case, it does not seem to me that the difficulty vanishes just because it concerns an unordered set of independent choices. For example, if I believe that there exist functions $y(x)$ such that, whatever x may be, y is never a value obtained from x in any algebraic equation with integer coefficients, this is because I believe, as does Hadamard, that it is possible to construct such a function. But in my opinion this does not follow immediately from the existence, whatever x may be, of numbers y which are not a value obtained from x in any equation with integer coefficients.[2]

I agree completely with Hadamard when he states that to speak of an infinity of choices without giving a rule presents a difficulty that is just as great whether or not the infinity is denumerable. When one says, as in the argument that you criticize, "since this choice can be made for each of the sets M', it can be made for the set of these sets," one says nothing unless the terms being used are explained. To make a choice can be to write down or to name the element chosen. To make an infinity of choices cannot be to write down or to name the elements chosen, one by one; life is too short. Hence, one must say what it means to make them. By this, we understand in general that a rule

[2] While correcting the proofs, I will add that in fact the argument by which we ordinarily justify Hadamard's statement A (p. 262) justifies at the same time statement B. And, in my opinion, it is because it justifies B that it justifies A.

is given which defines the elements chosen. For me as for Hadamard, this rule is equally indispensable whether or not the infinity is denumerable.

All the same, perhaps I still agree with you on this point since, although I find no theoretical difference between//the two kinds of infinity, from 269
the practical point of view I distinguish strongly between them. When I hear of a rule defining a transfinite infinity of choices, I am very suspicious because I have never seen such a rule, whereas I know of rules defining a denumerable infinity of choices. Still, this is only a question of habit. Upon reflection, I see difficulties which, in my opinion, are sometimes just as great in arguments involving only a denumerable infinity of choices as in arguments involving a transfinite number. For example, if I do not regard the classical argument as establishing the proposition that every non-denumerable set contains a subset whose power is that of Cantor's second number-class, I do not grant any greater validity to the argument showing that a set which is not finite has a denumerable subset. Although I seriously doubt that a set will ever be named which is neither finite nor infinite, it has not been proved to my satisfaction that such a set is impossible. But I have already spoken to you about these questions.

H. LEBESGUE

IV. *Letter from Hadamard to Borel*

The question appears quite clear to me now, after Lebesgue's letter. More and more plainly, it comes down to the distinction, made in Tannery's article, between what is *determined* and what can be *described*.

In this matter Lebesgue, Baire, and you have adopted Kronecker's viewpoint, which until now I believed to be peculiar to him. You answer in the negative the question posed by Lebesgue (above, p. 265): Can one prove the existence of a mathematical object without defining it? I answer it in the affirmative. I take as my own, in other words, the answer that Lebesgue himself gave regarding the set Γ (p. 266).

I grant that it is impossible for *us*, at least at present, to *name* an element in this set. That is the issue for you; it is not the issue for me.

There is only one point, it seems to me, where Lebesgue is inconsistent with himself. That is when he does or//does not allow himself to use the 270
existence of an object, according to the way in which its existence was proved. For me, the *existence* about which he speaks is a fact like any other, or else it does not occur.

As for Baire, the question takes the same form. I would prefer not to base it as he does (p. 264), following Hilbert, on the *non-contradictory*, which still seems to me to depend on psychology and to take into account the properties of our brains. I do not understand very well how Zermelo could have *proved* that *we do not perceive* a contradiction, *etc.* This cannot be *proved* but only *ascertained*: One perceived it or one did not perceive it.

Leaving this point aside, it is clear that the principal question, that of knowing if a set can be well-ordered, does not mean the same thing to Baire (any more than to you or Lebesgue) that it does to me. I would say rather—Is a well-ordering possible?—and not even—Can *one* well-order a set?—for fear of having to think who this *one* might be. Baire would say: Can *we* well-order it? An altogether subjective question, to my way of thinking.

Consequently, there are two conceptions of mathematics, two mentalities, in evidence. After all that has been said up to this point, I do not see any reason for changing mine. I do not mean to impose it. At the most, I shall note in its favor the arguments that I stated in the *Revue générale des Sciences* (30 March 1905), to wit:

1. I believe that in essence the debate is the same as the one which arose between Riemann and his predecessors over the notion of function. The *rule* that Lebesgue demands appears to me to resemble closely the analytic expression on which Riemann's adversaries insisted so strongly.[3] And even an analytic expression that is not too unusual. Not only does the *cardinality* of the choices fail to alter the question, but, it seems to me, their
271 *uniqueness* does not alter it either. I do not see//how we have the right to say, "For each value of x there exists a number satisfying .... Let y be this number ...," whereas, since "the bride is too beautiful," we cannot say, "For each value of x there exists an infinity of numbers satisfying .... Let y be one of the numbers ...."

2. Tannery's arbitrary choices lead to numbers v which *we would be* incapable of defining. I do not think that these numbers fail to exist.

As for the arguments proposed by Bernstein (*Math. Annalen*, vol. LX, p. 187) and, consequently, his objections to Zermelo's proof, I do not find them convincing. All the same, my opinion on this matter is independent of the question that we have been discussing.

Bernstein begins with Burali-Forti's paradox (*Circolo Matematico di Palermo*, 1897) concerning the set W of *all* ordinal numbers. To circumvent the contradiction obtained by Burali-Forti, he supposes the ordinal number of W to be such that it is impossible to add one to it. In my opinion this supposition, as well as the arguments that Bernstein adduces in its favor, is unacceptable. In Cantor's theory the order established between the elements of W and the additional element (it is this order which Bernstein attacks) is merely a *convention* that one is always free to make and that the properties of W, whatever they may be, cannot alter.

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[3] I believe it necessary to reiterate this point, which, if I were to express myself fully, appears to form the essence of the debate. From the invention of the infinitesimal calculus to the present, it seems to me, the essential progress in mathematics has resulted from successively annexing notions which, for the Greeks or the Renaissance geometers or the predecessors of Riemann, were "outside mathematics" because it was impossible to describe them.

The solution is different. It is the very existence of the set *W* that leads to a contradiction. In the definition of *W*, the general definition of the word *set* is incorrectly applied. We have the right to form a set only from previously existing objects, and it is easily seen that the definition of *W* supposes the contrary.

Same observation for *the set of all sets* (Hilbert, Heidelberg Congress).

Let us return to the original question. I submit in this regard, not an argument (since I believe that we shall rest eternally on our respective positions) but a consequence of your principles.

Cantor considered the set of all those functions on the interval (0, 1) that
assume only the values zero and one. To my mind this set has//a clear 272
meaning and its power is $2^{\aleph}$, as Cantor stated.[4] Likewise, the set of all functions of x makes sense to me, and I see clearly that its power is $\aleph^{\aleph}$.

What meaning does all of this have for you? It appears obvious to me that it cannot have any. For on each function you impose an additional condition which has no mathematical meaning—that of being *describable to us.*

Or rather, this is what it means: From your point of view, one should consider only those functions definable in a finite number of words. But, for this reason, the two sets formed above are *countable* and, indeed, so is every other possible set.

J. HADAMARD

V. *Letter from Borel to Hadamard*

... First of all, I would like to call your attention to an interesting remark that Lebesgue made at the meeting of the Society on 4 May: How can Zermelo be certain that in the different parts of his argument he is always speaking of *the same* choice of distinguished elements, since he characterizes them in no way *for himself.* (Here it is not a question that someone may contradict him but rather of his being intelligible to himself.)

As for your new objection, here is my response.

I prefer not to write alephs. Nevertheless, I willingly state arguments equivalent to those which you mention, without many illusions about their intrinsic value, *but intending them to suggest other more serious arguments.* To give you a practical example, I refer to Note III which I inserted at the end of my recent little book (*Leçons sur les fonctions de variables réelles*, edited by Maurice Fréchet). The argument used there was obviously suggested by Cantor's argument, which I reported in my first *Leçons sur la théorie des fonctions*, page 107.[5]

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[4] [(Translator) Here $\aleph$ is the power $2^{\aleph_0}$ of the continuum, which is assumed to be an aleph.]

[5] In Notes I and II of this little book, I continually used arguments of the sort that you deny me the right to make. So I was constantly filled with scruples, and each of these two Notes ended with a very restrictive remark.

273 //The form that I adopted in Note III is not absolutely satisfactory, as I indicated at the bottom of the last page of my book. But the analogous argument which Lebesgue gives in his article in the *Journal de Jordan* (1905) is, I believe, completely irreproachable, in the sense that it leads to a precise result expressible in a finite number of words. Nevertheless, it originated from that of Cantor.

One may wonder what is the real value of these arguments that I do not regard as absolutely valid but that still lead ultimately to effective results. In fact, it seems that if they were completely devoid of value, they could not lead to anything, since they would be meaningless collections of words. This, I believe, would be too harsh. They have a value analogous to certain theories in mathematical physics, through which we do not claim to express reality but rather to have a guide that aids us, by analogy, in predicting new phenomena, which must then be verified. It would require considerable research to learn what is the real and precise sense that can be attributed to arguments of this sort. Such research would be useless, or at least it would require more effort than it would be worth. How these overly abstract arguments are related to the concrete becomes clear when the need is felt.

I would agree with you that it is self-contradictory to speak of the set of all sets, for, by the argument from page 107 cited above, we can form a set whose power is still greater. But I believe that this contradiction arises because sets that are not really defined have been introduced.

E. Borel