On Peirce's Discovery of Cantor's Theorem[1]

Sobre a Descoberta de Peirce do Teorema de Cantor

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Abstract: In 1897 C.S. Peirce published a proof of what is now known as Cantor's Theorem. Though Peirce always claimed credit for discovery of this result, Cantor had published a proof of it in 1891, in a paper of which Peirce is known to have had a copy. I argue that Peirce's discovery took place in 1896 and was indeed independent, though it was stimulated by the definition of cardinal exponentiation that Cantor first published in 1895.

Key-words: Cantor. Collection. Multitude. Peirce. Power set.

Resumo: Em 1897, C.S. Peirce publicou uma prova do que é hoje conhecido como Teorema de Cantor. Embora Peirce tenha sempre reivindicado crédito pela descoberta desse resultado, Cantor tinha publicado uma prova do mesmo em 1891, em um trabalho do qual se sabe que Peirce tinha uma cópia. Argumento que a descoberta de Peirce ocorreu em 1896 e foi, de fato, independente, embora tenha sido estimulada pela definição de exponenciação cardeal que Cantor primeiro publicara em 1895.

Palavras-chave: Cantor. Coleção. Conjunto de poder. Multitude. Peirce.

C.S. Peirce devoted a good deal of energy, in the final decades of his life, to the theory of collections, that is, to approximately what we now call set theory. Set theory as we know it derives chiefly from the pioneering work of Georg Cantor, which had a profound influence on Peirce. At the same time Peirce's collection theory diverges, in important respects, from Cantor's, and thus diverges from whatever set theoretic orthodoxy can reasonably be said to exist in our own day. Of course Peirce's value often lies precisely in his departures from our orthodoxies, and Randall Dipert has urged that this is the case with Peirce's theory of collections (DIPERT 1997: 55-58). I entirely agree; but the task

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of understanding, let alone assessing and appropriating, Peirce's contributions in this area is largely unbegun.[2] An indispensable first step is to map out what Peirce took over, and what he rejected, from Cantor. Properly taken, this would be a giant step; for much of what Peirce says about collections — both mathematically and philosophically — is a conscious reaction to Cantor, or rather, to his (not very thorough, and not always accurate) reading of Cantor.

In this essay I want to sort out one pivotal, and confusing, episode in the history of Peirce's reading of Cantor: his discovery of a diagonal argument for a version of what has come — with good reason — to be known as Cantor's Theorem. My focus will be primarily historical, and more directly on Peirce's mathematics than on his philosophy. This is only a matter of degree: as always with Peirce, his philosophical preoccupations are never very far from the surface. But though my ultimate interests in this material are philosophical, I will concentrate here on the preliminary task — whose troubles will prove to be more than sufficient unto the day — of setting the historical record straight.

1. Terminology

Cantor's Theorem is a basic fact about the cardinalities of sets; Peirce's version, which I call his Step Lemma, is a basic fact about the multitudes of collections. I will not dwell on the similarities and differences between the Peircean concepts of collection and multitude, on the one hand, and the corresponding Cantorian concepts of set and cardinality on the other. The differences will not much matter for our purposes; in any case it would be putting the cart before the horse to pursue the comparative questions very far without first having figured out at least the broad outlines of the history.

Still, it should count for something that Peirce deliberately chose to think and write about these issues in a somewhat different language than the one we have inherited (in translation) from Cantor; and without being completely fastidious about the matter, I will as a rule use Cantorian terms when discussing Cantor, and Peircean terms when discussing Peirce. Little purpose would be served by duplication at the formal level: so I will use $|C|$ for both the power of the set C and the multitude of the collection C,[3] and

[2] The chief exceptions are Dipert's paper, and Dauben's historical essays (DAUBEN 1977, 1981, 1982, 1995). The more extensive literature on Peirce's continuum often touches on collections: see especially Herron's paper on infinitesimals (HERRON 1997) and Putnam's introduction to (PEIRCE 1992b). Myrvold's study (MYRVOLD 1995) is a particularly valuable resource, to which I am much indebted. The mathematical chapters in Murphey's book (MURPHEY 1961), and Eisele's essays collected in (EISELE 1979b), stand out among less recent works.

[3] Cantor's own notation for power — $\overline{\overline{M}}$ rather than $|M|$ — is expressive of his philosophical conception of power as a property arrived at by a double abstraction, from both the nature and the order of the elements. In adopting a more standard modern notation I am ignoring this important aspect of Cantor's thinking. It would need to be taken into account in comparing Cantor's powers with Peirce's multitudes; but it does not really affect the points at issue in this paper. The philosophical difficulties inherent in Cantor's approach have received a thorough going-over from Michael Hallett (HALLETT 1984: 128-141).

P(C) for both power sets and power collections. The formal notations can serve as a neutral vocabulary where one is needed.

2. Cantor's Theorem

In 1895 Cantor published, in Volume 46 of *Mathematische Annalen*, the first part of a compendious exposition of his work on set theory entitled "*Beiträge zur Begründung der transfiniten Mengenlehre*" (*B1*)[4] (CANTOR 1895a). In §1 of that work Cantor defines sameness of power in terms of one-one correspondence (*B1*, 86-87): two sets *M* and *N* have the same power (cardinal number) just in case there is a bijective map between them. In §2 he explains what it means for the cardinal of M to be *less than* that of N (formally, $|M| < |N|$): this is so when there is a proper subset *N'* of *N* which has the same power as *M*, but no proper subset *M'* of *M* which has the same power as *N* (*B1*, 89).

This was hardly new ground for Cantor at the time he wrote *B1*. Two decades earlier, in one of his most famous publications (CANTOR 1874), he had proved that the power of the set **N** of natural numbers was less than that of the set **R** of real numbers. More recently, in 1891, he had published a short paper entitled "Über eine elementare Frage der Mannigfaltigkeitslehre" (*EFM*) (CANTOR 1891),[5] in which he showed more generally that for any cardinal μ there is a larger one. There are two main arguments in that paper, and we will need to have a fairly detailed idea of how both of them work.

In the first argument Cantor considers a manifold *M* each of whose elements is of the form

$$E = (x_1, x_2, \ldots, x_\gamma, \ldots)$$

where γ ranges over the set of positive integers. He does not tell us anything about the domain from which the x_γ are taken, but he does tell us that each of them has one of the mutually exclusive characters *m* and *w*. (We will soon see that the domain is of very little importance: it is *m* and *w* that matter, and the countable infinity of indices for the sequences in *M*.) He proves that "such a manifold *M* does not have the power of the sequence 1,2,...,ν,..." (*EFM*, 921), by proving that

> If $E_1, E_2, \ldots, E_\nu, \ldots$ is any simply infinite sequence of elements of the manifold *M*, then there is always an element E_0 of *M* which corresponds to no *E*. (*EFM*, 921)

Cantor considers such a sequence where each E_μ is of the form

$$E_\mu = (a_{\mu,1}, a_{\mu,2}, \ldots, a_{\mu,\nu}, \ldots)$$

and reminds the reader that "the $a_{i,\nu}$ are determinately *m* or *w*." We now define an element *b* of *M* which escapes the enumeration: "if $a_{\nu,\nu} = m$ then $b_\nu = w$, and if $a_{\nu,\nu} = w$ then $b_\nu = m$" (*EFM*, 921).

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[4] Abbreviations for some frequently cited titles will be introduced in this way when the work is first mentioned; the abbreviations will also be used in giving page references to these works. Page references to *B1* will be to Jourdain's English translation (CANTOR 1915).

[5] Page references to *EFM* will be to Ewald's English translation (CANTOR 1996b).

Cantor's description of the b_ν involves a striking confusion of types: he slides into thinking of the E_μ as sequences of characters, rather than sequences of elements *having* those characters. A similar confusion shows up a bit earlier, where he is giving examples of elements of M (*EFM*, 921); his three examples are

$$E^{I} = (m, m, m, m, \ldots)$$

$$E^{II} = (w, w, w, w, \ldots)$$

$$E^{III} = (m, w, m, w, \ldots)$$

The confusion does not invalidate the argument, of course; we need only modify the construction of b so that if $a_{\nu,\nu}$ has the character m we set $b_i = x^w$, where x^w is a member of the domain having the character w, and proceed similarly if $a_{\nu,\nu}$ has the character w. We could select x^m and x^w at the outset; in any case the reconstructed argument supposes that for each of the characters there is at least one element of the original domain with that character, an (innocuous) assumption that Cantor does not make explicit. Perhaps he was taking it for granted that his readers would think of the set $\mathbf{N}^+$ of positive integers as the domain, in which case there would be any number of possibilities (evenness and oddness, for instance) for m and w. He prefaces the first proof with a discussion of his earlier paper on the powers of **N** and **R**; and for that application of his general result, $\mathbf{N}^+$ would indeed be the domain of choice.

In the reconstructed argument, the underlying domain drops out, as it does in Cantor's original exposition; so do the characters m and w, in favor of two distinct individuals, say, 0 and 1. This is just the approach Cantor takes in the second argument in *EFM*, which he offers as evidence of the generality and power of diagonalization: "the principle followed [in the first argument] can be extended immediately to the general theorem that the powers of well-defined manifolds have no maximum, or, what is the same thing, that for any given manifold L we can produce a manifold M whose power is greater than that of L" (*EFM*, 921-922). The specific application he gives is the exhibition of a manifold whose power exceeds that of **R**. Thus he uses his new technique to take his earlier result of 1874 one step further; but the method he uses does not rely on any special features of **R** and so can be extended to any set of any power.

Cantor takes as his L the closed interval [0,1], and chooses as his M the set of all functions f mapping L into {0,1}. (I will call any function with this range a *binary function.*) He then offers a *reductio* to show that M and L cannot be of the same power. If they were, then there would be a bijection $\mathbf{\Phi}$ mapping each z in L to a function $\mathbf{\Phi}_z$ in M. That is, we would have $M = \{ \mathbf{\Phi}_z : z \in L\}$.[6] We can now easily define a g in M which is not in the range of $\mathbf{\Phi}$, completing our *reductio*. Let z be an arbitrary element of L. Then we set $g(z) = 0$ if $\mathbf{\Phi}_z(z) = 1$, and vice versa. Thus for each z in L, g disagrees with $\mathbf{\Phi}_z$ on z itself; so g cannot be $\mathbf{\Phi}_z$ for any value of z.

6 What Cantor actually says is that if L and M were of the same power then "M could be thought of in the form of a single-valued funct ion of the two variables x and z $\mathbf{\Phi}(x,z)$ such that to every value of z there corresponds an element $f(x) = \mathbf{\Phi}(x,z)$ of M, and, conversely, to every element $f(x)$ of M there corresponds a single determinate value of z such that $f(x) = \mathbf{\Phi}(x,z)$" (*EFM*, 922). I find it a bit more perspicuous to think of $\mathbf{\Phi}$ as mapping L onto M and to write the second argument of Cantor's function as a subscript.

All of this Cantor proved four years before the publication of *B1*. In *B1* itself he gives explicit definitions for some of the main concepts involved in *EFM*. He opens *B1*, §4, with the definition of what he calls a "covering of *N* with *M*," that is, a function from *N* into *M*.[7] He then defines $(N|M)$ to be the set of all such coverings; and defines μ^{ν} (where $\nu = |N|$ and $\mu = |M|$) to be $|(N|M)|$. This has become the standard notation for cardinal exponentiation, and I will adopt it here; I will use the more suggestive '${}^{N}M$' (cf. (Kunen 1980:31)) instead of Cantor's '$(N|M)$'.

According to Dauben (DAUBEN 1990: 175) Cantor hit upon these ideas just as *B1* was going to press, and prevailed upon Felix Klein, the editor of *Mathematische Annalen*, to add the new material in before publication. He had good reason to be so insistent, as he shows in his discussion of exponentiation in §4. He first observes that his exponentiation operator obeys the familiar laws of exponents, and then shows, by way of the binary representation for real numbers, that the power of the interval [0,1] is the same as that of ${}^{\mathbf{N}}\{0,1\}$, i.e., that $|[0,1]| = 2^{\aleph_0}$ (where $\aleph_0 = |\mathbf{N}|$). It then follows by the laws of exponents that $|[0,1]|^{\aleph_0} = |[0,1]|$. "Thus," Cantor concludes, "the whole contents [of his 1878 paper on dimension (CANTOR 1878)] are derived purely algebraically with these few strokes of the pen from the fundamental formulae of the calculation with cardinal numbers" (*B1*, 97).

There are two notable omissions here. The machinery Cantor sets up in *B1*, §4, suffices for an elegant recasting, not just of his 1878 paper, but also of his 1874 and 1891 papers on the nondenumerability of the reals. Perhaps he felt that there was no need for further illustration of the machinery's utility. In any case, there is no mention of diagonalization anywhere in *B1*. he second omission is any mention of the power set as such. What is really surprising, to one who knows these works only by reputation, is that there is no mention of the power set in *EFM* either; for that paper is very often said (for instance, by Ewald in the headnote to his English translation (CANTOR 1996b) of *EFM*) to "prove that, for any set *X*, the cardinality of the power set [of *X*]... is greater than the cardinality of *X*" (EWALD 1996: 920). That characterization is not altogether unjust; for it is a very short step from the conclusion of Cantor's second argument in *EFM* to the fact about power sets that we know as Cantor's Theorem: one need only remark that we obtain a bijection from *P(S)* onto ${}^{S}\{0,1\}$ by mapping each subset *T* of *S* to its characteristic function (that is, the function that is 1 on *T* and 0 outside of it). But Cantor does not take that step, or even pay any particular attention to *P(S)*;[8] this is an important difference, as we will soon see, from Peirce's treatment.

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7 This is another point at which I am simplifying Cantor for purposes of the present discussion: what he actually says is that a covering is a "*law* by which with every element *n* of *N* a definite element of *M* is bound up" (*B1*, 94) [my emphasis]. The intensional strains in both Cantor's and Peirce's thinking and ways of speaking about *Mengen* (collections) need a much more searching comparative study, which I intend to provide in work now in progress.

8 The same goes for the letter to Dedekind (CANTOR 1996a: 939-940) in which Cantor explains his Paradox: his Theorem is invoked as a way of generating, from a given set, a set of higher cardinality, and not as a fact about the cardinality of power sets in particular.

3. The Step Lemma: What and When

In the January 1897 issue of the *Monist* Peirce published a review essay, entitled "The Logic of Relatives" (*LR*) (PEIRCE 1897a),[9] on the third volume of Schröder's *Vorlesungen über die Algebra der Logik* (SCHRÖDER 1895). The final section of that paper, "Introduction to the Logic of Quantity," contains the following, rather charming, diagonalization:

> [I now] ask whether the multitude of possible ways of placing the subjects of a collection in two houses can equal the multitude of those subjects. If so, let there be such a multitude of children. Then, each having but one wish, they can among them wish for every possible distribution of themselves among two houses. Then, however they may actually be distributed, some child will be perfectly contented. But ask each child which house he wishes himself to be in, and put every child in the house where he does not want to be. Then, no child would be content. Consequently, it is absurd to suppose that any collection can equal in multitude the possible ways of distributing its subjects in two houses (*LR*, 548).

Here Peirce shows that for any multitude M, $M < 2^M$. He does not assume that there are only finitely many children, and he immediately exploits this generality by drawing a corollary about *infinite* multitudes: "Accordingly, the multitude of ways of placing a collection of objects abnumeral of the first dignity into two houses is still greater in multitude than that multitude, and may be called abnumeral of the second dignity" (*LR*, 549). This is just the conclusion of Cantor's second argument in *EFM*. By 'abnumeral' Peirce means what we mean by 'uncountable'. His multitude "abnumeral of the first dignity" thus corresponds to Cantor's $\aleph_1$, and that "of the second dignity" to Cantor's $\aleph_2$. (The correspondence is only partial: the two pairs of numbers correspond in being, for their respective authors, the first two uncountable infinite sizes, but Peirce thought he knew something about how to generate his abnumerals that Cantor realized that he did not know.)

So far, so good. But now Peirce jumps to some more questionable conclusions:

> There will be a denumerable succession of such dignities. But there cannot be any multitude of an infinite dignity; for if there were, the multitude of ways of distributing it into two houses would be no greater than itself (*LR*, 549).

Peirce does not fully speak his mind in the first of these two sentences. If we let M_0 = $\aleph_0$, and let $M_{i+1} = 2^M$ for every natural number i, then the first sentence says that there is an infinite sequence $\{M_i: i \in \mathbf{N}\}$ formed in this way. What Peirce does not say here is that for each i, M_{i+1} is the *next* multitude after M_i: that there are no multitudes in between any adjacent members of his sequence. However, it is clear from roughly contemporaneous writings that he did believe that, when he wrote *LR*,[10] and so far as I

[9] On conventions for citing works by Peirce, see the "Note on Peircean Citations" preceding the reference list for this essay.

[10] It is quite explicit in "On Quantity": "there will be a denumerable succession of these abnumerals, numbered according to the finite whole numbers [...] there can be no

can tell he believed it until the day he died.[11] This is why I call Peirce's result his *Step Lemma*—because it tells us how to step from one infinite multitude to the next; or rather, the Lemma does that when combined with the assumption Peirce leaves unstated in *LR*, which amounts, for all intents and purposes, to what is now known as the Generalized Continuum Hypothesis (GCH).[12] His final corollary says that the M_i ($i \in \mathbf{N}$), are all the multitudes there are.

Both of these "corollaries" are highly problematic, according to present-day theories of infinity. The GCH is independent of the generally accepted axioms of set theory (as encoded in Zermelo-Fraenkel set theory with Choice, or ZFC); indeed, the majority of set theorists nowadays would reject it as a general truth about sets (though it holds in some important models of ZFC, most notably in Gödel's class **L** of constructible sets.) The rejection of multitudes of "infinite dignities" is likewise incompatible with ZFC: the axiom of Replacement, which has won universal acceptance among set theorists, implies the existence of $\aleph_\omega$ (ω being Cantor's first infinite ordinal number, the least ordinal greater than all the natural numbers).[13]

Peirce, however, denies that there is an M_ω after all the M_i of "finite dignity." He does not give his argument for this in *LR*, but he hints at it in the concluding sentence of the passage just quoted. There Peirce asserts that if there were a collection C_ω of multitude M_ω, then we would have

$$|C_\omega| = |P(C_\omega)|$$

multitude intermediate between these multitudes" (PEIRCE 1896[?]-a: 52). As I will argue below, this text is almost certainly earlier than *LR*. See also the third Cambridge Conferences Lecture: "the multitude of irrational quantities [...] I term [...] the *first abnumeral* multitude. The next multitude is that of all possible collections of collections of finite multitudes. I call it the *second abnumeral multitude*. The next is the multitude of all possible collections of collections of collections of finite multitudes. There will be a denumeral series of such abnumeral multitudes" (PEIRCE 1992b: 157-158).

11 See e.g. PEIRCE 1908a: 654.

12 Putnam and Ketner (PEIRCE 1992b: 275, note 75) also ascribe GCH to Peirce. Their reading of his set theory overall (PEIRCE 1992b: 46-47) is largely the same as mine, though I have serious qualms about their discussion of Ω, which is supposed to be "the cardinal of the universe of sets" (PEIRCE 1992b: 47); this seems to me to oversimplify — or at least to facilitate the oversimplification of — a question that for Peirce was very complex. Myrvold (MYRVOLD 1995: 514-515) reviews some of Peirce's attempted arguments for the Continuum Hypothesis and concludes: "Unable to prove the truth of the generalized continuum hypothesis, Peirce for the most part simply assumed it to be true."

13 Set theorists might prefer to say that Peirce rejects $\beth_\omega$ rather than $\aleph_\omega$: the $\beth$s are the sequence of cardinals obtained by exponentiation in Peirce's manner (allowing infinite indices), and in the absence of the GCH need not be identical with the whole sequence of cardinals. On the motivations for Replacement, and the connection with $\aleph_\omega$, see (MADDY 1997: 57-60). Interestingly, Quine demurs somewhat from the consensus, even expressing discomfort with $\beth_\omega$, though for different reasons from Peirce's: "I recognize indenumerable infinities only because they are forced on me by the simplest known systematizations of more welcome matters. Magnitudes in excess of such demands, e.g., $\beth_\omega$ or inaccessible numbers, I look upon only as mathematical recreation and without ontological rights" (QUINE 1986: 400). Peirce actually uses '$\beth$', with the standard definition for finite indices, in his letter to Cantor (PEIRCE 1900b: 778).

contrary to the Step Lemma. This is very reminiscent of Cantor's Paradox, laid out in the famous letter to Dedekind of August 1899 (CANTOR 1996a: 939-940), so I will call this claim about M_ω Peirce's *Paradox of Multitude*.[14] Peirce lays out his "proof" of the Paradox in full in an untitled manuscript (PEIRCE 1897[?]) from around 1897, to which Richard Robin has assigned the title "On Multitudes."[15] The argument rests on a fallacious analysis of infinite exponentiation. Since every infinite multitude of "finite dignity" can be written as a finite staircase of twos with M_0 at the top, the first multitude of infinite dignity would be a countably infinite staircase of twos with M_0 at the top. Let C_ω be a collection of that multitude. Then $P(C_\omega)$ would be $M_{\omega+1}$, obtained from $|C_\omega|$ by adding one more two to the staircase that fixes the latter. But if we add one step to a countably infinite staircase, the result is no different from the staircase we began with. So we have

$$|C_\omega| = |M_\omega| = |M_{\omega+1}| = |P(C_\omega)|$$

contrary to the Step Lemma. So we must reject our initial assumption that there is a multitude of infinite dignity.[16]

The fallacy in the argument has been well explained by John Myhill and Wayne Myrvold:[17] it turns on a confusion between $\omega+1$ (the order type of the sequence consisting of $\mathbf{N}$ and one new element at the end) and $1+\omega$ (the order type of the sequence obtained by adding a new element at the beginning of $\mathbf{N}$). Peirce thinks that the order type of the infinite exponentiation defining M_ω would be $1+\omega$, which he correctly observes is the same as ω itself. But the order type is actually $\omega+1$, which is *not* the same as ω.

14 For a very thorough discussion of this Paradox, to which my own is very much indebted, see (MYRVOLD 1995). My analysis of Peirce's fallacy is pretty much a paraphrase of Myrvold's (MYRVOLD 1995: 516-517).

15 The manuscript is undated, but it can be placed around 1897 because of its close connections with another manuscript entitled "Multitude and Number" (PEIRCE 1897b), which can confidently be placed in that year because it contains a reference to 1899 as "the year after next" (PEIRCE 1897b: 172). "Multitude and Number" contains a somewhat confused version of the argument for the Paradox of Multitude, which is taken to prove that there *is* a multitude larger than all of the multitudes in Peirce's countable sequence of abnumerals (PEIRCE 1897b: 218); "On Multitudes," by contrast, gives a fuller and clearer exposition of the argument and comes to the more satisfactory conclusion that a collection whose multitude exceeded all of the abnumerals M_i, i finite, could not strictly speaking be said to *have* a multitude (PEIRCE 1897[?]: 86). Overall, "On Multitudes" gives the impression of being a more concise and confident attack on some of the issues treated in "Multitude and Number." So it is probably somewhat later; but the the two texts are obviously closely linked. (Readers who follow up my reference to "Multitude and Number" should be aware that Hartshorne and Weiss have taken serious liberties with Peirce's notation: see (DAUBEN 1995: 157-159).

16 Peirce gives a somewhat different argument for the Paradox in one of his draft letters to Cantor (PEIRCE 1900b: 777-778). Since it substantially postdates Peirce's initial discovery of the Step Lemma, I will not go into it here; there is some discussion of it in (HERRON 1997: 625-627).

17 Murphey offers this analysis in his discussion of the Paradox and attributes it to Myhill (MURPHEY 1961: 262); for Myrvold's version, see (MYRVOLD 1995: 517).

(More intuitively, Peirce's error was to forget that each new exponentiation involves the addition of a two at the bottom of the staircase, which means that the infinite exponentiation corresponds to a staircase with no bottom step. A new step added after all the steps in this infinitely descending staircase is unlike all its predecessors [except for the topmost step] in having no *immediate* predecessor; so its addition to the staircase does change the order type.)

The manuscript of "The Logic of Relatives" was delivered to *The Monist* in August of 1896. So we know that Peirce had found the Step Lemma, and drawn the (supposed) corollaries by August of 1896. We can push the date of discovery back even further by examining a letter Peirce wrote to Francis Russell on 26 April 1896. In the final paragraph of that letter Peirce writes:

> I have just completed a memoir I intended reading to the National Academy of Sciences in Washington this last week. But I was unable to get there. In the introductory part of this memoir I undertake to state in general terms what is logically possible, and what not. This assumes that some things are impossible although they do not involve any contradiction, such for example as that there should exist only two or only three things, that there should be a relation which could not exist between a certain set of things although there were no contradiction involved, etc. etc. Having thus described the logically possible, I go on to consider the multitudes of collections. I succeed in that way in proving that of two collections not equal one must be greater than the other; although there is no contradiction in supposing that in every possible way of setting them off into pairs, one object of each pair belonging to the one collection and the other to the other, there should always remain unpaired objects among both collections. I also show that greater than the collection equal to all finite whole numbers there are a series of possible collections each next greater than the last, but infinitely greater than that last; and these collections are equal, in the multitude of them, to the finite whole numbers; and greater than them all is a possible collection, than which no collection can be greater. (PEIRCE 1896a: 965)

Though Peirce does not mention the Step Lemma itself here, he does list all of its major corollaries *except* for the Paradox of Multitude; the most natural explanation, by far, is that he had found the Lemma by the time he wrote to Russell. His mention of a greatest possible collection shows some uncertainty about abnumerals of "infinite dignity" (cf. note 15), but there is a postscript, scribbled on the first page above the salutation, in which he reports, along with some other miscellaneous results about infinite multitudes, the leading "insight" behind the Paradox of Multitude: "The maximum collection equals its own exponential" (PEIRCE 1896a: 965). So all of the pieces for the discussion in *LR* were in place by the end of April 1896.

This *terminus ad quem* for the discovery of the Step Lemma is about as solid as such things get, in the fragmentary world of the Peircean corpus; a *terminus a quo* is a more complicated question. I will make a tentative case here for early 1896, based on three pretty much indisputable facts, one fairly well-supported hypothesis, and two more speculative conjectures. The facts are that (F1) Volume 46 of *Mathematische Annalen*, in which *B1* first appeared, was stamped as received by the Astor Library in New York City on 11 January 1896; that (F2) Peirce signed the Astor Library's visitor's log on 18 January 1896, stating his intention to consult works on mathematics, and did not sign in again until 29 April of that year; and that (F3) the manuscript "On Quantity" (*OQ*) (PEIRCE 1896[?]-a) which contains a diagonal argument for the Step Lemma, can only have been written *after* Peirce saw *B1*. The hypothesis is that *OQ* is earlier than Peirce's letter to Russell, and is in fact Peirce's earliest surviving proof. The conjectures are that (C1) Peirce first read *B1* at the Astor Library on 18 January 1896, and that (C2) this precipitated his discovery of the Step Lemma. By itself, (F3) yields a *terminus a quo* some time in the last third of 1895. This is almost certainly too wide a window, however. Adding in the rest of what we know or can reasonably suppose, we arrive at the conclusion that Peirce discovered the Step Lemma some time between 18 January (when he saw *B1* at the Astor) and 24 April 1896, when he wrote to Russell.

The evidence for (F1) and (F2) is in the collection of the New York Public Library: the Astor's date-stamped copy of *Mathematische Annalen*, 46, is still in the general collection; and the visitor's log is Volume 151 of the Astor Library Records on deposit with the Library's Manuscripts and Archives Division.[18] As for (F3), the priority of *B1* to *OQ* is pretty decisively established by Peirce's use in the latter of Cantor's notation for cardinal exponentiation (*OQ*, 54-57), which made its first appearance in the former.[19] Cantor communicated the discovery of exponentiation to Felix Klein in a letter dated 19 July 1895 (DAUBEN 1990: 175); it is therefore just possible that Peirce learned about it through the scientific grapevine, in which case he could have written *OQ* as early as, say, mid-August of 1895. But I know of nothing that indicates that Cantor's new ideas were circulating through any grapevine that Peirce was plugged into. Since we know that he could have seen *B1* at the Astor in January of the following year, it is far likelier that he first learned of cardinal exponentiation then.

This is of course just my first conjecture, (C1). It is no more than a conjecture, because such positive evidence as we have is consistent with Peirce's not having seen *B1* at the Astor on that day, or with his having first seen it elsewhere. At this point any alternative account would be more speculative still.[20] The evidence for my hypothesis,

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18 Astor Library Records, Manuscripts and Archives Division, The New York Public Library.

19 A further, though less decisive, argument for the priority of *B1* is Peirce's definition in *OQ* of "less than" for multitudes. It is almost exactly Cantor's from *B1*, §2, and is attributed to him. Cantor's definitions have been given on p. 3 above. Here is what Peirce says in *OQ*: "The multitude of one collection is defined by G. Cantor to be *less* than another when the members of the former can be put into one-to-one correspondence with a part of the members of the latter, while all the members of the latter cannot be put into one-to-one correspondence with members of the former collection" (*OQ*, 49). Unlike the definition of exponentiation, this did not debut in *B1*; it had already appeared in (Cantor 1890) (the definition is on p. 413 in Zermelo's edition (CANTOR 1960)). Peirce did have a copy of this work, but it was sent to him by Cantor in 1901 (see p. 26).

20 One alternative looks plausible enough to be worth eliminating. The bound volume referred to in note 27 below contains Gerbaldi's Italian translation of *B1* (CANTOR 1895c). Could that not have been the first version Peirce saw? Probably not, for two reasons. The handwriting of the marginal annotations in the translation puts them after 1900. Also, we have manuscript notes on the German original (PEIRCE 1896[?]-b), whose handwriting dates them in the late 1890s. They appear to reflect a *first* reading of the sections that influence *OQ*: Peirce lodges criticisms against Cantor (for instance, that he omitted a proof of Cardinal Trichotomy) that he would have made *only* very early on. So it was very

about the priority of *OQ* to the letter to Russell, is a good deal stronger. The letter clearly alludes to the Paradox of Multitude, which is missing from *OQ*. This does not quite clinch the matter, because Peirce could have simply neglected to mention the Paradox in *OQ*. The obvious importance of the Paradox makes it hard to believe, though, that he would have failed to mention it if he had known about it. Moreover, what he does say there strongly suggests that he was not yet in a position to derive the Paradox, which relies on his "infinite staircase" construction (see p. 11 above). There are some relatively small finite staircases in *OQ* (55-57), but Peirce says nothing about the infinite case.

Overall the state of Peirce's thinking in *OQ* corresponds quite closely to what he reports in the main body of his letter to Russell. (Recall that the Paradox comes in only in the postscript.) Though there are some differences in the order of topics, the contents of §4 of *OQ*, the section on multitude, pretty closely match the memoir Peirce describes in the letter: after some introductory remarks on multitude (including Cantor's definition of "less than": *OQ*, 48-49), Peirce raises the question of cardinal trichotomy, and after a brief discussion of the general issue of logical possibility, gives a "proof" of Trichotomy (*OQ*, 49-50). This is followed by a proof that the multitude of the natural numbers is the first multitude greater than them all (*OQ*, 50-51); then comes the Step Lemma and the description of the sequence of abnumerals (*OQ*, 51-58).

But there are differences as well, all pointing towards an earlier date for *OQ*. First and foremost, in *OQ* Peirce denies (*OQ*, 52), and in the letter affirms, the existence of a multitude beyond the finitely indexed abnumerals. As evidence for chronology, this seems ambiguous at first, because he appears to vacillate between affirmation and denial in later writings on collections. For instance, in "Multitude and Number," which dates from 1897 (see note 15), he affirms the existence of such a multitude. In the Cambridge Conferences Lectures of 1898, however, he takes a different line. He does allow there for the existence of collections that in some sense exceed, in multitude, all collections whose multitudes are (as he puts it in *LR*) of "finite dignity." These are continuous collections, whose members do not have distinct identities; Peirce is careful to insist that a continuous collection does not have a multitude as a collection of distinct individuals does.

Even this very abbreviated survey should show that 'vacillation' does not do justice to Peirce's struggles with this question: there is a dialectic here, in which earlier positions are corrected but not completely discarded by later ones. Accustomed as we are to the high-flying infinities of set theory, we may find Peirce's countable sequence of abnumerals to be a rather paltry thing, but it would not have seemed so in the freshly uncovered light of the Step Lemma. The infinity, rather than the countability, of the sequence would be its salient feature, and so it is in *OQ*: just after describing the sequence in *OQ* Peirce writes, "There can be no maximum multitude" (*OQ*, 52) rather than "there is no

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likely the German original, and not the Italian translation, that he saw first. I suspect that Gerbaldi's translation was sent to Peirce by Cantor in their exchange of letters; Peirce's drafts of his first letter to Cantor date from December 1900. Taken together with the handwriting evidence, this puts Peirce's first inspection of the translation after the turn of the century. (I am much indebted here to Professor André De Tienne's expert knowledge of Peirce's handwriting.)

multitude beyond all these." The infinite staircase argument, with its roughly equal measures of error and brilliant insight, got him thinking about a multitude beyond, and in both of the later texts he sticks to the affirmation that there is *something* beyond; what remains in doubt is how to characterize its transcendence in terms of multitude. None of his answers is completely adequate, but their sophistication increases monotonically if we put *OQ* at the beginning of the development.

The remaining indications are more straightforward. The discussion of logical possibility in *OQ* is brief and lacks one of the examples Peirce mentions to Russell ("that there should be a relation which could not exist between a certain set of things although there were no contradiction involved"); moreover, the discussion of cardinal trichotomy in the letter shows a more adequate appreciation of the depths of the issue, when compared with the somewhat offhand treatment it receives in *OQ*. Most likely *OQ* is an earlier version of the memoir Peirce describes to Russell; or it may, since none of the surviving manuscripts perfectly fits Peirce's description,[21] have been the draft Peirce then had in hand, in which case what he described to Russell was a projected improvement of what he had already written. Clearly he was still in the heat of discovery.

If the reader has come with me this far, she will agree that the proof in *OQ* was written no later than 29 April 1896. It is therefore earlier than the other proofs of the Step Lemma mentioned to this point: *LR* was written that summer; "Multitude and Number" dates from 1897, and the Cambridge Conferences Lectures from late 1897/early 1898 (PEIRCE 1992b: 19-35). Every other proof of the Lemma that I have found in Peirce's writings is even later than this. Robin gives 1899-1900 as the date for (PEIRCE 1899-1900: 467-469), and 1904 as the date for (PEIRCE 1904: 19-23). Peirce's letter to the editor of *Science*, which contains a proof of the Lemma, was published in 1900 (PEIRCE 1900a: 566). There is a proof of the Lemma in his definition of 'Mathematical Logic' (PEIRCE 1900(?): 745-746) for Baldwin's *Dictionary*, which was published in 1901. Peirce proves the Lemma in one of his Lowell Lectures from 1903 (PEIRCE 1903b: 385-387). There are three versions of a proof in drafts of a letter to E.H. Moore (PEIRCE 1903a), written in December of that year; Eisele prints one of them in the third volume of *New Elements of Mathematics* (pp. 922-923), and the others can be found on pages 27-28 and 35-37 of the manuscript. Peirce gives the proof in "Prolegomena to an Apology for Pragmaticism" (PEIRCE 1906:532), which appeared in *The Monist* in 1906. The latest proof known to me is in the letter to Jourdain (PEIRCE 1908b: 883-885), which was written at the end of 1908. That leaves the proof in Ms 33 (PEIRCE 189[?]: 4-6), which is undated. However, as Peirce develops the Paradox of Multitude there, and the Paradox is absent from *OQ*, it is likely that Ms 33 is later.

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21 There is a lot of overlap between *OQ* and MS 15, which opens with a promise to treat exactly the same topics as *OQ*: the nature of mathematics, quantity, continuity, infinity and cardinal trichotomy. Both manuscripts are entitled "On Quantity, with special reference to Collectional and Mathematical Infinity," and both are labelled as "memoirs." It is hard to escape the conclusion that these are two versions of the same memoir, with *OQ* the later and more fully developed of the two.

If this is a complete inventory of Peirce's proofs of the Step Lemma, and all the dating is correct, then the proof in *OQ* is indeed the earliest one we have. Not having read every word Peirce wrote, I cannot be sure that the inventory *is* complete: something might turn up that is earlier than everything I have listed here. So the strong form of my hypothesis, that *OQ* is the earliest, could be overthrown by further evidence. But even allowing for that possibility, I think that the evidence I have provided very strongly supports the weaker hypothesis that *OQ* is very close to Peirce's first discovery of the Lemma, and this is really all that I will need in the sequel, where I will appeal to the *OQ* proof in support of my suggestion as to how Peirce made the discovery in the first place. My argument for the weaker hypothesis rests on a comparison of *OQ* with all of the later texts I have identified. We will see in a moment that the *OQ* argument is a *logician's* proof in a way that all the rest are not; and it is not uncommon for the proofs that mathematicians present to others to show no trace of the often inelegant machinery that facilitated the initial insight. In *OQ* Peirce analyzes the problem in terms of possible assertions; this approach quickly gives way to the heuristic of "sorting into two houses" that he uses in *LR*, and this in turn gives way in the Cambridge Conferences Lectures (see p. 21 below) to an even slicker argument which then becomes (with variations and elaborations) his preferred method of proof thereafter.

The *OQ* proof, then, uses an approach which Peirce quickly and permanently abandoned; thus it is very likely to have been written down soon after Peirce's initial discovery of the Step Lemma. All that remains, then, is to argue for (C2), my guess about how Peirce arrived at his diagonalization.

4. The Step Lemma: How

It will be helpful to have the full text of Peirce's proof from *OQ* before us:

> We now come to a theorem of prime importance in reference to multitudes. It is that the multitude of partial multitudes composed of individuals of a given multitude is always greater than the multitude itself, it being understood that among these partial multitudes we are to include *none* and also the total multitude. Since we are only inquiring whether the grade of multitude can of itself prevent the formation of partial multitudes whose multitude is greater than the primitive multitude itself, it can make no difference what kind of objects the individuals may be. To fix our ideas, then, let there be a collection of individuals the *S*s, which may be numerable or innumerable. Take any predicate *p*; and consider all those possible assertions each of which in reference to each *S* either affirms *p* or denies it. Call these assertions the *A*s. Now I say that if there be any relation, *r*, such that every *A* is in that relation to an *S*, — or in briefer phrase, such that every *A* is *r* to an *S*, — then there must be two different *A*s which are *r* to the same *S*. For if this were not the case, an absurdity would result as would readily be shown in two ways, which do not, however, differ substantially.
>
> The first way depends upon the fact that of all possible assertions as to what *S*s are and what are not *p* some one must be true, whatever the facts may be. Suppose, then, that every *S* to which an *A* is *r* had the quality in reference to *p* altered if necessary, so as to make that *A* false of that *S*. If then every *A* were *r* to an *S*, and no two to the same *S*, every *A* would become false. That is, every

> possible assertion would be false, which is absurd.
>
> The second way of showing the absurdity consists in showing that as long as every *A* is *r* to an *S*, and no two to the same *S*, there is a possible assertion omitted. Namely, form an assertion by taking each *A* finding the *S* to which it is *r*, and contradicting this *A* in reference to this *S*. If there are any *S*s to which no *A* is *r*, it makes no difference whether the new assertion affirms or denies *p* of them. This new assertion plainly is inconsistent with every one of the *A*s, that is with every possible assertion, which is absurd.
>
> It is therefore absurd to suppose that the multitude of classes formed from the individuals of a collection (including 0 and the whole collection) should be no greater than the multitude of the collection itself. (*OQ*, 51-52)

There is a very strong resemblance between these arguments and Cantor's first diagonalization in *EFM*: Cantor begins with two mutually exclusive characters, Peirce with a predicate *p* and its negation; Cantor replaces each element of each sequence with an object having the opposed character, Peirce alters the character of the element. So it is fair to say that the underlying idea of Peirce's arguments is the same as that of Cantor's: they are all diagonalizations. But there are also differences. For one thing, Cantor's second argument uses a function whose range is the set that turns out to be larger, while Peirce's arguments turn on a relation that has the larger collection as its domain. Cantor proceeds by showing that his function cannot be onto, Peirce by showing that his relation cannot be one-one. Indeed, the more closely one looks at Peirce's arguments, the more sharply the differences stand out between the two men's approaches. Since this is (if my chronology is correct) a very early proof, these differences tell us something about the *origins* of Peirce's discovery.

Cantor's first argument has an intensional element which he negotiates rather awkwardly, and which lands him in a type confusion which he then overcomes in his second, more fully extensional argument.[22] Peirce's arguments are intensional throughout, and suffer from a different awkwardness which he never overcomes in *OQ*. Let φ be the assumed correspondence between *S* and *A*. (For ease of exposition, I am trading in Peirce's relation for a function, and reversing its direction.) In his first argument, Peirce describes a possible situation by supposing, for each *s* in *S*, that *p* holds for *s* if $\varphi(s)$ says otherwise (and similarly, *mutatis mutandis*, if $\varphi(s)$ says that *p* does hold for *s*); in his second he constructs a proposition by asserting, for each *s*, the opposite of what $\varphi(s)$ says about it. (If *S* is infinite, then each element of *A* will presumably be an infinite conjunction, saying for each *s* whether *p* does or does not hold for it.) The result in the first argument is a possible situation described by no proposition, and in the second a

[22] The second argument *is* fully extensional in the sense that its functions might as well be just sets of ordered pairs. Cantor himself was still using intensional *language* in talking about functions when he wrote *B1*: for example, he describes a covering function as a "law by which with every element *n* of *N* a definite element of *M* is bound up ..." (*B1*, 94). I see no reason to think that the issue of intensionality was even in the back of Cantor's mind when he wrote *EFM*, though he may well have recognized the awkwardness of his first argument. Peirce, on the other hand, was deeply and consciously concerned with issues that we would now recognize as bound up with intensionality.

proposition contradicting every proposition. As Peirce observes, the two arguments are at bottom one; for propositions and possible situations go hand in hand.

The awkwardness in Peirce's arguments is that his diagonalizations require him to change the status of his individuals, with respect to the predicate *p*, at will, or at least (as we might put it) to consider a possible world in which that status was different. This is not quite the same as the awkwardness in Cantor's first argument, but it is close: Cantor's idea is to substitute different objects with different properties, while Peirce's is to retain the objects but modify their properties. How much sense such modification makes depends upon the *S*s, and upon *p*: it seems all right if the *S*s are human beings and *p* is "*X* is right-handed," but not if the *S*s are integers and *p* is "*X* is odd." Peirce dispels the awkwardness in *LR* by making a clever choice of collection and predicate. Of course the *LR* proof *is* using binary functions, in all but name: just label the two houses 0 and 1, respectively. In fact, if we look back at *OQ* we see that Peirce already knew that "any collection whose multitude is 2^m can be put into complete correspondence with the collection of possible ways in which a collection of places of multitude *m* can all be filled, each either with a 0 or a 1" (*OQ*, 56). But he uses the correspondence, not to make a diagonal argument about multitude, but rather to make an argument about linear orderings. This reflects the content of *B1*, where Cantor uses his covering functions to define exponentiation but not for diagonalization.

In comparing the two men's proofs, we should perhaps not lean too hard on the idea that the *LR* proof "really" uses binary functions. From where we stand in the historical development of set theory, the idioms of binary functions, subset formation and sorting into houses are readily interchangeable. Yet Cantor used only the first, and Peirce pretty much stuck to the second and third as far as the Step Lemma was concerned. The residual traces of their correspondence make it clear that they did realize that they were talking about the same thing. But the consistent differences in their ways of talking about it suggest, especially in the nascent state of the subject, different angles of approach.

The proof in *LR* does eliminate the intensional clumsiness of the *OQ* proof, but the price of clarity is an admixture of non-mathematical metaphor. Soon thereafter, in the Cambridge Conferences Lectures of 1898 (PEIRCE 1992b: 158), Peirce succeeded in eliminating the metaphor as well, not by adopting the Cantorian idiom of binary functions, but by brilliantly exploiting the idiom of subsets: what he does, in effect, is to consider a function ξ mapping *S* into *P(S)*, and the collection *D* consisting of exactly those elements *s* of *S* such that $s \notin \xi(s)$. It is then easy to verify that *D* is not in the range of ξ.[23] This is true to form: from the outset Peirce saw the Step Lemma as a fact about power collections. This is how he announces the result in *OQ*, though the proof is not directly about subcollections of *S*, but rather about assertions about the members of *S*. When the proof is finished he restates the result, but this time with a revealing difference: where in the initial statement he said that the Lemma was about "the multitude of partial multitudes composed of individuals of a given multitude," in the summation he says that it is about "the multitude of *classes* formed from the individuals of a collection (*including 0* and the whole collection)" [emphasis added]. A 21st century reader, coming to this

[23] This has become a standard approach to Cantor's Theorem: see, e.g. John Burgess's introduction (p. 136) to Part II of (BOOLOS 1998).

passage with the now-standard notion of power set, might feel that some connecting material has been omitted: no doubt a little reflection will bridge the gap between Peirce's collections of assertions (or his housing plans for children) and power sets, but it is rather inconsiderate of him to leave that work to the reader. The phrases I have italicized in the foregoing quote suggest a reason why Peirce might not have seen the need for further reflection. The unremarked substitution of the Boolean term 'class' for 'collection' and the use of '0' to denote the empty collection are vital interpretive clues. Good Boolean that he is, Peirce thinks of a predicate as a way of selecting some class of individuals out of a background collection;[24] he does not explain the connection between all the subcollections of *S*, and all the ways the predicate *p* could hold (or not) of *S*'s members, because the connection goes without saying.

All this puts us in a position to flesh out my earlier remark about different angles of approach. Both Peirce and Cantor were looking for ways to generate ever larger infinities.[25] Cantor attacked the problem directly, generalizing the strategy he had followed to good effect in the proto-diagonal arguments from his 1874 paper and §12 of the *Grundlagen* (CANTOR 1883c: 909-910), namely, assuming the existence of a certain kind of enumeration, and then showing that there is an element that escapes enumeration.[26] The overtly logical, intensional scaffolding of his first proof in *EFM* falls away in his second; the leading idea of that second proof is isolated and generalized after the fact, in the definition of cardinal exponentiation. Power sets remain, unmentioned, in the wings throughout.

For Peirce, on the other hand, power collections begin at center stage — it is through them that he finds his unbounded sequence of multitudes. If the proof in *OQ* is as early as I believe it to be, then it will probably still hew pretty closely to the line of discovery. So it is highly significant, for our genetic question, that Peirce uses Cantor's notation for cardinal exponentiation as he does there, and that he states the Lemma in the first place as a fact about power collections. A few pages after the proof, he says that if we let *D* be the multitude of the natural numbers, then "the first abnumeral being the multitude of ways in which *D* things can be distributed into 2 places is 2^D" (*OQ*, 54-55). Here he uses the heuristic language of sorting into two places that he will use in proving the Lemma in *LR*. And it is clear that already in *OQ* he sees this *as* a way of designating $|P(D)|$, and not just as a way of explaining what the value of the exponentiation 2^D is; for just after the proof of the Step Lemma, he identifies the first abnumeral (i.e., 2^D) with the multitude "of possible groups of cardinal [i.e., natural] numbers" (*OQ*, 52). So the intimate connection between power sets and cardinal exponentiation, not made explicit by Cantor in *B1*, was highly salient for Peirce.

I submit that it was this very connection that led Peirce to the Step Lemma in the first place. Having noted the connection, and seen how deftly Cantor used exponentiation

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[24] "The symbol *x* operating upon any subject comprehending individuals or classes, shall be supposed to select from that subject all the *X*s which it contains" (BOOLE 1847:15).

[25] As the passage from the *Grand Logic* given below (p. 30) makes clear, Peirce's interest in the matter goes back at least as far as 1893.

[26] For a comparative discussion of Cantor's proto-diagonal and diagonal arguments, see (HALLETT 1984:75-81).

to label and reason about cardinalities in *B1*, it would have been natural for Peirce to ask himself how *P(S)* compared in size with *S* itself. In his first statement of the Step Lemma he insists that *P(S)* must include both the empty collection, and *S* itself: this is essential if the Lemma is to hold for the finite exponents 0, 1 and 2, and suggests that he had done some playing with simple cases, which would have suggested to him that in general $|P(S)|$ should be strictly larger than $|S|$, and obtainable by exponentiation base 2. He had certainly seen Cantor's proto-diagonalizations;[27] and these would have planted in his mind the strategy of assuming the existence of a bijection and then constructing an element that was not in its range. It stands to reason that a logician like Peirce would think about power collections in terms of possible assertions. Put all of this together in a first rate mathematical mind, and a diagonal argument much like that in *OQ* could very well pop out.

To sum up, then, my aetiological hypothesis is that Peirce's first reading of *B1* got him thinking about what we would now call power sets, and their multitudes, and that these reflections led him to a general diagonal argument whose initial rough edges he continued to smooth off over the coming years. To be sure, he was standing on Cantor's shoulders, but plenty of other mathematicians had stood there without seeing what Peirce saw; so my hypothesis credits Peirce with a mathematical insight of the highest order. But there is plenty of independent reason to believe that Peirce was capable of such insight: he was the most promising son of America's foremost mathematician, and at Johns Hopkins he interacted as an equal with Sylvester and Cayley. Still, the admitted similarities between Peirce's arguments and Cantor's, and Cantor's indisputable priority of publication, cannot help giving one pause. To make matters worse, it is known that Peirce possessed an offprint of Vivanti's Italian translation of *EFM*, which appeared in the *Rivista di Matematica* in 1892,[28] four years before his first published diagonalization in *LR*. Surely the natural conclusion to draw from all of this is that Peirce's Step Lemma just *is* Cantor's Theorem, deliberately plagiarized.

The crucial point, in assessing the charge of plagiarism, is of course not when *EFM* was published, but rather when Peirce first saw it. The documentary record is, as usual, patchy; but what there is points clearly to 1901 as the likeliest date for Peirce's receipt of the Vivanti translation. In a draft letter to Christine Ladd-Franklin, dated 12 January 1902, Peirce writes:

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[27] It is one of the (few) fixed points of Peirce's history with Cantor that he first encountered the latter's work through the French translations that appeared in *Acta Mathematica* in 1883: Peirce's most detailed account of this is in a draft letter to Jourdain from 1908 (Peirce 1908b:883). Both the relevant portion of the *Grundlagen* and the 1874 paper were translated there.

[28] Carolyn Eisele incorrectly asserts (EISELE 1976:vii) that Peirce had this offprint bound, along with other works on logic and set theory, in a volume now among his papers at Harvard's Houghton Library (call number Math 205.34*, Houghton Library). There is an Italian translation from *Rivista di matematica* in that volume: Gerbaldi's translation of *B1* (CANTOR 1895c) from 1895. (See note 28 for more on the contents of this volume.) This seems to have been a slip on Eisele's part: elsewhere (EISELE 1979a:227) she gives a list of the contents of the volume that mentions the Gerbaldi translation but not the Vivanti. The Vivanti offprint is in MS CSP 1600, Box 4, at Houghton.

> Now there is no greatest collection, as I have very prettily proved. (But Cantor marks a passage in one of his papers which he sends to me from which it appears that he had reached the same result by means of his ordinal numbers.) (PEIRCE 1902: 180)

Among Peirce's papers there are a number of works of Cantor's;[29] the offprint of *EFM* is the only one of them with a marking answering Peirce's description, and it answers perfectly. The passage marked is translated (from the original German) by William Ewald as follows: "This proof is remarkable not only because of its great simplicity, but more importantly because the principle followed therein can be extended immediately to the general theorem that the powers of well-defined manifolds have no maximum, or, what is the same thing, that for any given manifold *L* we can produce a manifold *M* whose power is greater than that of *L*" (*EFM*, 921-922).[30] So Cantor was the source of Peirce's copy of *EFM*. But he cannot possibly have sent the offprint to Peirce before 1901: we have drafts of letters from Peirce to Cantor dated December of 1900, from which it is clear that there had been no prior contact. Though Cantor's reply has been lost, Peirce's copy of *Zur Lehre vom Transfiniten* bears an inscription in Cantor's hand, dated 9 July 1901, and Grattan-Guinness (GRATTAN-GUINNESS 1971: 114) has published a letter from Cantor to P.E.B. Jourdain dated 15 July 1901 inquiring as to the identity of this C.S. Peirce who had written him some months before. So Peirce's copy of *EFM* did not come into his hands until five years after the discovery of his diagonal argument.

Of course Peirce could have seen another copy of *EFM*, either in German or in Italian, and cribbed his proof from that.[31] But if this was a deliberate theft, Peirce went on to exhibit a recklessness which quite defies belief. The general diagonal argument is preceded by a statement of the special case of the natural and real numbers:

> Let there be a denumerable collection, say the [finite] cardinal numbers; and let there be two houses. Let there be a collection of children, each of whom wishes to have those numbers placed some way into those houses, no two children

[29] In addition to *EFM*, there are (a) *Acta Mathematica* volume 2, number 1 (in CP MS 1599, Houghton Library)) and (b) the works bound in the volume mentioned in note 27. Item (a) contains French translations (CANTOR 1883j, 1883l, 1883i, 1883a, 1883e, 1883f, 1883g, 1883h, 1883b) of Cantor's major German language publications to date (CANTOR 1871, 1872, 1874, 1878, 1879, 1880, 1882, 1883c, 1883k), and one new paper in French (CANTOR 1883d). Item (b) contains the two parts of the *Beiträge* — Part I in Italian translation (CANTOR 1895c) and Part II in German (CANTOR 1895b) — and *Zur Lehre vom Transfiniten* (CANTOR 1890).

[30] In Vivanti's Italian: "Questa dimostrazione sembra notevole, non solo per la sua grande semplicità, ma anche perchè il principio in essa seguito può senz'altro estendersi al teorema generale, che le potenze degli aggregati ben definiti non hanno alcun massimo, o, ciò che è lo stesso, che per qualunque dato aggregato *L* si può determinare un altro *M* di potenza superiore" (CANTOR 1892: 2).

[31] In particular one cannot absolutely rule out the possibility that he saw the original German publication in the *Jahresbericht der Deutschen Mathematiker-Vereinigung*. There is clear manuscript evidence (PEIRCE 1903c) that he read a memoir of Schönflies's (SCHÖNFLIES 1900) in the *Jahresbericht* in 1903. This does not show that he knew of the *Jahresbericht* before 1896, however; and I have seen no positive evidence to indicate that he did.

> wishing for the same distribution, but every distribution being wished for by some child. Then, as Dr. George Cantor has proved, the collection of children is greater in multitude than the collection of numbers (*LR*, 547).

(Hartshorne and Weiss give a footnote to this passage which cites *EFM*, but the proper citation is of course the 1874 paper containing the narrower result.) But then in a footnote to the more general proof in *LR*, having just gone out of his way to acknowledge Cantor's priority in proving a closely related result, he says that "the above theorem is, as I believe, quite opposed to the opinion prevalent among students of Cantor" (*LR*, 549n1). This is a review, mind you, of a work by his disciple and correspondent Schröder, who was very likely to see the review,[32] and was moreover in an excellent position to spot any unattributed uses of Cantor's ideas. Schröder had written to Peirce in February of 1896 reporting that he was "working hard with proving several theorems of Prof. *Georg Cantor* on one to one correspondence and 'Gleichmächtigkeit von Mengen,' hitherto unproved" (SCHRÖDER 1890-1898: 13); it would have been a tremendous, indeed insane, risk to announce a plagiarized result in an essay so likely to pass under the eye of someone in such an excellent position to spot it. (Remarkably, there is to the best of my knowledge no evidence that Schröder *did* spot this overlap between Peirce and Cantor.) At this time Peirce still nursed hopes of obtaining an academic post (PEIRCE 1992b: 15-17, 30). A plagiarism scandal would have dashed those hopes for good: it is hardly credible that Peirce knowingly put himself in such serious danger of one.

The draft letters to Cantor contain further evidence against deliberate plagiarism. These contain proofs of the Step Lemma (PEIRCE 1900b: 768-769,777), which in one draft is grouped with "a few conclusions and points of view [about multitudes], which I do not find set forth in your papers" (PEIRCE 1900b: 767). If Peirce really was pretending to be the discoverer of a theorem he knew he had stolen, it would have been completely irrational for him to write to the true discoverer in this way.

As a general rule Peirce was very diligent about giving credit where he knew it to be due: as we have just seen, in *LR* he is careful to credit Cantor with discovering that there are more real than natural numbers. Perhaps the outstanding example of this diligence is the praise Peirce lavished on his student Mitchell for the latter's role in the discovery of the quantifier.[33] One would expect Cantor, faced with such a letter as Peirce apparently sent him, to have set the record straight; and to judge from what Peirce tells Ladd-Franklin, this is exactly what happened. For sure enough, after 1901 (when he heard back from Cantor) we find Peirce no longer claiming sole credit for the

[32] This is the strongest statement about Schröder's knowledge of *LR* that I am presently in a position to back up. We do know that Peirce sent Schröder a copy of "The Regenerated Logic" (PEIRCE 1896b), an earlier review in *The Monist*, because Schröder thanks him for it in a letter dated 2 March 1897 (SCHRÖDER 1890-1898: 17). I have been unable to find any evidence that he also sent him the later review, though it seems more likely than not that he did so. In any case, Schröder and Carus corresponded, and Schröder would have had an obvious interest in obtaining a copy and could readily have obtained one even if Peirce did not send him one.

[33] On Peirce's praise of Mitchell, and the extent to which it was warranted, see (DIPERT 1994).

Lemma: in a manuscript composed in 1908, intended for publication in his "Amazing Mazes" series in *The Monist*, he claims to have been "the first to prove that the multitude of ways of distributing the singulars of any collection under two heads is always greater than the multitude of those singulars themselves; although it was soon found that they coincided with certain multitudes less clearly and less accurately defined by Cantor" (PEIRCE 1908a:674). Peirce claims only to have been the *first* to prove the theorem, not the only one to do so. This is hardly a ringing acknowledgment of Cantor's discovery, and is in fact a denial of Cantor's priority, but it is not surprising that a bitterly disappointed old man, whose pride had already suffered so many grievous wounds, could go no further than this. Indeed, he seems to have gone out of his way to remain oblivious to the whole truth of the matter. It is obvious from what he tells Ladd-Franklin that he did not really read the offprint Cantor sent him: had he done so, he would have seen immediately that Cantor's ordinals played no role in the proof. Still more to the point, he would have recognized Cantor's priority in the really momentous discovery they both had made, namely, diagonalization. In the half-acknowledgment from 1908 Peirce takes refuge in the rather pathetic fiction that his own system of infinite numbers was a vast improvement over Cantor's, and that Cantor's achievements in that field should be viewed as a footnote to his own. This is all pretty threadbare, and there is more than a little self-deception here: Cantor clearly had priority, both of discovery and of publication, and I suspect that Peirce knew that, in his heart of hearts.[34] But self-deception is one thing, and plagiarism quite another; and Peirce seems to have been guilty, at worst, of the former.

I have been arguing against the suggestion that Peirce deliberately plagiarized his diagonal argument from Cantor. The chief arguments are: the fact that Peirce's surviving copy of *EFM* did not come into his hands until after 1900; the almost incredible recklessness that would have been required for Peirce to act as he did, if he had consciously plagiarized; and the inconsistency with plagiarism of his communications with Cantor, and of his (admittedly half-hearted) acknowledgment of Cantor's role. On top of this there are the internal differences, previously noted, between the two men's proofs. Any one of these pieces of evidence could easily be squared with the hypothesis of deliberate theft, but collectively they constitute an overwhelming case against it.

But perhaps Peirce appropriated Cantor's proof, not deliberately, but unwittingly. In that case he would have had to absorb the content of *EFM* thoroughly enough to translate its underlying idea, at some subsequent date, into the more Boolean language of his own formulations; and at the same time to absorb it so subliminally as to have no subsequent recollection of having gotten the idea there, and so as not to recognize the source when confronted with it some years later. Abstracting from the particulars of the case, one can readily imagine this sort of thing happening. Bookish people read a lot, so much that they often do not remember everything they read. It is, moreover quite possible (in my experience, anyhow) to bang one's head uncomprehendingly against a

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[34] Or perhaps Peirce sincerely believed that Cantor's infinities were "less clearly and accurately defined" than his own. Certainly his theory answers a lot of questions that Cantor's does not. But we now know that this is because Cantor dug more deeply than Peirce, and recognized deep difficulties that Peirce overlooked. Of course Peirce still saw much further into the subject than any ordinary reader could have done.

mathematical text and only a bit later to see what the author was getting at. Peirce himself evidently had this very experience in his reading of Listing.[35] But coming back now from the abstract to the particular case in hand, *EFM* is a highly unlikely candidate for this kind of scenario. Cantor's exposition is quite lucid, and does not involve any particularly exotic ideas. It is a very dull student indeed who is unable to grasp the diagonal proof that there are more real than natural numbers, and Peirce was anything but dull.

Furthermore, he was thinking hard about infinite multitudes, and in particular about the question of how many there are, in between 1891 (when *EFM* appeared) and 1896 (when he wrote *LR*). In 1892, in "The Law of Mind," he writes that "of infinite collections there are but two grades of magnitude" (PEIRCE 1892: 317). But just one year later, in the *Grand Logic* of 1893, he is not so sure, and at the same time recognizes that Cantor's work bears directly on the problem:

> from the point of view of the original definitions given above, the three classes of multitude [finite, countably infinite and uncountable] seem to form a closed system. Still, nothing in those definitions prevents there being many grades of multiplicity in the third class. I leave the question open, while inclining to the belief that there are such grades. Cantor's theory of manifolds appears to me to present certain difficulties but I think that they may be removed (PEIRCE 1893: 121).

Could the man who wrote these lines have looked at *EFM*, so casually that seeing Vivanti's translation would not jog his memory, and yet at the same time have read it closely enough that its contents would creep willy-nilly into his own work? This is a work, readily comprehensible to a person of Peirce's training, on a topic of great interest to him, by an author whose views on that topic he took very seriously. With this particular reader and this particular text, there just is not a psychologically plausible middle ground between the reader not seeing the text at all, and the reader comprehending and being vividly impressed by it. (Recall that the letter to Ladd-Franklin suggests that Peirce did not really read the offprint that Cantor sent him.) So unconscious imitation does not have much more going for it, as an explanation of Peirce's discovery, than deliberate theft.

If my positive account of Peirce's discovery is correct, then it is not quite accurate to call that discovery independent, and leave it at that. It is unlikely that he would have found the Step Lemma entirely on his own, without the very big boost he got from Cantor's brilliantly innovative analysis of cardinal exponentiation in *B1*, not to mention the proto-diagonalizations from Cantor's earlier papers. Still, if this is how things actually went, one can only marvel at Peirce's ability — at nearly sixty years of age, no less — to find his way to such a fundamental fact, from the barest of hints, in such a short time. What might the result have been, if the two had kept up a sustained and searching correspondence? As things are, the best we can do is to try to reconstruct the dialogue they might have had, and to pick it up where they would have left off.

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35 Murphey (MURPHEY 1961: 197) reports that Peirce's early notes on Listing contain such comments as "I don't understand this, so far, at all"; but "in 1904 [Peirce] wrote: 'the most important topical investigation that has ever yet been conducted . . . is that of Johann Benedict Listing.'"

Note on Peircean Citations

In citing works by Peirce, I have used the best readily available edition. When the cited version is not that listed in the relevant entry in the reference list, the former will be specified in a note in square brackets at the end of the entry. So for example, '[NEM3]' says that page numbers in citations come from the third volume of *New Elements of Mathematics* (PEIRCE 1976), and '[CP4]' says that citations are by paragraph number in volume 4 of the *Collected Papers* (PEIRCE 1931-1958). Other abbreviations used in the bracketed notes are 'EP1' for volume 1 of *The Essential Peirce* (PEIRCE 1992a) and 'RLT' for Putnam and Ketner's edition of the Cambridge Conferences Lectures (PEIRCE 1992b). The citation source is always the *first* listing in the bracketed note; where appropriate I will also include the manuscript number (e.g., 'Ms 28') from Robin's catalog (ROBIN 1967).

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