Was Cantor Surprised?

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Abstract. We look at the circumstances and context of Cantor's famous remark, "I see it, but I don't believe it." We argue that, rather than denoting astonishment at his result, the remark pointed to Cantor's worry about the correctness of his proof.

Mathematicians love to tell each other stories. We tell them to our students too, and they eventually pass them on. One of our favorites, and one that I heard as an undergraduate, is the story that Cantor was so surprised when he discovered one of his theorems that he said "I see it, but I don't believe it!" The suggestion was that sometimes we might have a proof, and therefore *know* that something is true, but nevertheless still find it hard to believe.

That sentence can be found in Cantor's extended correspondence with Dedekind about ideas that he was just beginning to explore. This article argues that what Cantor meant to convey was not really surprise, or at least not the kind of surprise that is usually suggested. Rather, he was expressing a very different, if equally familiar, emotion. In order to make this clear, we will look at Cantor's sentence in the context of the correspondence as a whole.

Exercises in myth-busting are often unsuccessful. As Joel Brouwer says in his poem "A Library in Alexandria,"

> . . . And so history gets written
> to prove the legend is ridiculous. But soon the legend
> replaces the history because the legend is more interesting.

Our only hope, then, lies in arguing not only that the standard story is false, but also that the real story is more interesting.

1. THE SURPRISE. The result that supposedly surprised Cantor was the fact that sets of different dimension could have the same cardinality. Specifically, Cantor showed (of course, not yet using this language) that there was a bijection between the interval $I = [0, 1]$ and the n-fold product $I^n = I \times I \times \cdots \times I$.

There is no doubt, of course, that this result is "surprising," i.e., that it is counterintuitive. In fact Cantor said so explicitly, pointing out that he had expected something different. But the story has grown in the telling, and in particular Cantor's phrase about seeing but not believing has been read as expressing what we usually mean when we see something happen and exclaim "Unbelievable!" What we mean is not that we actually do not believe, but that we find what we know has happened to be hard to believe because it is so unusual, unexpected, surprising. In other words, the idea is that Cantor felt that the result was hard to believe even though he had a proof. His phrase has been read as suggesting that mathematical proof may engender rational certainty while still not creating intuitive certainty.

The story was then co-opted to demonstrate that mathematicians often discover things that they did not expect or prove things that they did not actually want to prove. For example, here is William Byers in *How Mathematicians Think*:

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> Cantor himself initially believed that a higher-dimensional figure would have a larger cardinality than a lower-dimensional one. Even after he had found the argument that demonstrated that cardinality did not respect dimensions: that one-, two-, three-, even n-dimensional sets all had the same cardinality, he said, "I see it, but I don't believe it." [**2**, p. 179]

Did Cantor's comment suggest that he found it hard to believe his own theorem even after he had proved it? Byers was by no means the first to say so.

Many mathematicians thinking about the experience of doing mathematics have found Cantor's phrase useful. In his preface to the original (1937) publication of the Cantor-Dedekind correspondence, J. Cavaillès already called attention to the phrase:

> ...these astonishing discoveries—astonishing first of all to the author himself: "I see it but I don't believe it at all,"[1] he writes in 1877 to Dedekind about one of them—, these radically new notions... [**14**, p. 3, my translation]

Notice, however, that Cavaillès is still focused on the description of the result as "surprising" rather than on the issue of Cantor's psychology. It was probably Jacques Hadamard who first connected the phrase to the question of how mathematicians think, and so in particular to what Cantor was thinking. In his famous *Essay on the Psychology of Invention in the Mathematical Field*, first published in 1945 (only eight years after [**14**]), Hadamard is arguing about Newton's ideas:

> ...if, strictly speaking, there could remain a doubt as to Newton's example, others are completely beyond doubt. For instance, it is certain that Georg Cantor could not have foreseen a result of which he himself says "I see it, but I do not believe it." [**10**, pp. 61–62].

Alas, when it comes to history, few things are "certain."

2. THE MAIN CHARACTERS.

Our story plays out in the correspondence between Richard Dedekind and Georg Cantor during the 1870s. It will be important to know something about each of them.

Richard Dedekind was born in Brunswick on October 6, 1831, and died in the same town, now part of Germany, on February 12, 1916. He studied at the University of Göttingen, where he was a contemporary and friend of Bernhard Riemann and where he heard Gauss lecture shortly before the old man's death. After Gauss died, Lejeune Dirichlet came to Göttingen and became Dedekind's friend and mentor.

Dedekind was a very creative mathematician, but he was not particularly ambitious. He taught in Göttingen and in Zurich for a while, but in 1862 he returned to his home town. There he taught at the local Polytechnikum, a provincial technical university. He lived with his brother and sister and seemed uninterested in offers to move to more prestigious institutions. See [**1**] for more on Dedekind's life and work.

Our story will begin in 1872. The first version of Dedekind's ideal theory had appeared as Supplement X to Dirichlet's *Lectures in Number Theory* (based on actual lectures by Dirichlet but entirely written by Dedekind). Also just published was one of his best known works, "Stetigkeit und Irrationalzahlen" ("Continuity and Irrational Numbers"; see [**7**]; an English translation is included in [**5**]). This was his account of how to construct the real numbers as "cuts." He had worked out the idea in 1858, but published it only 14 years later.

[1] Cavaillès misquotes Cantor's phrase as "je le vois mais je ne le crois point."

Georg Cantor was born in St. Petersburg, Russia, on March 3, 1845. He died in Halle, Germany, on January 6, 1918. He studied at the University of Berlin, where the mathematics department, led by Karl Weierstrass, Ernst Eduard Kummer, and Leopold Kronecker, might well have been the best in the world. His doctoral thesis was on the number theory of quadratic forms.

In 1869, Cantor moved to the University of Halle and shifted his interests to the study of the convergence of trigonometric series. Very much under Weierstrass's influence, he too introduced a way to construct the real numbers, using what he called "fundamental series." (We call them "Cauchy sequences.") His paper on this construction also appeared in 1872.

Cantor's lifelong dream seems to have been to return to Berlin as a professor, but it never happened. He rose through the ranks in Halle, becoming a full professor in 1879 and staying there until his death. See [**13**] for a short account of Cantor's life. The standard account of Cantor's mathematical work is [**4**].

Cantor is best known, of course, for the creation of set theory, and in particular for his theory of transfinite cardinals and ordinals. When our story begins, this was mostly still in the future. In fact, the birth of several of these ideas can be observed in the correspondence with Dedekind. This correspondence was first published in [**14**]; we quote it from the English translation by William Ewald in [**8**, pp. 843–878].

3. "ALLOW ME TO PUT A QUESTION TO YOU." Dedekind and Cantor met in Switzerland when they were both on vacation there. Cantor had sent Dedekind a copy of the paper containing his construction of the real numbers. Dedekind responded, of course, by sending Cantor a copy of his booklet. And so begins the story.

Cantor was 27 years old and very much a beginner, while Dedekind was 41 and at the height of his powers; this accounts for the tone of deference in Cantor's side of the correspondence. Cantor's first letter acknowledged receipt of [**7**] and says that "my conception [of the real numbers] agrees entirely with yours," the only difference being in the actual construction. But on November 29, 1873, Cantor moves on to new ideas:

> Allow me to put a question to you. It has a certain theoretical interest for me, but I cannot answer it myself; perhaps you can, and would be so good as to write me about it. It is as follows.
>
> Take the totality of all positive whole-numbered individuals n and denote it by (n). And imagine, say, the totality of all positive real numerical quantities x and designate it by (x). The question is simply, Can (n) be correlated to (x) in such a way that to each individual of the one totality there corresponds one and only one of the other? At first glance one says to oneself no, it is not possible, for (n) consists of discrete parts while (x) forms a continuum. But nothing is gained by this objection, and although I incline to the view that (n) and (x) permit no one-to-one correlation, I cannot find the explanation which I seek; perhaps it is very easy.

In the next few lines, Cantor points out that the question is not as dumb as it looks, since "the totality $\left(\frac{p}{q}\right)$ of all positive rational numbers" *can* be put in one-to-one correspondence with the integers.

We do not have Dedekind's side of the correspondence, but his notes indicate that he responded indicating that (1) he could not answer the question either, (2) he could show that the set of all *algebraic* numbers is countable, and (3) that he didn't think the question was all that interesting. Cantor responded on December 2:

> I was exceptionally pleased to receive your answer to my last letter. I put my question to you because I had wondered about it already several years ago, and was never certain whether the difficulty I found was subjective or whether it was inherent in the subject. Since you write that you too are unable to answer it, I may assume the latter.—In addition, I should like to add that I have never seriously occupied myself with it, because it has no special practical interest for me. And I entirely agree with you when you say that for this reason it does not deserve much effort. But it would be good if it could be answered; e.g., if it could be answered with no, then one would have a new proof of Liouville's theorem that there are transcendental numbers.

Cantor first concedes that perhaps it is not that interesting, then immediately points out an application that was sure to interest Dedekind! In fact, Dedekind's notes indicate that it worked: "But the opinion I expressed that the first question did not deserve too much effort was conclusively refuted by Cantor's proof of the existence of transcendental numbers." [**8**, p. 848]

These two letters are fairly typical of the epistolary relationship between the two men: Cantor is deferential but is continually coming up with new ideas, new questions, new proofs; Dedekind's role is to judge the value of the ideas and the correctness of the proofs. The very next letter, from December 7, 1873, contains Cantor's first proof of the uncountability of the real numbers. (It was not the "diagonal" argument; see [**4**] or [**9**] for the details.)

4. "THE SAME TRAIN OF THOUGHT . . . " Cantor seemed to have a good sense for what question should come next. On January 5, 1874, he posed the problem of higher-dimensional sets:

> As for the question with which I have recently occupied myself, it occurs to me that the same train of thought also leads to the following question:
>
> Can a surface (say a square including its boundary) be one-to-one correlated to a line (say a straight line including its endpoints) so that to every point of the surface there corresponds a point of the line, and conversely to every point of the line there corresponds a point of the surface?
>
> It still seems to me at the moment that the answer to this question is very difficult—although here too one is so impelled to say *no* that one would like to hold the proof to be almost superfluous.

Cantor's letters indicate that he had been asking others about this as well, and that most considered the question just plain weird, because it was "obvious" that sets of different dimensions could not be correlated in this way. Dedekind, however, seems to have ignored this question, and the correspondence went on to other issues. On May 18, 1874, Cantor reminded Dedekind of the question, and seems to have received no answer.

The next letter in the correspondence is from May, 1877. The correspondence seems to have been reignited by a misunderstanding of what Dedekind meant by "the essence of continuity" in [**7**]. On June 20, 1877, however, Cantor returns to the question of bijections between sets of different dimensions, and now proposes an answer:

> . . . I should like to know whether you consider an inference-procedure that I use to be arithmetically rigorous.
>
> The problem is to show that surfaces, bodies, indeed even continuous structures of ρ dimensions can be correlated one-to-one with continuous lines, i.e.,

> with structures of only *one* dimension—so that surfaces, bodies, indeed even continuous structures of ρ dimension have the same *power* as curves. This idea seems to conflict with the one that is especially prevalent among the representatives of modern geometry, who speak of simply infinite, doubly, triply, ..., ρ-fold infinite structures. (Sometimes you even find the idea that the infinity of points of a surface or a body is obtained as it were by squaring or cubing the infinity of points of a line.)

Significantly, Cantor's formulation of the question had changed. Rather than asking *whether* there is a bijection, he posed the question of *finding* a bijection. This is, of course, because he believed he had found one. By this point, then, Cantor knows the right answer. It remains to give a proof that will convince others. He goes on to explain his idea for that proof, working with the ρ-fold product of the unit interval with itself, but for our purposes we can consider only the case $\rho = 2$.

The proof Cantor proposed is essentially this: take a point (x, y) in $[0, 1] \times [0, 1]$, and write out the decimal expansions of x and y:

$$(x, y) = (0.abcde\ldots, 0.\alpha\beta\gamma\delta\epsilon\ldots).$$

Some real numbers have more than one decimal expansion. In that case, we always choose the expansion that ends in an infinite string of 9s. Cantor's idea is to map (x, y) to the point $z \in [0, 1]$ given by

$$z = 0.a\alpha b\beta c\gamma d\delta e\epsilon\ldots$$

Since we can clearly recover x and y from the decimal expansion of z, this gives the desired correspondence.

Dedekind immediately noticed that there was a problem. On June 22, 1877 (one cannot fail to be impressed with the speed of the German postal service!), he wrote back pointing out a slight problem "which you will perhaps solve without difficulty." He had noticed that the function Cantor had defined, while clearly one-to-one, was not onto. (Of course, he did not use those words.) Specifically, he pointed out that such numbers as

$$z = 0.120101010101\ldots$$

did not correspond to any pair (x, y), because the only possible value for x is $0.100000\ldots$, which is disallowed by Cantor's choice of decimal expansion. He was not sure if this was a big problem, adding "I do not know if my objection goes to the essence of your idea, but I did not want to hold it back."

Of course, the problem Dedekind noticed is real. In fact, there are a great many real numbers not in the image, since we can replace the ones that separate the zeros with *any* sequence of digits. The image of Cantor's map is considerably smaller than the whole interval.

Cantor's first response was a postcard sent the following day. (Can one envision him reading the letter at the post office and immediately dispatching a postcard back?) He acknowledged the error and suggested a solution:

> Alas, you are entirely correct in your objection; but happily it concerns only the proof, not the content. For I proved *somewhat more* than I had realized, in that I bring a system $x_1, x_2, \ldots, x_\rho$ of unrestricted real variables (that are ≥ 0 and ≤ 1) into one-to-one relationship with a variable y that does not assume all values of

that interval, but rather all with the exception of certain y''. However, it assumes each of the corresponding values y' only *once*, and that seems to me to be the essential point. For now I can bring y' into a one-to-one relation with another quantity t that assumes all the values ≥ 0 and ≤ 1.

I am delighted that you have found no other objections. I shall shortly write to you at greater length about this matter.

This is a remarkable response. It suggests that Cantor was very confident that his result was true. This confidence was due to the fact that Cantor was already thinking in terms of what later became known as "cardinality." Specifically, he expects that the existence of a one-to-one mapping from one set A to another set B implies that the size of A is in some sense "less than or equal to" that of B.

Cantor's proof shows that the points of the square can be put into bijection with a subset of the interval. Since the interval can clearly be put into bijection with a subset of the square, this strongly suggests that both sets of points "are the same size," or, as Cantor would have said it, "have the same power." All we need is a proof that the "powers" are linearly ordered in a way that is compatible with inclusions.

That the cardinals are indeed ordered in this way is known today as the Schroeder-Bernstein theorem. The postcard shows that Cantor already "knew" that the Schroeder-Bernstein theorem should be true. In fact, he seems to implicitly promise a proof of that very theorem. He was not able to find such a proof, however, then or (as far as I know) ever.

His fuller response, sent two days later on June 25, contained instead a completely different, and much more complicated, proof of the original theorem.

I sent you a postcard the day before yesterday, in which I acknowledged the gap you discovered in my proof, and at the same time remarked that I am able to fill it. But I cannot repress a certain regret that the subject demands more complicated treatment. However, this probably lies in the nature of the subject, and I must console myself; perhaps it will later turn out that the missing portion of that proof can be settled more simply than is at present in my power. But since I am at the moment concerned above all to persuade you of the correctness of my theorem . . . I allow myself to present another proof of it, which I found even earlier than the other.

Notice that what Cantor is trying to do here is to convince Dedekind that his theorem is true by presenting him a correct proof.[2] There is no indication that Cantor had any doubts about the correctness of the result itself. In fact, as we will see, he says so himself.

Let's give a brief account of Cantor's proof; to avoid circumlocutions, we will express most of it in modern terms. Cantor began by noting that every real number x between 0 and 1 can be expressed as a continued fraction

$$x = \cfrac{1}{a + \cfrac{1}{b + \cfrac{1}{c + \cdots}}}$$

[2]Cantor claimed he had found this proof before the other. I find this hard to believe. In fact, the proof looks very much like the result of trying to fix the problem in the first proof by replacing (nonunique) decimal expansions with (unique) continued fraction expansions.

where the partial quotients $a, b, c, \ldots$, etc. are all positive integers. This representation is infinite if and only if x is irrational, and in that case the representation is *unique*.

So one can argue just as before, "interleaving" the two continued fractions for x and y, to establish a bijection between the set of pairs (x, y) such that both x and y are irrational and the set of irrational points in [0, 1]. The result is a bijection because the inverse mapping, splitting out two continued fraction expansions from a given one, will certainly produce two *infinite* expansions.

That being done, it remains to be shown that the set of irrational numbers between 0 and 1 can be put into bijection with the interval [0, 1]. This is the hard part of the proof. Cantor proceeded as follows.

First he chose an enumeration of the rationals $\{r_k\}$ and an increasing sequence of irrationals $\{\eta_k\}$ in [0, 1] converging to 1. He then looked at the bijection from [0, 1] to [0, 1] that is the identity on [0, 1] except for mapping $r_k \mapsto \eta_k$, $\eta_k \mapsto r_k$. This gives a bijection between irrationals in [0, 1] and [0, 1] minus the sequence $\{\eta_k\}$ and reduces the problem to proving that [0, 1] can be put into bijection with $[0, 1] - \{\eta_k\}$.

At this point Cantor claims that it is now enough to "successively apply" the following theorem:

> A number y that can assume all the values of the interval $(0 \ldots 1)$ with the solitary exception of the value 0 can be correlated one-to-one with a number x that takes on all values of the interval $(0 \ldots 1)$ without exception.

In other words, he claimed that there was a bijection between the half-open interval (0, 1] and the closed interval [0, 1], and that "successive application" of this fact would finish the proof. In the actual application he would need the intervals to be *open* on the right, so, as we will see, he chose a bijection that mapped 1 to itself.

Cantor did not say exactly what kind of "successive application" he had in mind, but what he says in a later letter suggests it was this: we have the interval [0, 1] minus the sequence of the η_k. We want to "put back in" the η_k, one at a time. So we leave the interval $[0, \eta_1)$ alone, and look at (η_1, η_2). Applying the lemma, we construct a bijection between that and $[\eta_1, \eta_2)$. Then we do the same for (η_2, η_3) and so on. Putting together these bijections produces the bijection we want.

Finally, it remained to prove the lemma, that is, to construct the bijection from [0, 1] to (0, 1]. Modern mathematicians would probably do this by choosing a sequence x_n in (0, 1), mapping 0 to x_1 and then every x_n to x_{n+1}. This "Hilbert hotel" idea was still some time in the future, however, even for Cantor. Instead, Cantor chose a bijection that could be represented visually, and simply drew its graph. He asked Dedekind to consider "the following peculiar curve," which we have redrawn in Figure 1 based on the photograph reproduced in [**4**, p. 63].

Such a picture requires some explanation, and Cantor provided it. The domain has been divided by a geometric progression, so $b = 1/2$, $b_1 = 3/4$, and so on; $a = (0, 1/2)$, $a' = (1/2, 3/4)$, etc. The point C is (1, 1). The points $d' = (1/2, 1/2)$, $d'' = (3/4, 3/4)$, etc. give the corresponding subdivision of the main diagonal.

> The curve consists of infinitely many parallel line segments $\overline{ab}$, $\overline{a'b'}$, $\overline{a''b''}$ and of the point c. *The endpoints* $b, b', b'', \ldots$ *are not regarded as belonging to the curve.*

The stipulation that the segments are open at their lower endpoints means that 0 is not in the image. This proves the lemma, and therefore the proof is finished.

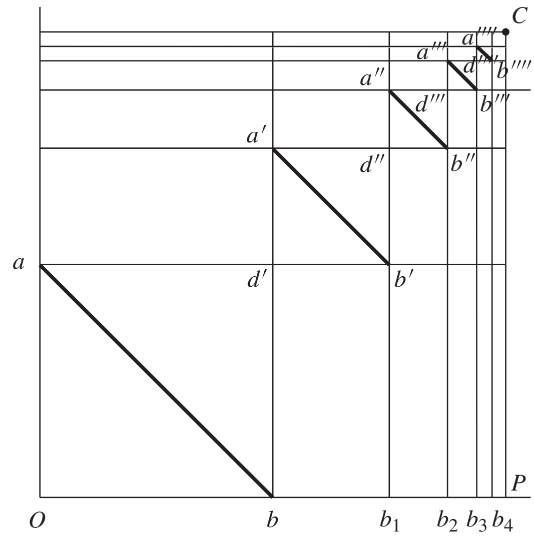

Figure 1. Cantor's function from [0, 1] to (0, 1].

Cantor did not even add that last comment. As soon as he had explained his curve, he moved on to make extensive comments on the theorem and its implications. He turns on its head the objection that various mathematicians made to his question, namely that it was "obvious" from geometric considerations that the number of variables is invariant:

> For several years I have followed with interest the efforts that have been made, building on Gauss, Riemann, Helmholtz, and others, towards the clarification of all questions concerning the ultimate foundations of geometry. It struck me that all the important investigations in this field proceed from an unproven presupposition which does not appear to me self-evident, but rather to need a justification. I mean the presupposition that a ρ-fold extended continuous manifold needs ρ independent real coordinates for the determination of its elements, and that for a given manifold this number of coordinates can neither be increased nor decreased.
>
> This presupposition became my view as well, and I was almost convinced of its correctness. The only difference between my standpoint and all the others was that I regarded that presupposition as a theorem which stood in great need of a proof; and I refined my standpoint into a question that I presented to several colleagues, in particular at the Gauss Jubilee in Göttingen. The question was the following:
>
> "Can a continuous structure of ρ dimensions, where $\rho > 1$, be related one-to-one with a continuous structure of one dimension so that to each point of the former there corresponds one and only one point of the latter?"
>
> Most of those to whom I presented this question were extremely puzzled that I should ask it, for it is quite *self-evident* that the determination of a point in an extension of ρ dimensions always needs ρ independent coordinates. But whoever penetrated the sense of the question had to acknowledge that a proof was needed to show why the question should be answered with the "self-evident" *no*. As I say, I myself was one of those who held it for the *most likely* that the

question should be answered with a *no*—until quite recently I arrived by rather intricate trains of thought at the conviction that the answer to that question is an unqualified *yes*.[3] Soon thereafter I found the proof which you see before you today.

So one sees what wonderful power lies in the ordinary real and irrational numbers, that one is able to use them to determine uniquely the elements of a ρ-fold extended continuous manifold *with a single coordinate*. I will only add at once that their power goes yet further, in that, as will not escape you, my proof can be extended without any great increase in difficulty to manifolds with an infinitely great dimension-number, provided that their infinitely-many dimensions have the form of a simple infinite sequence.

Now it seems to me that all philosophical or mathematical deductions that use that erroneous presupposition are inadmissible. Rather the difference that obtains between structures of *different* dimension-number must be sought in quite other terms than in the number of independent coordinates—the number that was hitherto held to be characteristic.

5. "JE LE VOIS..." So now Dedekind had a lot to digest. The interleaving argument is not problematic in this case, and the existence of a bijection between the rationals and the increasing sequence η_k had been established in 1872. But there were at least two sticky points in Cantor's letter.

First, there is the matter of what kind of "successive application" of the lemma Cantor had in mind. Whatever it was, it would seem to involve constructing a bijection by "putting together" an infinite number of functions. One can easily get in trouble.

For example, here is an alternative reading of what Cantor had in mind. Instead of applying the lemma to the interval (η_1, η_2), we could apply it to $(0, \eta_1)$ to put it into bijection with $(0, \eta_1]$. So now we have "put η_1 back in" and we have a bijection between $[0, 1] - \{\eta_1, \eta_2, \eta_3, \dots\}$ and $[0, 1] - \{\eta_2, \eta_3, \dots\}$.

Now repeat: use the lemma on $(0, \eta_2)$ to make a bijection to $(0, \eta_2]$. So we have "put η_2 back in." If we keep doing that, we presumably get a bijection from $(0, 1)$ minus the η_k to all of $(0, 1)$.

But do we? What is the image of, say, $\frac{1}{3}\eta_1$? It is not fixed under any of our functions. To determine its image in $[0, 1]$, we would need to compose infinitely many functions, and it's not clear how to do that. If we manage to do it with some kind of limiting process, then it is no longer clear that the overall function is a bijection.

The interpretation Cantor probably intended (and later stated explicitly) yields a workable argument because the domains of the functions are disjoint, so it is clear where to map any given point. But since Cantor did not indicate his argument in this letter, one can imagine Dedekind hesitating. In any case, at this point in history the idea of constructing a function out of infinitely many pieces would have been both new and worrying.

The second sticky point was Cantor's "application" of his theorem to undermine the foundations of geometry. This is, of course, the sort of thing one has to be careful about. And it is clear, from Dedekind's eventual response to Cantor, that it concerned him.

Dedekind took longer than usual to respond. Having already given one wrong proof, Cantor was anxious to hear a "yes" from Dedekind, and so he wrote again on June 29:

[3]The original reads "...bis ich vor ganz kurzer Zeit durch ziemlich verwickelte Gedankereihen zu der Ueberzeugung gelangte, dass jene Frage ohne all Einschränkgung zu *bejahen* ist." Note Cantor's *Überzeugung*—conviction, belief, certainty.

> Please excuse my zeal for the subject if I make so many demands upon your kindness and patience; the communications which I lately sent you are even for me so unexpected, so new, that I can have no peace of mind until I obtain from you, honoured friend, a decision about their correctness. So long as you have not agreed with me, I can only say: *je le vois, mais je ne le crois pas.* And so I ask you to send me a postcard and let me know when you expect to have examined the matter, and whether I can count on an answer to my quite demanding request.

So here is the phrase. The letter is, of course, in German, but the famous "I see it, but I don't believe it" is in French.[4] Seen in its context, the issue is clearly not that Cantor was finding it hard to believe his *result*. He was confident enough about that to think he had rocked the foundations of the geometry of manifolds. Rather, he felt a need for confirmation that his *proof* was correct. It was his *argument* that he saw but had trouble believing. This is confirmed by the rest of the letter, in which Cantor spelled out in detail the most troublesome step, namely, how to "successively apply" his lemma to construct the final bijection.

So the famous phrase does not really provide an example of a mathematician having trouble believing a theorem even though he had proved it. Cantor, in fact, seems to have been confident [*überzeugt*!] that his theorem was true, as he himself says. He had in hand at least two arguments for it: the first argument, using the decimal expansion, required supplementation by a proof of the Schroeder-Bernstein theorem, but Cantor was quite sure that this would eventually be proved. The second argument was correct, he thought, but its complicated structure might have allowed something to slip by him.

He knew that his theorem was a radically new and surprising result—it would certainly surprise others!—and thus it was necessary that the proof be as solid as possible. The earlier error had given Cantor reason to worry about the correctness of his argument, leaving Cantor in need of his friend's confirmation before he would trust the proof.

Cantor was, in fact, in a position much like that of a student who has proposed an argument, but who knows that a proof is an argument that convinces his teacher. Though no longer a student, he knows that a proof is an argument that will convince others, and that in Dedekind he had the perfect person to find an error if one were there. So he saw, but until his friend's confirmation he did not believe.

6. WHAT CAME NEXT. So why did Dedekind take so long to reply? From the evidence of his next letter, dated July 2, it was not because he had difficulty with the proof. His concern, rather, was Cantor's challenge to the foundations of geometry.

The letter opens with a sentence clearly intended to allay Cantor's fears: "I have examined your proof once more, and I have discovered no gap in it; I am quite certain that your interesting theorem is correct, and I congratulate you on it." But Dedekind did not accept the consequences Cantor seemed to find:

> However, as I already indicated in the postcard, I should like to make a remark that counts *against* the conclusions concerning the concept of a manifold of ρ dimensions that you append in your letter of 25 June to the communication and the proof of the theorem. Your words make it appear—my interpretation may be incorrect—as though on the basis of your theorem you wish to cast doubt on the meaning or the importance of this concept...

[4] I don't know whether this is because of the rhyme vois/crois, or because of the well-known phrase "voir, c'est croire," or for some other reason. I do not believe the phrase was already proverbial.

> Against this, I declare (despite your theorem, or rather in consequence of reflections that it stimulated) my conviction or my faith (I have not yet had time even to make an attempt at a proof) that the dimension-number of a continuous manifold remains its first and most important invariant, and I must defend all previous writers on the subject... For all authors have clearly made the tacit, completely natural presupposition that in a new determination of the points of a continuous manifold by new coordinates, these coordinates should also (in general) be *continuous* functions of the old coordinates...

Dedekind pointed out that, in order to establish his correspondence, Cantor had been "compelled to admit a frightful, dizzying discontinuity in the correspondence, which dissolves everything to atoms, so that every continuously connected part of one domain appears in its image as thoroughly decomposed and discontinuous." He then set out a new conjecture that spawned a whole research program:

> ...for the time being I believe the following theorem: "If it is possible to establish a reciprocal, one-to-one, and complete correspondence between the points of a continuous manifold A of a dimensions and the points of a continuous manifold B of b dimensions, then this *correspondence itself*, if a and b are unequal, is necessarily *utterly discontinuous*."

In his next letter, Cantor claimed that this was indeed his point: where Riemann and others had casually spoken of a space that requires n coordinates as if that number was known to be invariant, he felt that this invariance required proof. "Far from wishing to turn my result against the article of faith of the theory of manifolds, I rather wish to use it to secure its theorems," he wrote. The required theorem turned out to be true, indeed, but proving it took much longer than either Cantor or Dedekind could have guessed: it was finally proved by Brouwer in 1910. The long and convoluted story of that proof can be found in [**3**], [**11**], and [**12**].

Finally, one should point out that it was only some three months later that Cantor found what most modern mathematicians consider the "obvious" way to prove that there is a bijection between the interval minus a countable set and the whole interval. In a letter dated October 23, 1877, he took an enumeration ϕ_ν of the rationals and let $\eta_\nu = \sqrt{2}/2^\nu$. Then he constructed a map from [0, 1] sending η_ν to $\eta_{2\nu-1}$, ϕ_ν to $\eta_{2\nu}$, and every other point h to itself, thus getting a bijection between [0, 1] and the irrational numbers between 0 and 1.

7. MATHEMATICS AS CONVERSATION. Is the real story more interesting than the story of Cantor's surprise? Perhaps it is, since it highlights the social dynamic that underlies mathematical work. It does not render the theorem any less surprising, but shifts the focus from the result itself to its proof.

The record of the extended mathematical conversation between Cantor and Dedekind reminds us of the importance of such interaction in the development of mathematics. A mathematical proof is, after all, a kind of challenge thrown at an idealized opponent, a skeptical adversary that is reluctant to be convinced. Often, this adversary is actually a colleague or collaborator, the first reader and first critic.

A proof is not a proof until some reader, preferably a competent one, says it is. Until then we may see, but we should not believe.

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