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C. S. Peirce's Philosophy of Infinite Sets

A study of Peirce's interest in the infinite related to the birth of American mathematics and contemporary work of Cantor and Dedekind.

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I fear I might seem to talk gibberish to you,
so different is your state of mental training and mine.[1]
Charles Sanders Peirce

American mathematics, like American science generally in the nineteenth century, remained underdeveloped, depended heavily upon European models, and made few independent and recognized contributions of its own. Though presidents like Jefferson might take a pedagogical interest in mathematics and its teachning, and while Garfield, in fact, discovered an interesting variation on the many proofs of the Pythagorean theorem, American mathematics generally remained without support, either institutional or financial, until late in the century.[2]

Despite the lack of incentives to pursue a mathematical career in America, there were nevertheless some who made important contributions to mathematics in the United States. One of the most interesting of these, Charles Sanders Peirce, made fundamental discoveries, largely independent of his European counterparts, in set theory and mathematical logic. This paper explores the nature and significance of Peirce's contributions.

Though his study of continuity led him to produce results paralleling in some ways the contributions of Georg Cantor and Richard Dedekind in Germany, Peirce's work was dramatically different in its origins, inspiration, and ultimate mathematical character. In order to understand the fate of Peirce and his mathematical studies of continuity and infinity, it is necessary first to say something about the status of American science in general, and of American mathematics in particular, in the nineteenth century. Following a brief sketch of the major developments of set theory, largely in the hands of Cantor and Dedekind, we shall then turn to consider Peirce, his mathematics, and finally the reasons why his genius and multitudinous insights did not exert more influence upon his contemporaries than they did.

Mathematics in America in the nineteenth century

Alexis de Tocqueville, in assessing the status of science in America in the early nineteenth century, remarked that Americans found it easier to borrow their science from Europe than to pursue it earnestly themselves. "I am convinced", he wrote, "that if the Americans had been alone in the world... they would not have been slow to discover that progress cannot long be made in the application of the sciences without cultivating the theory of them".[3] Mathematics, as the handmaiden in particular to astronomy and physics, was as essential, but just as neglected, as the other basic sciences in America until well past mid-century. In large measure de Tocqueville found the unusual combination of democracy and economic opportunities responsible for America's indifference to basic science. By this he meant that the egalitarian ideal encouraged the idea that anyone, with hard work,

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could transform the nation's national resources into personal fortune. Thus if anything was sought in science, it was only the immediate means by which nature might be exploited. Europe had its monarchies and aristocracies to encourage pure science, but de Tocqueville was certain that it could flourish equally as well in the United States, if only constituted authorities here would give it encouragement and support.

Thus while utility was highly praised for reasons that were religious, political and entrepreneurial, little interest was paid to abstract study which seemed to offer no evidence of immediate usefulness.[4] Mathematics was no exception. In fact, most of America's mathematicians until the end of the century were individuals of means, and the case of Josiah Willard Gibbs is illustrative. Gibbs taught at Yale University for many years without pay. In a country where "success" was more often than not associated with financial prosperity, it is no wonder that scientists in America played virtually no role in government or public affairs, unlike their European counterparts. Even as late as 1902, the mathematician C. J. Keyser of Columbia University, could write[5] that:

> I know personally of six young men, five of whom have relinquished the pursuit of science and the (sixth) of whom told me only yesterday that he seriously contemplated doing so, all of them, for the reason that, as they allege, the university career furnishes either not at all, or too tardily, a financial competence and consequent relief from practical condemnation to celibacy.

As for the American government, it consistently refused to support any national organization for science until after the civil war, and it was only through the bequest of an Englishman that the Smithsonian Institution was finally established.[6] Often, in fact, Americans were better known abroad than at home. Nothing reflects so poorly upon the state of American science even towards the end of the century than a story J. J. Thomson[7] once told:

> When a great university was founded in 1887, the newly elected President came over to Europe to find professors. He came to Cambridge and asked me if I could tell him of anyone who would make a good professor of molecular physics. I said, "You need not come to Europe for that; the best man you could get is an American, Willard Gibbs". "Oh", he said, "you mean Wolcott Gibbs", mentioning a prominent chemist. "No, I don't", I said, "I mean Willard Gibbs", and told him something of Gibbs' work. He sat thinking for a minute or two and then said, "I'd like you to give me another name. Willard Gibbs can't be a man of much personal magnetism or I should have heard of him".

Mathematicians in America could blame their lack of status on apathy and indifference. One mathematician assessing mathematical productivity in the United States echoed de Tocqueville's words when he noted that "educational and scientific activity shall come to be generally understood, and especially in proportion as we learn to value the things of mind, not merely for their utility, but for their spiritual worth".[8] The assessment was C. J. Keyser's, and in part he blamed the low level of productivity by American mathematicians before the turn of the century upon their isolation, saying that "in general there was no suspicion that, on the other side of the Atlantic, mathematics was a vast and growing science".[9]

Throughout the nineteenth century, mathematics, like the sciences generally, became more complex, more technical, more specialized. More formal training was required, more professionalization, and at first Americans clearly followed the pattern de Tocqueville had described in 1835. If Americans wanted to learn the newest techniques, to study the latest theories, they went to Europe, many to Germany, and to such centers for mathematics as Berlin and Göttingen. In rare instances, Europeans came to America. Perhaps no one was more influential for the future of American mathematics in this capacity than was J. J. Sylvester, whose arrival in America accompanied another significant development.[10] As a counterpart to the growing specialization of European science, advanced training at the graduate level was regarded increasingly as imperative. Consequently, following the examples of the great European schools like the *École Polytechnique* and the University of Berlin, America's first graduate school was founded in Baltimore.

In 1876 Johns Hopkins University opened, and one of its first great attractions was the English mathematician J. J. Sylvester. Both he and Johns Hopkins exerted a tremendous influence upon.

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American mathematics, in part through the *American Journal of Mathematics*, edited by Sylvester and begun at Johns Hopkins in 1878. It was perhaps symptomatic that not only was the journal originally edited by an Englishman, but that to a great extent the journal's articles were written by foreigners. Since Johns Hopkins was primarily a graduate school, it served to encourage graduate studies at other U.S. universities. Two similar institutions were founded by the end of the century: Clark University in Worcester, Massachusetts, in 1889; and the University of Chicago in 1892, which was extremely influential in the Midwest.

But the single strongest factor in organizing and promoting mathematical research in America was the American Mathematical Society.[11] Its precursor, The New York Mathematical Club, had been founded in 1888, and at first was little more than a small group meeting at Columbia University. But soon the Mathematical Club became the New York Mathematical Society, publishing a monthly bulletin. In 1894 this organization was again transformed, becoming the American Mathematical Society with membership then at nearly four hundred. Bi-monthly meetings were held, summer meetings were scheduled around the country, and soon sections scattered from coast to coast were established. The first was the Chicago section, chaired by E. H. Moore on April 24, 1897; five years later, in 1902, the San Francisco Section was chaired by Irving Stringham; and shortly thereafter, in 1906, the Southwestern Section was established in St. Louis by E. R. Hedrick. In terms of its societies, journals, and publishing mathematicians, mathematics in America had come a long way from its status early in the century, when arithmetic was still taught in the first year at Harvard College, and only became an entrance requirement in 1816. By the end of the nineteenth century, in many respects if not completely, the words of the French mathematician Laisant were in large measure true. As he surveyed the progress Americans had made in mathematics, he concluded[12] that:

> Mathematics in all its forms and in all its parts is taught in numerous universities, treated in a multitude of publications, and cultivated by scholars who are in no respect inferior to their fellow mathematicians of Europe. It is no longer an object of import from the old world, but it has become an essential article of national production, and this production ihcreases each day both in importance and in quantity.

Before turning to assess the character and significance of the contribution made by C. S. Peirce to American mathematics, in particular to various aspects of set theory, it is necessary to survey briefly the state of that art in Europe at the time. Above all, until about 1895 at least, the development of transfinite set theory was almost exclusively the work of the German mathematician Georg Cantor.

Georg Cantor (1845–1918)

Georg Cantor, creator of transfinite set theory, published a theorem in 1872 which brought him to the attention of the mathematical world, and which also marked the beginning of his work in set theory. His theorem established the uniqueness of functional representations by trigonometric series over domains from which certain infinite sets of points could be excepted.[13] The only restriction limited the set of exceptional points to first species sets P, ones for which the nth derived set P^n was empty for some finite value of n. But in order to provide a satisfactory foundation for his proof, Cantor discovered that he was obliged to introduce a rigorous construction of the real numbers. He did so in terms of equivalence classes of infinite sequences of rational numbers subject to the Cauchy criterion for convergence, and was also led to formulate his Axiom of Continuity, which postulated the equivalence of the arithmetic and geometric continuums. In the same year, 1872, Richard Dedekind (1831–1916) published his construction of the real numbers in terms of his famous "cuts", and he did not fail to acknowledge the similar work Cantor had done in his paper on trigonometric series.[14] Cantor's unexpected results seem to have spurred his interest in the properties of continuous domains in general, and late in 1873 he discovered that the set of all real numbers was non-denumerable.[15] In 1879 he finally managed to publish a startling proof showing that any continuous space of n-dimensions could be mapped (though not continuously) in a one-to-one fashion onto the real line. Cantor was so unprepared for this discovery that it prompted one of his most oft-quoted remarks: "I see it, but I don't believe it".[16]

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Cantor's first systematic presentation of his transfinite numbers was published in 1883. His *Grundlagen einer allgemeinen Mannichfaltigkeitslehre* was as much philosophy as it was mathematics,[17] a combination that was also to be characteristic of much of C. S. Peirce's research. In the *Grundlagen* Cantor introduced his transfinite ordinal numbers. He began by identifying two principles of generation. The first produced new ordinals by the successive addition of units, hence $1, 2, 3, \ldots, n, n+1, \ldots$. The second principle was called upon to introduce a new number representing the totality of all ordinals generated by the first principle when such a sequence continued without coming to an end. For example, though it was not permissible to think of a last of all natural integers, one could posit a least number coming after *all* the natural numbers. This number Cantor defined as ω, and it represented the totality of all the positive integers in their natural order. It was then possible to apply the first principle of generation to produce higher transfinite ordinals: $\omega + 1, \omega + 2, \ldots, \omega + n, \ldots$. When this sequence continued without end, Cantor again called upon his second principle of generation to produce the least number following all those of the form $\omega + n$, namely the ordinal number 2ω, and so on. Later Cantor would define the second number class of such transfinite ordinals as the class $Z(\aleph_0)$, the totality of all order types α of well-ordered sets of cardinality $\aleph_0$. The power of this second number class $Z(\aleph_0)$ Cantor denoted by the second transfinite cardinal number $\aleph_1$.[18]

Cantor, of course, was not the only mathematician interested in the properties of infinite sets. In 1888, Richard Dedekind published a small pamphlet, *Was sind und was sollen die Zahlen*,[19] in which he introduced, among other things, the distinction between finite and infinite collections that has since become a classic:

> A system S is said to be *infinite* when it is similar to a proper part of itself, in the contrary case S is said to be a *finite* system.

This definition is of particular interest for it seems that Peirce had been led to an equivalent distinction, even earlier, and for very different reasons.

In 1891 Cantor published his famous method of diagonalization, whereby it was possible to generate an unending sequence of sets of greater and greater cardinality.[20] In 1874 Cantor had shown only that the set of real numbers was nondenumerable. The result of 1891 was considerably more powerful, and impressively general, for he was able to show that for any exponent $\aleph$, the power $2^{\aleph} > \aleph$. If $\aleph_0$ be taken as the cardinality of the denumerable set of natural numbers, then $2^{\aleph_0}$ was a set of greater cardinality representing the set of all real numbers. Moreover, Cantor could show that there were sets of cardinality even greater than the real numbers, for example the set of all single-valued functions on the interval $(0, 1)$.

Between 1895 and 1897 Cantor's most ambitious and influential work appeared in the *Mathematische Annalen*: his "Beiträge zur Begrundung einer transfiniten Mengenlehre".[21] In Part I (1895) he presented his general theory of the order types of simply-ordered sets like the rationals taken in their natural order (type η), and the reals (type θ); in Part II (1897) he defined his transfinite cardinal numbers in terms of well-ordered sets, and explored in detail basic arithmetic properties of his transfinite numbers. He also took the opportunity to condemn the doctrine of infinitesimals in the recently published work of the Italian mathematician Giuseppe Veronese. Cantor had always been an ardent opponent of infinitesimals, and at one point called them the "infinitary cholera bacillus of mathematics".[22] Early in his career Cantor rejected the idea of infinitesimals, and when Mittag-Leffler asked if there might not be other kinds of numbers between the rational and real numbers, Cantor responded with an emphatic "no".[23] In 1887 Cantor published a proof of their logical impossibility, based not surprisingly upon the Archimedean character of what he called linear numbers, and some years later Peano published a similar proof against infinitesimals in his own journal, the *Rivista di matematica*.[24] To have admitted infinitesimals, it might be added, would have complicated greatly Cantor's continuum hypothesis, which asserted that the cardinality of the set of all real numbers, $2^{\aleph_0}$, was equal to that of his second number class, in other words, $2^{\aleph_0} = \aleph_1$. To allow infinitesimals in addition to the rationals and irrationals would have made this conjecture concerning the power of the continuum considerably more complicated.

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By the end of the century, the status of Cantor's work was brought dramatically into question by discovery of the paradoxes of set theory. While Burali-Forti was the first to publish his paradox of the largest ordinal number, Cantor had discovered the paradoxes of both a largest ordinal and cardinal number even earlier, probably as early as 1895. Cantor sketched a proof for Dedekind in 1899, in which he concluded that it was a direct consequence of the paradoxical nature of the unending sequence of *all* cardinals that the continuum must be a set whose cardinality was one of Cantor's transfinite alephs.[25]

But in 1897 Burali-Forti drew a very different conclusion from his study of the collection of all ordinal numbers. Such a collection, he argued, must have an ordinal number δ greater than any ordinal in the collection. But if the set contained all ordinal numbers, then it must contain δ, and Burali-Forti was forced to the contradictory conclusion that $\delta > \delta$. From this he did not draw comfort, as did Cantor, in alleging that this was the key to solving much deeper problems of set theory. Instead, Burali-Forti concluded that mathematicians could only agree to abandon any hope of strict comparability between transfinite numbers.[26] In 1902 Bertrand Russell constructed a strictly logical paradox and shocked Frege by showing that there were certain antinomies inherently part of logic, and consequently of mathematics as a form of structured reason.[27]

In considering the paradoxes of set theory, in particular those of the greatest ordinal and cardinal numbers, Peirce agreed with Bertrand Russell that these were, properly speaking, questions of logic. The basic business of mathematics, for Peirce, was the formation of hypotheses.[28] In terms of set theory, this meant the determination of what grades of multitude between infinite collections were mathematically possible. And, as we shall see, Peirce drew from the paradox of the largest cardinal number a principle which he felt might help to resolve the question of the *true* nature of continuity.

Charles Sanders Peirce (1839–1914)

C. S. Peirce, the son of Benjamin Peirce, was born in Cambridge, Massachusetts, on September 10, 1839.[29] Benjamin Peirce, a professor of mathematics and natural philosophy at Harvard University, was careful to direct his son's schooling, and saw that young Charles had as rigorous a scientific education as he and the private schools of Boston could provide. When Peirce graduated from Harvard with an Sc. B. in chemistry, in 1863, he did so *summa cum laude*. But Peirce was not to go on immediately to devote himself to the study of pure science. His interests ran more to the study of method and logic, and in hopes of gaining more experience in the nature and method of scientific investigation, he joined the U.S. Coast Survey. For more than thirty years Peirce remained with the Survey, and in addition to working on the nautical almanac, he conducted numerous pendulum experiments, was a special assistant in gravity research, and devoted a good deal of time to the observation of solar eclipses.

As for teaching, Johns Hopkins University made it possible for Peirce to lecture in logic from 1879 to 1884, and some of his earliest work of relevance to set theoretic problems dates from this period. In fact, in 1881, Peirce published a paper in the *American Journal of Mathematics*, "On the Logic of Number", in which (he was later always proud to emphasize) he had characterized the difference between finite and infinite sets well before Dedekind had done so in 1888.[30] Peirce asserted that Dedekind's *Was sind und was sollen die Zahlen* was doubtless influenced by his own paper, because Peirce had sent Dedekind a copy.[31] But the most interesting feature of Peirce's entire approach to mathematics was not the way in which it was like the research then being conducted in Europe, but in the ways it was unlike the approaches taken by Georg Cantor and Richard Dedekind to the problems of continuity and infinity.

Cantor, as we have seen, was motivated to study the continuum of real numbers as a result of his early study of the representation theorem for trigonometric series. Similarly, Dedekind's characterization of the continuum and his introduction of the now famous "Dedekind cut" to define the real numbers was also inspired by analysis. In trying to teach the basic elements of the differential calculus, particularly theorems involving limits, Dedekind realized that geometric intuition, though a guide, was not rigorously satisfactory. And so he turned to produce a purely arithmetic study of continuity and the irrational numbers.[32]

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Peirce, on the contrary, took an entirely different approach. His inspiration was not analysis, and his interests were not in probing the foundations of mathematics in order to provide a certain, unshakable beginning from which function theory could proceed without difficulty. Instead, Peirce was led to study the mathematics of infinity, infinitesimals, and continuity as a result of his interests in logic and philosophy. In this difference lies the key to understanding why Peirce differed so markedly from Cantor and Dedekind in his approach to the problems of continuity and the infinite.

Peirce's first publication describing the difference between finite and infinite classes appeared in 1881, while he was lecturing in logic at Johns Hopkins. His paper[33] began with a definition (though insufficient) of continuity:

> "A continuous system is one in which every quantity greater than another is also greater than some intermediate quantity greater than that other".

But since the rationals would be continuous under this definition, Peirce's description is inadequate, although it does represent a necessary feature of any continuum. Peirce, however, was only beginning his study of continuity at the time; the most interesting feature of his paper appeared towards the end, where he offered a distinction between finite and infinite collections. He announced that a set was finite if no one-to-one correspondence could be found between the set and any proper subset. Peirce's favorite example[34] was a syllogism which appeared in numerous equivalent forms throughout his mathematical and logical writings:

> Every Hottentot kills a Hottentot,
> No Hottentot is killed by more than one Hottentot,
> Hence, every Hottentot is killed by a Hottentot.

The syllogism is true only if the set of Hottentots is finite. The form of the syllogism, Peirce noted, was due to De Morgan, who called it the *syllogism of transposed quantity*.[35] Thus Peirce's interest in the infinite was inspired by studies in logic and the consequences one might draw from the syllogism of transposed quantity.

In keeping with his interests, and in pushing his study of quantity, both finite and infinite, as far as he could, Peirce decided that a perfectly logical definition of continuity was needed, and in 1897, when he published "The Logic of Relatives" in *The Monist*, he wrote[36] that:

> A perfectly satisfactory logical account of the conception of continuity is required. This involves the definition of a certain kind of infinity, and in order to make that quite clear, it is requisite to begin by developing the logical doctrine of infinite multitude. This doctrine still remains, after the works of Cantor, Dedekind, and others, in an inchoate condition. For example, such a question remains unanswered as the following: is it, or is it not, logically possible for two collections to be so multitudinous that neither can be put into a one-to-one correspondence with a part or the whole of the other? To resolve this problem demands, not a mere application of logic, but a further development of the conception of logical possibility.

But what did Peirce mean by the need to define a certain kind of infinity before the concept of continuity could be accounted for logically? What was "inchoate" about the work of Cantor and Dedekind? Why was the comparability of cardinals, in Peirce's view, impossible to establish without developing further the concept of logical possibility? What, in fact, did Peirce mean by logical possibility? The answers to all these questions hinge on Peirce's view of the infinite, and upon a very important discovery, one he apparently made independently of Georg Cantor, and one for which Peirce was always, and justifiably so, very proud.

Peirce proved (though exactly when he did so for the first time is unclear), that the power of the set of all subsets of a given set is always greater than the power of the original set itself. In other words, for any exponent $\aleph$, $2^{\aleph} > \aleph$, a result, as Peirce put it, of "prime importance".[37] Beginning from the smallest infinite set, the set of all integers, he concluded that it was always possible to produce increasingly larger sets of greater and greater power. Peirce designated the collection of all integers "denumerable". The set of all real numbers comprised what he called the "first abnumeral" or the "primipostnumeral" multitude. The set of all subsets of the real numbers produced the "second

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abnumeral", or the "secundo-postnumeral" multitude, and so on. Since one could always form from such sets the set of all subsets, Peirce noted that there could be no maximal multitude. But Peirce also commented that as for the second abnumeral, mathematics could offer no example of such a multitude, and added that in fact mathematics had no occasion to consider multitudes as great as this,[38] a comment that is somewhat puzzling, as we shall see, particularly in light of his construction of infinitesimals and his assertion that continua were greater in power than *any* postnumeral multitude.

In a letter to his friend E. H. Moore, Peirce commented[39] upon the significance of his discovery that there was no maximum multitude:

> "Here we have a hint about continuity . . . . The continuum is a General. It is a General of relation. Every General is a continuum vaguely defined."

By a "General" Peirce seems to have meant that which was neither discrete nor definite, "General" as opposed to particular or to something completely specified. But what did Peirce see in all this to help solve the mystery of continuity?

Peirce argued that if a continuum did not contain all the points that it possibly could, then there would be gaps or discontinuities present.[40] Thus it was a problem of the utmost importance to determine what the maximum possible multitude *was* in order to determine the power of continua. But Peirce had already shown that there could be *no* such greatest possible multitude, that the process of forming power sets was unending, and that consequently it was a process that remained potential, indefinite. Similarly, if the continuum were to contain the maximum possible multitude of points, it had to be correspondingly potential, indefinite.

Since Peirce regarded the essence of multitudes to be their definiteness, thus making it possible to determine their powers or cardinalities, it was reasonable for him to assert that since the collection of all abnumerals was unending, potentially infinite, thus entirely indefinite, it could not properly be called a set. Or, in Peirce's terminology, it could not be called a multitude. Likewise, the elements constituting a continuum could not be regarded as comprising a set or multitude of objects. Thus the complete determination of continua was impossible, for Peirce regarded the concept as intrinsically potential, essentially general.[41]

Consequently, Peirce was led to reject Cantor's view that the geometric continuum was somehow made up of a multitude of points. Peirce realized that there were two features of such continua that had to be considered: one involved quantity, the other involved order. Cantor had published his analysis of what he called the simply-ordered type θ of the continuum of real numbers in 1895.[42] Peirce, however, disagreed on two counts. The collection of points comprising any continuum must be infinitely larger than any Cantor contemplated because, Peirce claimed, the real numbers R as defined by Cantor were grossly insufficient to account for the geometric continuum.[43] This was so, he argued, because of his discovery that the set of abnumerals was unending. Since the line must contain all points possible, and since the set R corresponded to the multitude represented by $2^{\aleph_0}$, it could not possibly, as a completed multitude, account for the nature of continuity. As for the question of the order of elements constituting a continuum, Peirce suggested that something like the proper idea was approximated if between every pair of rational numbers one inserted a sequence of irrational numbers. Between any two irrationals of this sequence one could pack yet another such sequence, and so on, without end. Thus Peirce thought it was possible that between any two points of the continuum, however close, one could always pack sets of points of higher and higher multitude. The continuum would eventually be "cemented together", and not by virtue of discrete points.[44]

Peirce illustrated his case by imagining a series of photographs.[45] No matter how close the intervals, no motion will appear in any of them. But our perception of motion in time shows that time must be more than a succession of instants. More than single, isolated point "instants" must be present to our consciousness. What is this something more? Peirce claimed that (1) in a sensible time there exists room for *any* multitude of distinct instants. (2) The instants are so close together they merge and cannot be distinct. And this view of time and its continuity greatly influenced Peirce's view of the continuum and the logical status of continuity. Just as parts of time merged to lose their identity, so too the points of a line. If continuity consisted of nothing but a special type of serial order, he

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argued that two continuous lines thought to interesect might actually "slip through" each other, this presumably being possible wherever spaces between one object and its "next" in the serial ordering on each line might occur.[46] This certainly suggests that Peirce had a very different way of thinking about continuity and serial order than did Cantor.

Peirce, as he put it himself, took the very word *continuity* to mean that the instants of time or the points of a line were everywhere "welded together".[47] As evidence of this, he seems to have been content to argue that its justification was nothing more than common sense. Peirce concluded that the instants of time did not constitute a multitude (or set, whose elements had to be distinct and definite). In fact, the collection of instants in any continuum of time had to be *more* than any multitude.

To illustrate his idea of continuity, Peirce outlined a procedure which he called interpolation on the unit interval, and which involved decimal representations given with only the digits 0 and 1. At step (I), there was only one interpolation, at step (II) there were 2, at step (III), there were four, at step (IV) there were 8, and at step (N) there would be 2^{N-1}. Schematically:

	0.000...							1.000...
STEP (I)				.100....				
(II)		.010...				.110...		
(III)	.001...		.011...		.101...		.111...	

Here Peirce found, as he put it, a "premonition of continuity".[48] In carrying out this procedure, a nondenumerably infinite number of interpolations would be made. Somehow this process was to help Peirce explain how the continuum was able, in his words, to "stick together".[49] No finite collection could ever "stick together" in any order, nor could any denumerably infinite collection if considered in any well-ordered form. The difficulty in describing the continuity of the real line, Peirce believed, reduced to the fact that numbers *per se* could never account for continuity.[50] Numbers expressed nothing but the order, he believed, of discrete objects. Nothing discrete could possibly be multitudinous enough to account for the continuum.

For example, Peirce noted that in supposing the countable collection of the set of rational points to be completely present on the line, one was in effect supposing also the collection of irrational points in the sense that the irrationals could be considered as interpolated between the rationals.[51] Hence the denumerable set of rationals carried with it the existence of a nondenumerable collection of points, namely the first abnumeral multitude of irrational numbers. In exactly the same way, said Peirce, the system of irrational points on a line led to a secundo-postnumeral collection of points interpolated between the irrationals. This secundo-postnumeral collection for Peirce involved his infinitesimals, and it is possible to understand the role they played more clearly by returning for a moment to his diagram of decimal interpolations.

In a letter to M.F.C.S. Schiller of 1906, Peirce explained that by a Leibnizian infinitesimal of the first order he meant an assumed quantity smaller than any finite positive quantity.[52] It was the *first* quantity after the sequence .1, .01, .001, ... Peirce believed it was impossible to prove that there was *no* such quantity. In fact, he believed his infinitesimals were given an imprimatur of sorts by nature, since he took their existence to be necessary for physics.[53] To support this claim of the physical reality of infinitesimals, Peirce referred to the experience of memory. The perception of the flow of time must extend, he said, beyond a single instant. Yet Peirce could not see how such phenomena could be satisfactorily explained unless time were believed to be strictly infinitesimal. Moreover, there were reasons from physics which also established the necessary existence of infinitesimal magnitudes. In a letter written in 1908 to P. E. B. Jourdain,[54] Peirce struck up a theme which Cantor had sketched, but never developed, in a short article of 1885. Cantor had conjectured that in order to explain satisfactorily the phenomena of nature, one had to suppose two sorts of monads, material and aetherial. He conjectured that the power of the set of all material monads was denumerable, of cardinality $\aleph_0$, while the set of aetherial monads was equipotent with the second cardinal, $\aleph_1$. Peirce applied this idea in order to explain how matter could act on the brain to produce thought. Peirce posited two fluids, one vortex of matter-monads, and an infinitely subtler vortex of soul-monads, where the diameters of the soul-monads were to be taken as infinitesimals of infinite order, which

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Peirce felt was quite appropriate for the character of soul-monads. This theory was given the name of the "introvortical theory".[55] It all helped to convince Peirce that with physical counterparts for his logical infinitesimals, there was pragmatic justification for arguing their validity.

Peirce's most important reason for insisting that his infinitesimals were acceptable involved their self-consistency. Logically, being non-contradictory, there was no reason not to admit them into mathematics. Though Peirce did not undertake a careful arithmetic investigation of the properties of his infinitesimals, nor did he undertake any investigation of non-archimedean systems in general, he was a proto-proponent of nonstandard analysis in believing that there was perhaps more to understanding the nature of continuity than the rationals and irrationals together could manage to explain.

To make Peirce's point as clear as possible, it may be helpful, in closing, to sketch his reasons for arguing that the rationals and irrationals together were insufficient to account for the nature of continuity. While Cantor and Dedekind regarded the irrational numbers as completing the rationals, and thus conferring completeness on the real numbers, Peirce saw the relation between rationals and irrationals somewhat differently. He concluded that there was a kind of "nextness" in the reals which actually constituted a breach of continuity.[56] Suppose, he argued, that we do have a clear idea of a sequence of real numbers. If we have a clear idea of their order, it can be assumed that any set of objects sufficiently multitudinous can be similarly ordered. Assume each of these objects to be replaced with a sequence of points, similar in order to the real numbers on the open interval (0, 1). But there is no reason to stop here, and Peirce went on to replace each point of such series by yet other series, and so on, without end. In his own words:[57]

> The result is, that we have altogether eliminated points. We have a series of series of series, *ad infinitum*. Every part, however closely designated, is still a series and divisible into further series. There are no points in such a line. There is no exact boundary between any parts.

It would be easier to interpret the significance Peirce attached to his infinitesimals for mathematics had he ever commented explicitly upon Cantor's and Dedekind's axioms of continuity, which hypothesized the equivalence of the standard Archimedean arithmetic continuum and the geometric continuum. But all we have is Peirce's allegation that the real numbers were insufficient to account for the continuity of space and time. As for analysis, however, it never had need to consider multitudes greater in magnitude than that of the first abnumeral, which meant that the set of real numbers was enough for the interests of analysis and presumably for all of mathematics.[58]

Peirce wanted to explore the logical boundaries of the possible in terms of the infinitely large and the infinitely small, and found no logical contradictions or constraints in either conception. Since his interests were primarily philosophical, it is perhaps easier to understand why he never submitted his ideas to a more searching mathematical analysis than he did.

Ultimately Peirce's infinitesimals remained vague rather than rigorously defined mathematical entities. He never suggested how they might be useful in analysis. But then Peirce's interests had never been inclined towards analysis. From the very beginning he had been inspired by the purely logical implications of the syllogism of transposed quantity, and the logic of relations. Thus, unlike Cantor, he was not concerned to develop the arithmetic properties of his ideas, since their existence as numbers was not for him of great consequence. He was interested in illuminating a deep philosophical problem of long standing, namely that of the continuum, and he felt that conceptually he had found an approach to the subject that was the most satisfying of all.

Finally, how can we now draw all of these ideas together in order to interpret Peirce's claim that "the continuum is a General", meaning that it could not be defined as a set in Cantor's sense of a collection of distinct elements, and Peirce's statement that "Infinity is nothing but a peculiar twist given to generality"?[59] Peirce took generality to involve the potential infinite, in that anything not general, anything specific, was completed in some sense or other. He also took infinity to be a potentiality, something never completed, and thus it too reposed upon generality. Reasoning about such matters, he noted with perhaps too little emphasis, was always exceedingly puzzling. Almost all

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metaphysicians, and even mathematicians, he added, had fallen into pitfalls concerning the infinite where the "ground was spongy".[60] Trouble inevitably stemmed from quantifiers like "all and every", but if one were always certain to determine *how* objects in question were to be selected, he believed that erroneous reasoning could easily be avoided. The trouble with the collection of all abnumerable multitudes was simply that it could never be considered as completed. It was self-generating, without end. Such collections were so great that they were no longer discrete, and not being complete, no definite determination of their magnitude could be given. They were potential and as such, general. In the same way the line was general, indeterminate, since between any of its points, Peirce imagined that more could be packed representing supermultitudinous collections. To quote Peirce directly[61]:

> Such supermultitudinous collections stick together by logical necessity. Its constituent individuals are no longer distinct, or independent. They are not subjects but phases expressive of the properties of the continuum.

Peirce offered a graphic illustration. Suppose a collection of blades were to cut the line. So long as they did not comprise a supermultitudinous collection, the line would be cut up into bits, each of which was still a line. Peirce therefore urged mathematicians to discard all analytical theories about lines, and recommended that they begin from his view, a synthetic point of view.[62] By a simple mental experiment, he believed he had shown that the line refused to be cut up into points. But even if mathematicians refused to accept his arguments, Peirce insisted that only one thing mattered: his idea of continuity and of infinitesimals involved no contradiction. In closing, he warned[63]:

> I am careful not to call supermultitudinous collections multitudes. Multitudes imply an independence of the individuals of one another which is not found in supermultitudinous sets. Here the elements are cemented together, they become indistinct.

Here Peirce had reached the potentiality, the generality that he had earlier said was essential if one were to understand properly the structure of continuity. For Peirce, the essence of continuity depended upon the supermultitudinousness of the elements of the line, and their "intrinsic arrangement which is inseparable from the particular grade of multitude in which those phenomena of cohesion are found".[64]

It is now possible to see what Peirce meant when he wrote to Paul Carus, editor of *The Monist*, to say that at last he had seen where Cantor had gone wrong.[65] Continuity could not come from any collection of points because points were discrete, determined, and if anything, points represented *discontinuities* when removed from the line. In summarizing his view for Judge Francis Russell in 1909, Peirce interpreted the essence of continuity in terms of the potential and completely general nature of the ideas involved. "As to a straight line not having any definition proper, it is demonstrable that it cannot be, properly speaking, defined".[66]

If we may take "properly speaking" to mean "mathematically speaking", then there was not much Peirce could hope to offer in such a view to his mathematical colleagues. But it is also true that he did not feel mathematics needed to go so far, apparently, in the analysis of the continuum as he had gone. Peirce was interested in pushing the logical consequences of his ideas to their ultimate conclusions for the sake of philosophy, but analysis, he seemed to say, could stop at something less.

For Peirce, such ideas justified themselves as a matter of instinct, of common sense.[67] He had always held that his intuitive understanding of the continuum, or the continuity of space and time, were the ultimate guides in his analysis of continuity. Nothing could have been further from the aims of Weierstrassian analysis, which sought to reject all such intuitions and aimed to base mathematics upon more certain foundations of arithmetic. Weierstrass had constructed examples of everywhere continuous, nowhere differentiable functions to show the inadequacy of intuition. But Peirce was not convinced, and once even commented that Weierstrassian mathematics, in showing a distrust of intuition, betrayed an ignorance of fundamental principles of logic.[68] Peirce followed his intuitions as far as they would carry him, and it may have been this feature of his thinking, as much as his interest in philosophical and metaphysical arguments, that prevented his being more readily accepted by those of his contemporaries even aware of his work.

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Conclusion

Peirce once said that it was the business of science and mathematics to guess. For mathematics this reduced to the fabrication of hypotheses to be tested for logical self-consistency.[69] If no contradictions could be deduced, then the hypothesis, or the mathematical theory in question, stood as acceptable. This was the basis upon which Peirce argued most persuasively for the respectability of his ideas concerning infinitesimals and continuity. Kepler, to Peirce's mind, was the greatest guesser the history of science had ever seen. But in terms of American mathematics at the turn of the century, Peirce may easily have been an equally impressive guesser, producing the mathematical hypothesis of infinitesimals.

Kepler lacked sufficient mathematical techniques and a theory of gravitation to establish a convincing explanation of his laws, deficiencies Isaac Newton would later remedy. In much the same way, Peirce lacked sufficient techniques to produce a rigorous theory of non-Archimedean systems. But his hypotheses were eventually vindicated, in the twentieth century, by mathematicians like Schmieden, Laugwitz, Robinson, and Luxemburg.[70] Perhaps until the middle of this century, only a mathematician as interested in philosophy as was Peirce, and as isolated from the prevailing assumptions of established mathematics of the nineteenth century, could have pursued the problem of continuity in the way he did. Out of touch with much of European mathematics, Peirce considered infinitesimals with an unprejudiced eye to affirm not only their existence, but to argue as well that the arithmetic continuum of real numbers was only a very incomplete picture of the actual richness of any continuum. Working in obscurity, penury, and isolation, he nevertheless saw, if only a glimmer, what later generations might be more willing to accept. In light of current research in non-standard analysis, it is now possible to consider, more rigorously than did Peirce, alternatives to the traditional nineteenth-century view of the standard Archimedean continuum.

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Footnotes

The single major reference cited below is C. Eisele, ed.: *The New Elements of Mathematics by Charles S. Peirce* (The Hague: Mouton, 1976); this reference is cited as Charles S. Peirce: *Mathematical Papers.*

[1]Peirce: *Mathematical Papers*, volume 3, 109.

[2]No recent study of mathematics in the United States has appeared. For two studies of an introductory sort, see G. A. Miller: *Historical Introduction to Mathematical Literature* (Macmillan, New York, 1921), and D. E. Smith and J. Ginsburg: *A History of Mathematics in America before 1900* (The Mathematical Association of America, New York, 1934).

[3]Alexis de Tocqueville: *Democracy in America*, trans. H. Reeve (D. Appleton, New York, 1904), volume 2, 518.

[4]R. H. Shryock: "American Indifference to Basic Science during the Nineteenth Century", *Archives internationales d'histoire des sciences*, 28 (1948–1949) 3–18.

[5]C. J. Keyser: "Mathematical Productivity in the United States", *Educational Review*, 24 (November, 1902) 356.

[6]For recent studies of Smithson and the history of the Smithsonian Institution, consult L. Carmichael: *James Smithson and the Smithsonian Story* (Putman, New York, 1965), and W. Karp: *The Smithsonian Institution* (Smithsonian Institution, Washington, 1965). The significance of the Civil War for the fortunes of American Science has been studied by a number of historians, including I. B. Cohen: "Science and the Civil War", *Technology Review*, 48 (January, 1946), number 3; D. J. Struik: *Yankee Science in the Making* (Little, Brown, Boston, 1948), in particular Chapter XII in which Struik discusses the Civil War and the Morrill Act, pages 355–357; and A. H. Durpee: *Science in the Federal Government. A History of Policies and Activities to 1940* (Harvard University Press, Cambridge, 1957), especially Chapter IV, "The Fulfillment of Smithson's will", and Chapter VII, "The Civil War".

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[7]J. J. Thomson: *Recollections and Reflections* (Macmillan, New York, 1937) 185–186.

[8]Keyser (1902), 346. Miller also pointed directly to the major problem in America: "There is too much mathematical indifference", G. A. Miller: "American Mathematics", *Popular Science Monthly*, 79 (November, 1911), 461.

[9]Keyser (1902), 347.

[10]G. A. Miller (1911), 463, and Miller (1921), 30–32.

[11]Keyser (1902), 350–352; Miller (1921), 33–35.

[12]C. A. Laisant: *La mathématique, philosophie-enseignement* (G. Carré et C. Naud, Paris, 1898), translated by G. A. Miller (1911), 459.

[13]G. Cantor: "Über die Ausdehnung eines Satzes aus der Theorie der trigonometrischen Reihen", *Math. Ann.*, 5 (1872) 123–132.

[14]R. Dedekind: *Stetigkeit und irrationale Zahlen* (second edition, Vieweg, Braunschweig, 1892), translated by W. W. Beman: "Continuity and Irrational Numbers", *Essays on the Theory of Numbers*, (Dover, New York, 1963).

[15]G. Cantor: "Über eine Eigenschaft des Inbegriffes aller reellen algebraischen Zahlen", *J. Reine Angew. Math.*, 77 (1874), 258–262. Consult as well the letters Cantor wrote to Dedekind in this period: E. Noether and J. Cavaillès, eds.: *Briefwechsel Cantor–Dedekind* (Hermann, Paris, 1937).

[16]"Je le vois, mais je ne le crois pas", Cantor to Dedekind in a letter of June 29, 1877, *Briefwechsel Cantor–Dedekind*, 34.

[17]G. Cantor: *Grundlagen einer allgemeinen Mannichfaltigkeitslehre. Ein mathematisch-philosophischer Versuch in der Lehre des Unendlichen* (B. G. Teubner, Leipzig, 1883).

[18]G. Cantor: "Beiträge zur Begründung der transfiniten Mengenlehre", (Part I) *Math. Ann.*, 46 (1895) 481–512, translated by P. E. B. Jourdain: *Contributions to the Founding of the Theory of Transfinite Numbers* (Dover, New York, 1955), in particular section 6, 103–110; and (Part II) *Math. Ann.*, 49 (1897) 207–246, also translated by Jourdain. See in particular Cantor's discussion of the numbers of the second number class $Z(\aleph_0)$ in section 15 of Part II, in Jourdain's translation pages 160–169.

[19]R. Dedekind: *Was sind und was sollen die Zahlen?*, translated by W. W. Beman: "The Nature and Meaning of Numbers", *Essays on the Theory of Numbers*, (Dover, New York, 1963), 31–115, in particular page 63.

[20]G. Cantor: "Über eine elementare Frage der Mannigfaltigkeitslehre", *Jahresbericht der Deutschen Mathematiker-Vereinigung*, 1 (1891) 75–78.

[21]Cantor (1895), in particular sections 9 and 11, and Cantor (1897).

[22]Cantor in a letter to the Italian mathematician Vivanti, December 13, 1893, in H. Meschkowski: "Aus den Briefbüchern Georg Cantors", *Arch. History Exact Sci.*, 2 (1965) 505.

[23]Mittag-Leffler raised the question in a letter to Cantor of February 7, 1883; consult H. Meschkowski: *Probleme des Unendlichen. Werk und Leben Georg Cantors*, (Vieweg, Braunschweig, 1967), 234. Cantor argued the inconsistency of infinitesimals in G. Cantor: "Mitteilungen zur Lehre vom Transfiniten", *Zeitschrift für Philosophie und philosophische Kritik*, 91 (1887) 81–125; 92 (1888), 240–265; in particular section VI.

[24]G. Peano: "Dimostrazione dell'impossibilità di segmenti infinitesimi costanti", *Riv. Mat.*, 2 (1892) 58–62.

[25]In 1899 Cantor corresponded with Dedekind on the subject of the paradoxes of set theory and possible remedies. These were included in a poorly edited form in the edition of Cantor's collected works by E. Zermelo: *Georg Cantor. Gesammelte Abhandlungen mathematischen und philosophischen Inhalts* (J. Springer, Berlin, 1932), 443–450. See as well I. Grattan-Guinness: "The Rediscovery of the Cantor–Dedekind Correspondence", *Jahresbericht der Deutschen Mathematiker-Vereinigung*, 76 (1974) 104–139.

[26]C. Burali-Forti: "Una questione sui numeri transfiniti", *Rend. Circ. Mat. Palermo*, 11 (1897), 154–164.

[27]See Frege's discussion of the impact Russell's letter had upon his *Grundgesetze*, and Frege's attempt to repair the damage, in Appendix II to G. Frege: *Grundgesetze der Arithmetik, begriffsschriftlich abgeleitet*, II (Verlag Hermann Pohle, Jena, 1903), 127–143.

[28]Peirce commented upon the relevance of hypotheses in mathematics in his correspondence, and in papers like "On Quantity", and "On Multitude", in *Mathematical Papers*, 3, pages 41 and 73, respectively. For letters in which Peirce discussed such matters, see those to Georg Cantor (December 21, 1900), to F. W. Frankland (May 8, 1906), to William James (December 28, 1909), and to E. H. Moore (November 21, 1904), all in *Mathematical Papers*, 3, pages 769, 785, 875, 916, respectively.

[29]For studies of Peirce's life and work, consult C. Eisele: "Charles Sanders Peirce", *Dictionary of Scientific Biography*, ed. C. C. Gillispie (New York, 1974), X, 482–488; M. G. Murphey: *The Development of Peirce's Philosophy* (Cambridge, Mass., 1961); and P. P. Wiener and F. H. Young, eds.: *Studies in the Philosophy of Charles Sanders Peirce* (Cambridge, Mass., 1952).

[30]C. S. Peirce: "On the Logic of Number", *Amer. J. Math.*, 4 (1881) 85–95, in *Collected Papers of Charles Sanders Peirce*, eds. C. Hartshorne and P. Weiss (Harvard University Press, Cambridge, Mass., 1960) volume III, 158–170. Hereafter, this edition of Peirce's papers will be cited as *Collected Works*, and the convention of citing *paragraphs* rather than pages will be followed. Thus the article "On the Logic of Number" would be cited as Peirce: *Collected Works*, 3.252–288.

[31]Peirce in a letter to P. E. B. Jourdain, December 5, 1908, in *Mathematical Papers 3*, 883, and in the article

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"Multitude", in appendices to volume *3*, page 1117. Refer as well to a letter Peirce sent to the Editor of *Science* on March 16, 1900, "Infinitesimals", in *Collected Works*, 3.564.

[32]Dedekind discusses his motives in the preface to his essay *Stetigkeit und irrationale Zahlen*, pages 1–3 in the translation cited in note 14.

[33]C. S. Peirce: *Collected Works*, 3.256.

[34]Peirce gave the Hottentot version of the syllogism in his letter to Georg Cantor of December 23, 1900: *Mathematical Papers*, 3, 772. In his paper of 1881, Peirce used Texans: *Collected Works*, 3.288.

[35]A. De Morgan: "On the Syllogism and the Logic of Relatives", *Cambridge Philosophical Transactions*, 10 (1860).

[36]C. S. Peirce: "The Logic of Relatives", *The Monist* 7 (1897) 161–217, in *Collected Works*, 3.526.

[37]C. S. Peirce: *Mathematical Papers*, 3, 51. See as well Peirce's letters to Georg Cantor (December 23, 1900), and to F. W. Frankland (May 8, 1906), in *Mathematical Papers*, 3, 777 and 785, respectively. Peirce also discusses transfinite exponentiation and power sets in what appears to have been a draft for a lecture on "Multitude and Number", probably from 1897. Turn in particular to the section on "The Primipostnumeral", *Collected Works*, 4.200–212.

[38]This suggests that Peirce was either unaware of, or did not fully appreciate, Cantor's paper of 1891, in which he showed that the set of all single-valued functions on the unit interval was greater in power than the set of all real numbers. G. Cantor (1891), cited above, note 20.

[39]Peirce in a letter to E. H. Moore, March 20, 1902, in *Mathematical Papers*, 3, 925. For a discussion of "Generals", consult C. S. Peirce: "Notes on Symbolic Logic and Mathematics", in *Collected Works*, 3.642.

[40]Refer to the note to pages 880, 881 of volume 3, *Mathematical Papers*.

[41]*Mathematical Papers*, 3, 62, and Peirce in a letter to Francis Russell, April 15, 1909, *Mathematical Papers*, 3, 981.

[42]Cantor (1895), cited in note 18 above.

[43]C. S. Peirce, *Mathematical Papers*, 3, 122.

[44]*Mathematical Papers*, 3, 98.

[45]C. S. Peirce, "On Multitudes", *Mathematical Papers*, 3, 59.

[46]*Mathematical Papers*, 3, 60–61.

[47]*Mathematical Papers*, 3, 61–62.

[48]*Mathematical Papers*, 3, 87–88.

[49]*Mathematical Papers*, 3, 88.

[50]C. S. Peirce, *Collected Works*, 3.568, and *Mathematical Papers*, 3, 93–94.

[51]C. S. Peirce, "Multitude and Continuity", *Mathematical Papers*, 3, 94; and the draft of an addition Peirce meant for an article in the *Monist*, as given in the note to Peirce's letter to P. E. B. Jourdain, May 24, 1908, *Mathematical Papers*, 3, 880–881.

[52]Peirce to F. C. S. Schiller, September 10, 1906, *Mathematical Papers*, 3, 989.

[53]Peirce to C. J. Keyser, October 1–7, 1908, and to Josiah Royce, May 28, 1902, *Mathematical Papers*, 3, 898 and 957, respectively.

[54]In addition to Peirce's letters to Keyser and Royce cited in note 53, turn to Peirce's "The Question of Infinitesimals", *Mathematical Papers*, 3, 123–124, and his article on "Infinitesimals", *Collected Works*, 3.570.

[55]C. S. Peirce to C. J. Keyser, October 1–7, 1908, *Mathematical Papers*, 3, 896.

[56]C. S. Peirce: "On Continuous Series and the Infinitesimal", and "The Question of Infinitesimals", *Mathematical Papers*, 3, pages 125, 121–122 respectively.

[57]C. S. Peirce, *Mathematical Papers*, 3, 126.

[58]C. S. Peirce, *Mathematical Papers*, 3, 85.

[59]C. S. Peirce to William James, June 8, 1903, in *Collected Works*, 8.268.

[60]C. S. Peirce, *Mathematical Papers*, 3, 79.

[61]C. S. Peirce, *Mathematical Papers*, 3, 95.

[62]C. S. Peirce, *Mathematical Papers*, 3, 96.

[63]C. S. Peirce, *Mathematical Papers*, 3, 87–89.

[64]C. S. Peirce, *Mathematical Papers*, 3, 98–99.

[65]C. S. Peirce to Paul Carus, August 17, 1899, *Mathematical Papers*, 3, 792.

[66]C. S. Peirce to Francis Russell, April 15, 1909, *Mathematical Papers*, 3, 981.

[67]C. S. Peirce, "On Quantity, with Special Reference to Collectional and Mathematical Infinity", *Mathematical Papers*, 3, section 20, page 56.

[68]C. S. Peirce to Francis Russell, September 18, 1908, *Mathematical Papers*, 3, 968.

[69]C. S. Peirce to C. J. Keyser, October 1–7, 1908, *Mathematical Papers*, 3, 893.

[70]C. Schmieden and D. Laugwitz; "Eine Erweiterung der Infinitesimalrechnung", *Math. Z.*, 69 (1958) 1–39; W. A. J. Luxemburg: *Non-Standard Analysis* (Lecture Notes, California Institute of Technology, Pasadena, 1962; rev. ed. 1964); A. Robinson: *Non-Standard Analysis* (North-Holland, Amsterdam, 1966); W. A. J. Luxemburg and A. Robinson: *Contributions to Non-Standard Analysis* (North-Holland, Amsterdam, 1972).

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