THE CHARLES S. PEIRCE–SIMON NEWCOMB CORRESPONDENCE

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CAREFULLY tucked away in the files of the Manuscript Division of the Library of Congress and in the archives of Widener Library and Houghton Library at Harvard University are the two ends of a correspondence that stirs the imagination and quickens the pulse of the scientist or historian interested in scientific Americana of the late nineteenth century. The correspondents are two of the greatest intellects ever produced in America and their exchange of opinion regarding matters scientific and personal serves as an interesting personalized documentation of the scientific thought of their period. The renowned astronomer, Simon Newcomb, considered the sheaf of letters from Charles S. Peirce important enough to file away with those of other important men in science whom he knew. Peirce, America's belatedly recognized giant in logic and philosophy, preserved a number of Newcomb's letters as well as the drafts of some of his letters to Newcomb of which we have no other record . The correspondence is being published herein for the first time.[1] It is not all continuous although most of it lies in the period 1889–1894.

The fact that the two men were compatriots and contemporaries in science almost year for year in a time of unprecedented scientific inquiry and discovery suggests an inevitable personal and professional contact in the circumscribed society of American scientific life in those days. A short sketch of their lives will account for this exchange of opinion and criticism in their later years.

Although he was born a Canadian in the remote town of Wallace, Nova Scotia, Newcomb's heritage was as thoroughly New England as that of Peirce. Newcomb's father was a teacher, as was Peirce's father, the renowned Benjamin. However, while Newcomb the elder moved frequently from place to place in Nova Scotia to eke out a subsistence living, Benjamin Peirce was firmly established in Cambridge as one of the adornments of the Harvard faculty in the Professorship of Natural Philosophy and Mathematics. Charles was born into comfortable, upper middle class surroundings with the silver spoon of the intellectual aristocracy planted firmly in his mouth.

Both boys were avid readers of whatever came to hand. Neither needed much help in the conquest of elementary mathematics. Newcomb tells of his state of rapture when he stumbled, at the age of fifteen, on the beauties of *Euclid* in his grandfather's copy of the Simpson edition.[2] At the age of thirteen, Peirce came upon his brother's copy of Whately's *Logic* and at once absorbed the principles set forth in the text.

Newcomb was only thirteen years old when he was hired out by his father as a manual laborer to ease the financial strains at home. After three years, the boy had the opportunity to apprentice himself to a "man of science," a doctor who turned out to be a quack. Flight from what then proved to be an intolerable bondage brought him to the shelter of this country which his forebears had left for the deeper wilderness of the north. By the time he was nineteen, Newcomb reached Maryland, and accepted a teaching post in a country school. In 1856 he was employed as a tutor in a family on a plantation whose proximity to Washington at last brought him into a world where men "wrote books" and people "knew the men who wrote books."[3] The hunger for books took him to the Smithsonian

[1] The correspondence was discovered by the writer while doing research for a book on the activities of Charles S. Peirce as a historian of science under a grant from the American Philosophical Society. It is being published with the permission of the heirs of both men and of the Philosophy Department of Harvard University. The writer is indebted to Dr. Elizabeth McPherson of the Library of Congress who was first to suggest the possible existence of Peirce materials in the Newcomb Collection in the Manuscript Division.

[2] *Reminiscences of an astronomer*, by Simon Newcomb, 14, 17, 18.

[3] An excellent short account of Newcomb's life and work is found in the *Memoirs of the National Academy of Science* **17**, 1924, by W. W. Campbell. Three additional papers by R. C. Archibald are of much value: Simon Newcomb (1835–1909). Bibliography of his life and work, *Science*, n. s. **44** (1147): 871–878, Dec. 22, 1916; an earlier version of the foregoing in *The Proceedings and Transactions of the Royal Society of Canada*, 1905, 2nd ser., **11** Sect. III: 79; biographical note in the *Semicentennial history of the American Mathematical Society*, 124–139.

Institution where he met Joseph Henry and J. E. Hilgard. These two men, recognizing the young man's ability, sought to place him at first in the Coast Survey. But nothing came of their hope. Later their sponsorship led him at the age of twenty-two "into the world of sweetness and light on one frosty morning in January, 1857," when he took his seat as a computer between Joseph Winlock and John Runkle "before a blazing fire in the office of the 'Nautical Almanac' at Cambridge, Mass."[4]

The official routine of the Almanac office permitted great flexibility in the arrangement of working hours and thus made it possible for Newcomb to join the Lawrence Scientific School "early in '57 for the purpose of studying mathematics under Professor Benjamin Peirce."[5] He was graduated Bachelor of Science in 1858 and for the three ensuing years was classified as a resident graduate.

Charles Peirce, in his youth, had been under the personal tutelage of his father, and was prepared by the age of sixteen to enter Harvard College.[6] He was graduated without any particular distinction by the College in 1859 and was given an assignment as aid in the Coast Survey to which he was formally appointed in 1861. He, too, found it possible to do this work and attend classes simultaneously at the Lawrence Scientific School. With a specialization in chemistry, Peirce was graduated a Bachelor of Science in 1863, the first *summa cum laude* from that institution; he had been graduated a Master of Arts from Harvard University the year before.

Perhaps the fact that Peirce was four years younger than Newcomb and the fact that Newcomb lacked social roots in Cambridge accounted to a large extent for the surprising lack of evidence of a personal friendship such as Peirce enjoyed at the time with Alexander Agassiz or with William James or even, in a more formal way, with Chauncey Wright. However, a genuine friendliness between Peirce *père* and Newcomb was marked by mutual respect and esteem. When Charles boasts in his writings, as he often does, of the scientific élite who regularly visited his renowned father at home, Newcomb's name is invariably listed among the callers. Moreover, a warm friendship seems to have existed between Newcomb and Charles Henry Davis,[7] Benjamin's brother-in-law and Charles' uncle. Davis was Superintendent of the Nautical Almanac Office and in 1865 became Superintendent of the Naval Observatory, where Newcomb had been assigned also as a Professor of Mathematics in the Navy.[8]

In 1870 the United States Government sent a solar eclipse expedition to the Mediterranean under the direction of Benjamin Peirce who took with him, among others, the two young men. Newcomb was in the party stationed at Gibraltar while Charles did his observing in Sicily.

The friendship between Benjamin Peirce and Newcomb must have been strengthened by the overlap in their work in the Nautical Almanac Office, for Benjamin had been Consulting Astronomer to this agency since its organization in 1849. Later, in 1875, when Benjamin learned that Charles would not be considered as a candidate for the post of Director of the Harvard Observatory, he threw his considerable scientific weight to the support of Newcomb for the coveted position. He was unable to persuade Newcomb to consider an offer which had been made by President Eliot. Newcomb wanted the Observatory post no more than that of the Superintendency of the Coast Survey which Sylvester had urged him to seek after Patterson's resignation in 1881.[9] Two years later, on September 15, 1877, Newcomb became head of the Nautical Almanac Office.

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[4] *Reminiscences of an astronomer*, 1.

[5] Letter to Rev. J. Walker from Newcomb dated March 25, 1857, re conditions for prizes. Sent from Nautical Almanac Office. Now in the archives of Widener Library. Mrs. Elaine Trehub, then of the library staff, brought it to the attention of the writer. The writer wishes to express her gratitude to Mr. Kimball Elkins, Senior Assistant in the Archives, and to Mr. Clifford Shipton, Custodian of the Archives, for securing the permission of Harvard University to use this material.

[6] No definitive biography of Peirce has as yet been written. There is a short biographical account by Paul Weiss in the *American Dictionary of Biography.*

[7] Davis' paternal attitude toward Newcomb is revealed in "Formative influences," by Simon Newcomb, *Forum* **11**: 183–191, Mar.–Aug. 1891. See also "Professor Benjamin Peirce," by Newcomb, *Proceedings of the Royal Society of Edinburgh* **11**: 739–742, Nov. 1880–July 1882.

[8] Newcomb was appointed by President Lincoln in 1861.

[9] *Osiris* 1: 85–154. "Unpublished letters of James Joseph Sylvester and other new information concerning his life and work" by Raymond Clare Archibald. In a letter to Newcomb dated Oct. 20, 1881, Sylvester writes, "Who is to be the new superintendent of the Coast Survey? Why should you not allow it to be known that you would accept the appointment supposing you would be willing to do so!" Sylvester was the eminent British mathematician who served as the first chairman of the Department of Mathematics at the Johns Hopkins University (1876–1883). He returned to England in 1884 to occupy the chair of Savilian Professor of Geometry at Oxford.

Just as the official work of Benjamin Peirce and Newcomb had overlapped, so did that of Charles and Newcomb although they were affiliated with two different government agencies. For example, after Charles had begun his pendulum-swinging experiments as the Assistant in the Coast Survey in charge of the measurement of gravity, he mentioned in a report[10] that Newcomb had seemed to discover a possible new factor adding further difficulty to the pendulum-swinging problem. Again, when Peirce needed the calculations and map of the eclipse of June 29, 1878,[11] Newcomb was asked to furnish copies of the same. When, much later, Peirce was on the defensive against Superintendent Mendenhall's attack on the "backwardness of his work," he countered with, "Now anybody who has ever done such work in such a way,—ask such men as Langley or Newcomb,—will tell you that it is impossible to make any reliable estimate of how much time it will take."[12]

While Newcomb was building his scientific career in the Nautical Almanac Office and at the Naval Observatory, Charles Peirce was acquiring a not inconsiderable scientific reputation in the Coast Survey on three counts. His photometric researches[13] at the Harvard College Observatory brought him recognition as a first-rate astronomer; his pendulum work in Europe as well as in America brought him international recognition; and his measurement of the meter from the wave length of light was applauded at the time and proved to be the forerunner of similar work by Michelson and Morley.[14] Newcomb was later to attempt with Michelson to measure the velocity of light.[15]

Their scientific reputation brought both men membership in the National Academy of Science, Newcomb in 1869 and Peirce in 1877, where both later became members of the important Academy Committee on Weights and Measures.

Other similarities in interests and activities are numerous, but only a few of the more significant will be mentioned. In 1871, after fulfilling the duties of his solar eclipse expedition assignment, Newcomb "holed-in" at the Paris Observatory library, within ear-shot of the besieging nationalist forces, to make a study of the records of earlier astronomers, beginning with 1675, of the occultations of bright stars by the moon. He later considered this the most important "find" he ever made, for he was able, as a result of these studies, to confirm his suspicions that Hansen's tables, then in general use, were unreliable. Charles Peirce, driven similarly by an interest in past scientific achievement and a need to utilize historical documentary evidence in current scientific researches, made a thorough study of a manuscript of a thirteenth-century work[16] at the Bibliothèque Nationale while on an official Coast Survey mission in Paris.

Both men had teaching experience, and in both cases it was severely curtailed. In Newcomb's case the pressure of other responsibilities made it impossible for him to devote full time to an academic career.[17] In Peirce's case personal eccentricities and an inability to work in harmony and "in harness" with others was to frustrate continually his ardent desire for a formal academic connection.

Although creative mathematical skill was incidental to the work of both men, Peirce regarded

[10] From a letter dated July 1, 1873, in the Coast Survey files in the National Archives in Washington. Peirce writes, "Newcomb, in a paper in the last *Ast. Nachrichten*, says he finds that pendulums hung by springs twist and untwist as they oscillate and says this will affect the time of oscillation."

[11] *Ibid.* A letter dated April 26, 1878.

[12] National Archives. From a letter dated March 30, 1888. When Chas. A. Schott, Assistant in charge of the Computing Division of the Coast Survey, was asked to investigate a certain aspect of Peirce's procedure, he approved of it and added, "vide Prof. Newcomb's investigation to free the so-called standard R. A.'s from periodic errors; Wash. Obs. 1870." This remark is from a footnote to a letter to Superintendent Patterson dated Jan. 14, 1879.

[13] Photometric researches, in the *Annals of the Harvard College Observatory* 9, by Charles S. Peirce.

The writer read a paper entitled: Charles S. Peirce, nineteenth century man of science, to the Met. N. Y. Section of the History of Science Society on November 27, 1956. A similar paper has been accepted for publication in *Scripta Mathematica* in the near future.

[14] Michelson and Morley suggested the wave length of sodium light as the standard of measure.

[15] Under a Congressional appropriation, Newcomb worked in Washington while Michelson experimented in Cleveland (1880–1882).

[16] The *Epistle of Petrus Peregrinus on the lodestone*, Paris MS. No. 7378.

[17] Newcomb's professorship of mathematics in the Navy has already been mentioned. He later became Professor of Mathematics and Astronomy at the Johns Hopkins University (1884–1893; 1898–1900. Lecturer from 1876 to 1883). Peirce had lectured on the philosophy of science at Harvard (1864–1865); on philosophy (1869–1870); and on logic (1870–1871). He lectured at the Lowell Institute on the logic of science in 1866. He became a Lecturer on Logic under the auspices of the Mathematics Department under Sylvester at the Johns Hopkins University in the period 1879–1884. The official phrasing of the records veils, perhaps forever, the mystery of Peirce's enforced detachment from the University at a critical period of his life.

Newcomb as essentially a practical astronomer.[18] And yet Newcomb was later to be elected for the years 1897 and 1898 to the office of President of the American Mathematical Society, one of the many positions of high honor which he held in his lifetime.[19] Peirce's self-admitted primary interest and creative talent lay in the area of logic, especially the logic of scientific method. Because both men developed an unusually wide range of interests, both were asked to write extensively for newspapers and periodicals and both were members of editorial boards of dictionaries and encyclopedias. Newcomb wrote numerous books of popular interest in a facile style. Peirce, however, was to find it impossible to produce the manuscripts that would have made his work known in the longer book form.[20]

Later in life their careers again diverged. Newcomb, having retired at the mandatory age of sixty-two from the Nautical Almanac Office, was able to continue many of his professional activities. This was especially true of his important lunar studies which enjoyed the patronage of the Carnegie Institution after his retirement.[21] This support was first granted in 1903 and Newcomb continued with the work almost to the end of his life. He was highly revered and honored, decorated by foreign governments, a member of every important honorary scientific organization in the western world, and president of many learned societies. He seems indeed to have been a notable product of the studious application of his favorite motto, "Whatsoever thy hand findeth to do, do it with all thy might."

Peirce's unhappy star, on the other hand, brought him, despite the recognition of his valuable contributions to logic and his earlier celebrated work in science, such insecurity and misery that only the generous solicitude of a small group of sympathizers, headed by the noble William James, rescued him from actual destitution in the last years of his life. For the files of the Assistants' Correspondence in the National Archives, written in the decade after Benjamin's death in 1880, reveal the tragic story of the gradual decline in value to the Survey of Peirce's service as a member of that organization. His increasing inability to participate harmoniously in close teamwork with his colleagues led to personal skirmishes with Thorn, the "lawyer" Superintendent (1885–1889). Peirce's contemptuous attitude, as revealed in his letters, toward what he considered the scientific ineptitude of Mendenhall, Thorn's successor, rendered inevitable his own resignation from the ranks of the Survey in 1891.

Furthermore, despite Peirce's pretensions to sound business acumen, he found himself by the mid-nineties divested of family inheritance as well as the regular source of income which the Survey attachment had assured him. Beset by legal difficulties and faced with the permanent loss of his valuable library as well as the temporary loss of his Milford estate, "Arisbe," he found survival for himself and his wife barely possible by accepting numerous odd writing assignments, some of which will be referred to in the course of this paper. After negotiations had been concluded for him to use "Arisbe" as his permanent residence, he spent the remainder of his life working part of his land, writing and rewriting incessantly whenever the opportunity offered itself, and sending his manuscripts, whenever he was so employed, to the various publishers by mail. Occasionally he went farther afield to attend meetings of the National Academy of Science, to visit Langley on business at the Smithsonian,[22] or to lecture at Cambridge or in Boston.

And at the very end, Peirce became a victim of that dread malady, cancer. Strangely enough this disease was to fell Newcomb also. Both struggled to the very end to leave behind a precious heritage of ideas gleaned from a lifetime of

[18] Newcomb enjoyed a reputation as a political economist also.

[19] He became Vice-President of the American Philosophical Society on January 1, 1909. He had been elected to membership in the Society on January 18, 1878.

[20] The correspondence of Peirce with various publishers, now to be found in the Peirce Collection in the archives of Widener Library, reveals that Peirce had numerous opportunities to publish but either could or would not meet the conditions set by the publishers. References to details in correspondence other than that between Peirce and Newcomb in the Peirce Collection, are being made with the permission of the Philosophy Department at Harvard University.

[21] Established in 1902.

[22] Mr. H. W. Dorsey, who had been the Administrative Assistant to Secretary Langley in those days, recalled in a telephone conversation with the writer early in July, 1952, Pierce's ill-groomed appearance on such visits and the bitterness between Peirce and Langley created by the controversy over an article entitled by Peirce, "The laws of nature and Hume's arguments against miracles," which Peirce had prepared at Langley's request for publication by the Smithsonian Institution. The correspondence between Peirce and Langley on this subject has been published by Philip P. Wiener in *Proc. Amer. Philos. Soc.* **91** (2): 201–228, 1947. Further details appear in a paper by the writer in *Jour. Hist. Ideas*, Oct., 1957. It is entitled: The scientist-philosopher C. S. Peirce at the Smithsonian.

rich experience. Each man was just past seventy-four at the time of his death.[23]

In closing this brief outline of the careers of these men, it may be well to quote some of Peirce's opinion of Newcomb's work. What Peirce's opinion of Newcomb's contributions to the science of his time had been is obvious from the opening paragraph of his review[24] in the *Nation* of Newcomb's *The Reminiscences of an Astronomer*. It is expressed in the words,

> Newcomb is quite the most distinguished man of science in this country to-day, as well as one of the most eminent in the whole world. His name will remain upon the page of scientific history, and eventually take its place high in the second rank, distinctly above Leverrier's or even Hansen's because of the breadth of his work.

Earlier, in 1901, Peirce had written in the opening article for the *Post*, in its special issue dedicated to a Review of the XIXth Century,[25]

> Thus it happens that we have a magnificent group of great astronomers living among us to-day. We stand too close to them to take in their true proportions. But it is certain that the names of Chandler, Langley, Newcomb, Pickering, and several others are indelibly inscribed upon the heavens.

It is remarkable that Peirce could have so clearly anticipated what the judgment of Newcomb's work by the next generation was to be. For his opinion was confirmed a quarter of a century later when Albert Einstein wrote to a daughter[26] of Newcomb, "Your father's life-work is of monumental importance to astronomy." He mentioned especially her father's contributions to the calculus of perturbations. It was noted in the "Sketch of Simon Newcomb" that Newcomb "had at times remarked that a new law of nature must be discovered to explain the discrepancy between the calculated and observed positions of Mercury," and that, "Soon afterward the theory of relativity seemed to be this new law." Einstein concluded his letter with the sentence, "It was thus that the theory of relativity completed the work of the calculus of perturbations and brought about a full agreement between theory and experience." Without the foreknowledge of the scientific edifice which Newcomb's labors were capable of supporting, Peirce had judged his man fairly and well.

Newcomb's estimate of Peirce's general capabilities as a logician and scientist is difficult to judge, for materials with such references seem to be no longer extant. The correspondence which follows reflects Newcomb's judgment of Peirce's thought as regards numerous specific matters, mathematical and otherwise.

The first item[27] in the correspondence, chronologically speaking, reflects not only Newcomb's interest in political economy but Peirce's approach to questions in that field too. In the last of the series of five papers written for the *Monist* in 1893,[28] the paper entitled "Evolutionary Love," Peirce takes full advantage of an opportunity to castigate the author of a "handbook of political economy—the most typical and middling one I have at hand"—because the author had conferred the title "love of self" on what Peirce called pure greed. The handbook was the *Principles of Political Economy* (1886) and the author was Simon Newcomb. Writing in a similar vein in a manuscript of about 1906,[29] Peirce recalls and reaffirms at this late date his earlier reactions to Newcomb's work.

> I remember two passages in my writings in which I made as much fun as politeness would allow of writers who undertook to tell us what was "conducive to our welfare." Once it was Simon Newcomb who was talking like that in his book on Political Economy; and I remarked that an economist, far from having any qualifications for exploring this most occult of all matters, was particularly unfit for the task owing to his habit of taking it for granted that wealth was desirable.

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[23] Simon Newcomb (March 12, 1835–July 11, 1909). Charles S. Peirce (Sept. 10, 1839–April 19, 1914).

[24] 78 (2021): 237. March 24, 1904.

[25] The Century's great men in science, Sat. Jan. 12, 1901. Peirce contributed the leading article to the *Post's* Review of the XIXth century. On the second page the leading article was by Newcomb and was entitled: Advance in astronomical science.

[26] *Science*, n. s. No. 69: 248–249, March 1, 1929. Dr. Einstein sent this letter from Berlin to Mrs. Josepha Whitney on July 15, 1926. It is found also in a pamphlet entitled "A brief sketch of Simon Newcomb" which was prepared by this daughter and submitted by her and her sisters Anita McGee and Emily N. Wilson to the Electors of the Hall of Fame of New York University. Copy in Butler Library at Columbia University.

[27] From the Newcomb Collection, MS. Division, Library of Congress. The writer has made an effort to clarify all allusions in each letter. Limitations of space do not permit a mathematical analysis of detail in this paper. The writer has in preparation a critical review of Peirce's mathematical contributions.

[28] *The collected papers of Charles S. Peirce*, edited by Charles Hartshorne and Paul Weiss (6.291). Also found in *Chance, love, and logic* by Morris R. Cohen.

[29] *Ibid.*, 6.517.

One of the earliest attempts at an exchange of opinion on this subject has been preserved in the first item.

Dec. 17, 1871

U. S. Coast Survey Office
Washington

Dear Sir

What I meant by saying that the law of Supply and Demand only holds for unlimited competition is this. I take the law to be, that the price of an article will be such that the amount the producers can supply at that price with the greatest total profit, is equal to what the consumers will take at that price. This is the case with unlimited competition because nothing that any individual producer does will have an appreciable effect on the price; therefore he simply produces as much as he can profitably. But when production is not thus stimulated, the price will be higher and at that higher price a greater amount might be profitably supplied.

To state this algebraically:—

The amount that can be profitably produced at a certain price X is the value of y which makes $(X_y - z)$ a maximum so that $X - D_y z = 0$. But the price x which the producer will set will be that which will make $(xy - z)$ a maximum so that $y + D_x y(x - D_y z) = 0$.

Clearly $x > X$ because $D_x y < 0$. In the case of unlimited competition, however, the price is not at all influenced by any single producer so that x is constant $D_x y = \infty$ and then the second equation reduces to the first. If this differentiation by a constant seems outlandish, you can get the same result another way. But it is right for if the producer, in this case, lowers his price below what is best for him there will be an immense run upon him, if he raises it above that he will have no sales at all, so that $D_x y = -\infty$.

If the law of demand and supply is stated as meaning that no more will be produced than can be sold, then it shows the limitation of production, but is not a law regulating the price.

Yours very truly
C. S. Peirce

Prof. Simon Newcomb U. S. N.
Observatory
P. S. This is all in Cournot

Next in sequence is the draft of an incomplete letter[30] written by Peirce after Sylvester had returned to England and Newcomb had succeeded him in the position of Editor-in-Chief of the *American Journal of Mathematics* for the periods 1885–1893 and 1899–1900. There seems to have been considerable friction regarding the material Peirce wished to have published in that *Journal*, for echoes of his complaints about Newcomb's lack of cooperation are heard in many places. For example he writes in one place,[31]

I wrote out in 1885 a full systematic exposition of the notation and its use: but I did not print it. Indeed, it was, in effect, refused by the editor of the *Am. J. Math.*, Simon Newcomb, who said he would print it, if I would declare it was a mathematical paper. That I could not do.

Milford, Pa. 1889 Jan. 17

My dear Newcomb:

If the following proposition be not too well-known or obvious, possibly the proof I give of it may be worth filling up some corner of your journal with sometime.

Through a point in space let 4 right lines be drawn. Let 4 points, assumed one on each of these lines, be taken as the vertices of a tetrahedron; and let a second tetrahedron be formed in the same way. Then the lines of intersection of corresponding planes of the tetrahedron lie in one plane.

Proof. Suppose we have a system of Cartesian or homogeneous coordinates, but that all the equations contain terms in an additional and meaningless variable, which has to be eliminated. This the same as regarding the 3-dimensional space as a perspective from a 4-dimensional space. A single equation has no meaning, except considered as determining a 3-dimensional space. Two linear equations determine a plane, 3 a line, 4 a point then, any 3 lines determine . . . .

Incomplete

The third item[32] in the correspondence has led to the discovery of a most interesting public con-

[30] From the "fragments" of the Peirce Collection in the tunnel of Houghton Library at Harvard University. Another draft of this letter is found in the Charles S. Peirce Collection of the University Archives section of the Harvard College Library (1A Math. Box 4). It, too, is incomplete. It opens thus: "The following proposition must be included virtually in Hamilton's discussion of nets in space; but it is not stated by him and does not seem to me obvious. It may be well-known.

"Through a point in space describe four right lines. Assume a point on each and make these four points the vertices of a tetrahedron. Assume a second set of four points on the four right lines and make them the vertices of a second tetrahedron. Then corresponding faces of the tetrahedrons, being produced, cut one another in 4 complanar lines.

"The proof is excessively simple."

A third draft, identical with the above, is found in the Peirce Collection, Box V B2b.

[31] *Ibid.* 1A Math Box 4, folder 29.

[32] Newcomb Collection, MS. Division, Library of Congress.

troversy between Newcomb and Peirce in the columns of the *Nation.*

After the publication in 1889 of the *Century Dictionary* of which Peirce was an Associate Editor, a review of it appeared in the *Nation* on May 30.[33] The issue of June 13[34] carried a "Letter to the Editor of the Nation" which opened with the remark that "Your recent review of the 'Century Dictionary' ought to be supplemented by some remarks upon its definitions of terms in physical science, while there is still time to make corrections." Among the terms itemized and discussed is the *Law of action and reaction.* The letter concludes with the sentence, "It ought to be added that, so far as I have noticed, the definitions in mathematics and mathematical physics are not subject to this criticism." It is signed by Simon Newcomb.[35]

On June 20 an answer appeared in the "correspondence" section of that issue of the *Nation.*[36] It had been sent by Charles S. Peirce and in it he admitted authorship of the definitions under fire. He explained, among other things, that an important sentence from the *Principia* had been inadvertently omitted from the definition of Newton's "law of action and reaction" under the general discussion of the term *action.*

Newcomb replied in the next issue[37] which appeared on June 27. He expressed great surprise that Peirce had been responsible for the definitions under attack and writes, "The contrast which I mentioned between the definitions in mathematics and mathematical physics and those in astronomy and experimental physics I supposed to mark the line between his work and that of some less skilful hand." As for the *Law of action and reaction,* Newcomb could find no such definition in the *Principia.* He finally ends the letter with the sentence, "*Argus,* the constellation, is omitted, though *Aries* and *Aquarius* are included."

109 East 15th Street Century Club 1889 July 2
My dear Newcomb

As it doesn't seem very becoming for us to be disputing about *Argo* or *Argus* and the like, I won't answer your last (in) the Nation. But I am in a situation to know that such a criticism as yours tells upon the sales of the work. You will find the definition of action in the Principia toward the end of the Leges Motus. "Nam si æstimetur agentis actio ex ejus vi et velocitate, conjunction," etc.

Yours faithfully
C. S. Pierce

The series of letters[38] which next appears seems to have originated in Newcomb's aforementioned criticism of several of Peirce's definitions in the Century Dictionary. Although the reputations of both men had brought them the opportunity to serve as contributing editors on the staffs of dictionaries and encyclopedias,[39] each entertained a very low opinion of the competence of the other in that work. There exists an early draft of a letter[40] which Peirce presumably sent to Professor Baldwin at Princeton in which he severely criticizes "Newcomb's articles" for that dictionary. He reveals also that Newcomb had written to him and had said that Peirce's

views about limits were forty years behind the time. The correspondence which followed showed he [Newcomb] had not read any of the remarkable works on the logic of mathematics of late years and in short what it came to was that I was returning to a view which the nominalism of forty years before had persuaded him was wrong and he had not advanced a step since that time.

Peirce claimed also that Newcomb's views were "very narrow both on the philosophical and on the mathematical side"[41] and that he therefore

[33] No. 1248, p. 450.
[34] No. 1250, p. 488.
[35] Sent from Washington on June 8.
[36] No. 1251, p. 504. Sent from Milford, Pa., on June 14.
[37] No. 1252, p. 524.

[38] Nov. 30, 1890, Peirce MS. fragments at Houghton Library. Dec. 12, 1890, Peirce MS. fragments at Houghton Library. (. . .), 1890, Peirce Collection, Widener Library archives. Box VB2b. The upper right hand corner has been torn from the sheaf of pages of the last letter. The missing words are indicated by dots.

[39] Newcomb was an associate editor of Johnson's *Universal Cyclopedia* and of Funk and Wagnall's *Standard Dictionary.* He was mathematics editor with H. B. Fine of Baldwin's *Dictionary of Philosophy and Psychology.* Peirce was a contributing editor to the *Century Dictionary.* He was logic editor with C. Ladd-Franklin of Baldwin's *Dictionary.* He was color editor of Funk and Wagnall's *Standard Dictionary* until he was dropped for failure to meet deadlines.

[40] Written Oct. 20, 1900. Peirce Collection at Widener Library, Box VB2C.

[41] Written Dec. 26, 1900. Peirce Collection. General Correspondence, Widener Library. Peirce also said, "He is not a mathematician or at any rate has only become so late in life,—he is only a mathematical astronomer." See footnote 40. G. W. Hill, the famous mathematician on the staff of the Nautical Almanac, confirmed this opinion of Newcomb in an obituary note (*Science,* n. s. **30**(768): 353–357, Sept. 17, 1909). He said, "While Professor Newcomb wished always to be accounted a mathematician,

intended "to pitch into his articles and try to render them serviceable to students of philosophy."

There is an obvious "give and take" in these disputes between the two men. Moreover, lest the reader form the impression that Peirce's intellectual aggression was centered on Newcomb, other instances of it are cited. Unpublished correspondence, for example, in the archives at Widener Library reveals that Peirce had protested against Professor Fine's definition of *continuity* in the same dictionary. Fine retaliated[42] by questioning Peirce's use of the mathematical term *discrete*. Again, when Royce's Supplementary Essay on *The World and the Individual* appeared, Peirce's critical reaction impelled him to contribute a long article entitled "Infinitesimals"[43] to the Editor of *Science* who promptly published it.

Since the logical foundations and fundamental definitions in certain areas of mathematics were being questioned on all sides at that time, these letters serve as a peep-hole into the scene of the struggle for basic agreement on the direction of the further development of the subject.[44] The first of the letters in the series opens with the words

Don't revile me for this bulky document; but put it aside till you feel disposed to look at it.

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his work seems motivated by its possible application to astronomy, and no very weighty contribution from his pen has accrued to pure mathematics."

[42] Jan. 14, 1901. Peirce Collection. General Correspondence, Widener Library.

[43] *Science*, n. s. 11 (272): 430–433, Mar. 16, 1900. Dated Feb. 18, 1900. In a ten-issue article called: Zeno's arguments on motion, Cajori says in the *Amer. Math. Mo.* 22: 220. "In America C. S. Peirce has adhered to the idea of infinitesimals in the declaration: 'The illumination of the subject by a strict notation for the logic of relatives had shown me clearly and evidently that the idea of an infinitesimal involves no contradiction.' Apparently, before he acquired familiarity with the writings of Dedekind and Georg Cantor, C. S. Peirce had firmly recognized that for infinite collections the axiom, that the whole is greater than its part, does not hold."

Peirce reviewed vol. I of Royce's work in the *Nation*, **70** (1814): 267, Ap. 5, 1900; vol. II in **75** (1935): 94, July 31, 1902.

[44] Peirce's definitions of *limit, doctrine of limits*, and *infinitesimal* are to be found in the *Century Dictionary.* Newcomb's definitions of *infinite, infinitesimal, limit* are in the 1901 Appleton edition of *Johnson's Cyclopedia.* Newcomb wrote a paper entitled: Remarks on the doctrine of limits, in the *Analyst* No. 9: 114, 1882. In it he discusses definitions from Wood and Wentworth.

Milford Pa 1890 Nov 30

My dear Newcomb:

It is you I am sure who write, although your letter is not signed; and it is always of great advantage to me to have your criticisms. I infer, however, that you have not read the important papers of G. Cantor, which are condensed in Vol 3 of the Acta Mathematica. I do not agree with the whole of Cantor's conclusion, but regard his most fundamental points as established and among others we can perfectly well reason about infinite quantities. However, I do not derive this opinion from Cantor, but had already before seeing these papers reached demonstrative conviction of this. In fact the general forms of reasoning are perfectly applicable to infinite quantities, and it is finite quantities which are peculiar in that certain modes of influence hold in regard to them which do not hold with regard to objects in general.

In my definition of the doctrine of Limits I have thrown a sop to Cerberus by the remark that the best contention in favor of that doctrine was that nothing having no reference to conceivable experience could have any meaning, and therefore phrases relating to infinity must be interpreted as having some relation to what might be experienced. This is true; but I might have added that this principle does not suffice to indicate the Doctrine of Limits since infinite quantities are capable of being conceived as objects of experience, though not of measurement.

I find no fault with the doctrine of limits except that it is an unnecessary going about, founded on a logic that is no logic but a mere hampering by an association of ideas. The notion that reasoning about finite quantities is easy and intelligible, that about infinites incomprehensible, simply arises from strictly logical analysis being replaced by an intuitional process which does not easily generalize itself, or rise to wider occasions.

You say that if $A - B$ is less than every finite quantity, "*it follows*" that $A = B$ that is, that $A - B$ is zero. "*For*", you say, if A is not equal to B, their difference must be finite. But it seems to me that this is neither more nor less than what you have to prove.

The situation is precisely that of the non Euclidean geometry a few years ago. You say $A + h = A$ when h is infinitesimal is illogical or *inexact*. You of course admit, however, that $A + 0 = A$. So that you hold that an infinitesimal differs from zero. Of course, one way of urging the doctrine of limits is to say that differentials are really finite quantities. This, no doubt, is what you mean. You hold that $(A + h)^2 = A^2 + 2Ah$ approaches nearer and nearer to the truth without limit when h is diminished and is exactly true for $h = 0$. You regard it as inexact when h is infinitesimal. At least, that is your language; but you do not mean by it what I should do, taking it literally.

The results of the differential calculus can undoubtedly be reached through the doctrine of limits. It is an attractive method too. But in my opinion it is just as wrong as to reach the results of the imaginary calculus by means of an interpretation of imaginaries. "Wrong" in the sense of unphilosophical I mean, not incorrect.

Very faithfully,
C. S. Peirce

Milford 1890 Dec. 12

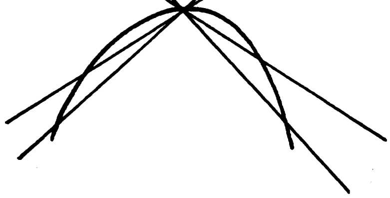
My dear Newcomb:

It is very gratifying to me to get so prompt a reply to my letter. I am confident discussion of these points must bring us to or toward an agreement.

1.—You say my treatment of *limit* and *infinitesimal* are not in accord with the best mathematical thought of the day. But allow me to say that as you have not read, or at least not deeply considered the papers of G. Cantor, you are in the situation toward the subject in which twenty years ago those mathematicians were who had not read or had not studied Lobatchewsky, in reference to that subject. If you were to say that my definitions mentioned are not in accord with the *prevalent* mathematical thought, perhaps you could prove your point; but I would rather be in accord with views which are in my opinion surely destined to prevail in a few years.

2.—You say you cannot comprehend what I mean by "the point at which a variable upon which a function depends passes through infinity."

I will explain. First, observe that I do not say there is such a variable when *limit* means simply boundary. Thus, where time passes from one day to the next, I do not say that at 0^h of the second day any variable considered passes through infinity. Second, I am not speaking of a variable connected with a function in a strictly definite manner. But when it is said that a limit is a value which a variable is conceived to approach indefinitely but never to reach, then I say there is an express reference to another variable. The words of *approach* and *never* are words of time and relate to some vaguely conceived variable. The word *never* indicated the point where *that* variable, assimilated to time in the language used, passes through infinity. I imagine (see figure) in a plane a line rotating about



one of its intersections with a curve. As it turns (clockwise), the other intersection runs from the left up toward the fixed intersection. In this conception, the vague variable imagined as time is introduced. If the rotation is something like uniform, the position for which the secant becomes a tangent is a mere boundary between left-handed and right-handed intersections. But in the differential calculus, those who use the doctrine of limits say that the tangent is the limit toward which the secant tends when its second intersection *indefinitely approaches* the first. To "indefinitely approach," can only mean, as some writers distinctly state, to approach closer than any given finite distance but *never* reach. Such approach supposes something imagined as time running on to infinity.

I think the *first* moment of my idea, that the idea of a limit, as used in the differential calculus, involves the idea of a variable (i.e. a vague variable) passing through infinity where the state of things called the limit is reached, is thus made clear. But there is a *second* moment of my thought, namely that this vague variable is only *passing through* infinity. I think it is quite in accordance with "the best mathematical thought of the day" to regard the value *infinity* as a mere point through which a variable passes without coming to any abrupt termination there or anywhere. Of course, the real thing which the mathematical object simulates may do so, but pure mathematical lines join themselves though (as in hyperbolic geometry) they have to pass through a region of imaginary values before they do so. And in pure mathematics there is no difference between a line and a linear quantity, unless a mere point of view. So that when it is said that a variable "never" reaches its limit, that will not prevent our conceiving it as getting there and *passing through* it, all the same.

3.—You say you object to my definition of limit, "*totally*." Now let us see what it is. I give first a generic definition, and then a specific one under it. (After which I criticize another definition which I do not assent to.) The general definition is: "the precise boundary between two continuous regions of magnitude or quantity." Thus, a tangent is the precise boundary between the region of *neighboring* right-handed secants and neighboring left-handed secants. I then give a specific definition under that: "the point at which a variable upon which some function depends passes through infinity." I have shown above why I give the first as the universal meaning, and why and how I hold that as used by writers on the differential calculus it really has the second meaning, though they fail to make themselves perfectly clear about it, even to themselves.

4.—The definition then given and criticized is *not* "intended to be the sense in which the word is always used." It is intended to be a statement of an attempt frequently made to analyze the usual meaning in the differential calculus. You say I am wrong in describing this definition as tacitly assuming that the variable depends upon another. I did not intend *quite* to say that, for I do not think it *quite* true. It would be true, if for "tacitly" we read "expressly." But *that* is not what I intended to say, for *that* is a statement to which I should not *object* at all. What I object to about the definition is, in the first place, that it assumes this vague variable, which I fully admit, *to increase by finite steps*. The variable it *expressly* and rightly asserts. The increment by finite steps it *tacitly* and *unnecessarily* assumes. This criticism I shall not defend because you do not attack it.

5.—You say you do not understand what I mean by saying that that definition's "overlooking the essential cond[it]ions of continuity." But that is not what I say. I say it overlooks the essential element of continuity. That is to say, it defines a limit by that very conception embodied by the word *infinitesimal* (which word I forgive because everybody overlooks or forgets its etymological meaning) namely as if it were nothing more than that toward which a series converges,—the sum of an infinite series. Here there is no *continuity*; while a limit, according to my notion of what people mean by it, is the boundary between two *continuous* regions.

6.—You say I do not define Method or doctrine of limits. I *profess* to do so. I do not *confine myself* to stating what I suppose to be its logical basis, nor do I state *at all* what I suppose to be its logical basis. I state what I conceive to be its *essence*; and that is as much as the dictionary can find room for in regard to any doctrine or method. Perhaps, I might have a little specimen of it.

7.—I don't understand what your proposition is about $a + i = a$. If a person adopts the conception of infinitesimals, this certainly seems a fundamental proposition of the calculus. If you adopt the method of limits an infinitesimal is regarded as the limit of a quantity that vanishes. That $A + \lim B = A$ when $\lim B = 0$ cannot well be denied, and this is all the meaning that an adherent to the doctrine of limits can see in $A + i = A$. But perhaps from that point of view it may not be a fundamental proposition.

8.—You say you think I am wrong as to mathematicians differing in regard to the interpretation of $A + i = A$. I think that shows you have not read recent discussions of infinitesimals etc. To support this you say all would agree that "all reasoning about infinitesimals and infinites" admits of being reduced to the forms of Euclid. If for the words in quotations you put "the results of the calculus, in regard to finite quantities," I admit it. Otherwise, it is quite indefensible. As for Euclid's axioms, I consider them as innocent as they are impotent,—except the ridiculous 9th, which has nothing to do with the subject. That "infinities and infinitesimals are non-existent in thought" you are altogether mistaken in supposing mathematicians agreed upon. This simply shows as I said you have not read the papers. In the next place, even if mathematicians were agreed on this psychological nonsense, it would not prove anything at all about the proposition $A + i = A$.

9.—You say you never heard of anyone claiming that we cannot reason correctly about *infinite quantities*. What? about *infinite quantities* strictly speaking, or about what the method of limits substitutes for them and calls by that name? What was the motive of Lagrange's Theorie des fonctions? The first book I pull out is Jordan's treatise where I read: "Quant à l'infini, il échappe à toute mesure et ne sauvait entrer dans un calcul."

Very faithfully
C. S. Peirce

Milford, Pa. 1890 (. . .)

My dear Newcomb:

Merry Christmas (. . . .) As you signify in your last that you have got through with your criticisms on my definitions of *infinitesimal, limit*, and *Doctrine of limits*, I will summarize them and my answers.

You "object to my definition of *limit* totally and of infinitesimal partially, as non-accordant with the best mathematical thought of the day, and savoring too much of notions current a century ago, and current still among men whose ideas are improperly co-ordinated."

Let us see by a review of the arguments whether you have succeeded in justifying this judgment or not.

Against my definition of *infinitesimal*, you object that I do not define it in terms of the doctrine of limits. I do refer to that mode of treatment, stating that it avoids sundry difficulties. I do not adopt as giving the (. . .) and accurate meaning of infinitesimal, (. . .) view which one of its chief defenders characterizes thus: "The method of limits is, in reality nothing more than one way of evading the use of the word infinite in its absolute sense." But this does not satisfy you. You thought "that for 50 years, at least, everyone who understood the subject accepted the view that an infinitesimal is simply a quantity approaching zero as its limit. In reply, I instanced Georg Cantor, when you remarked that the papers of Cantor "came before" you some years ago, but you could see nothing of value in them. Your expression suggests that you rejected something offered for the Am. J. Math. The editor of the Acta Mathematica, which I may without disrespect for your journal characterize as without doubt the most profound journal of mathematics published, saw so much in Cantor's papers that he gave up one entire number to them,—although most of them had already been published. The (. . .) excited a discussion in which some of the (. . .) mathematical heads of our time have taken part. Not to be acquainted with them is to be unprepared to enter into a modern discussion of the conception of infinity.

In order to show the difficulties with which the doctrine of infinitesimals is beset, I had remarked that it was necessary, i being infinitesimal, to put $A + i = A$, an equation "representing" a fundamental proposition of the calculus.

You think this statement "illogical or inexact." It "simply astonishes" you. Your "objection" is simply that of the school-boy. If $A + i = A$, then, substracting A, it follows by Euclid's axiom, $i = 0$." In reply, I pointed out that I had indicated in the Dictionary two ways which had been resorted to to escape this. One is to say $i = 0$. That *zero* is subject to all the rules of algebra without exception will, I take it, hardly be disputed at this time of day. It is true (. . .) as you say, with an assured air that taken in connection with its accompanying unconsciousness of what has been going on in the logical world is charming, that "zero cannot be *treated as* a term of a ratio under any circumstances." You do not say how you would prove this proposition. The other way of escaping the difficulty about $A + i = A$, I said was, to regard the sign of equality as meaning measurable equality. In order to make my meaning clear to you, I supposed a telepathic communication with infinitesimal beings. We can measure A but not i; they can measure i but not A. Neither of us can measure any difference between $A + i$ and A, *as* a difference; though they can measure i.

In order to have successfully attacked my definitions, the one thing you would have had to show was that the idea of a quantity immeasurably small involves contradiction. This you have utterly failed to do; and have shown you were not properly (equipped) for the discussion.

However, you return to the equation $A + i = A$ in your last, and undertake to prove that it does not represent a fundamental proposition of the calculus. You propose to make this out by showing that the application of it is equivalent to the "passage to the limit." This, however, even if established would leave your proof incomplete. It would still remain to be shown that the passage to the limit is not a fundamental part of the doctrine of limits; a premise required by your reasoning, though it would have a double back action if granted.

But you do not show that the application of $A + i = A$ is equivalent to the passage to the limit. You only show, what was well known, that there is a close analogy and correspondence between the two ways of reasoning. There is an almost precisely similar analogy between scientific induction and the *reductio ad absurdum*. There was an ancient dispute between the Epicureans and Stoics, the former advocating the use of induction, the latter only allowing it in cases where it could be shown to be nothing but a *reductio ad absurdum*. (. . .) the logic of the doctrine of limits is as stupid and narrow as that of the Stoics, its consequences are not so bad, since it happens that the most important results really can be established by the method of limits.

On the whole, I have nothing to modify in my definition of *infinitesimal*, though I could have made the matter clearer in much greater space. It is to be remembered that it is no purpose of a dictionary to inculcate sound doctrine; it is simply to explain words, ideas, etc. Moreover it would not be expected that under *infinitesimal*, I should repeat all that has been put under *infinite*.

To my definition of the word limit, you "object totally." You "cannot comprehend what is meant by the point at which a variable upon which some function depends, passes through infinity." It would seem then that you are not *au fait* of what has been done of recent years concerning the foundations of the theory of functions. The fashionable definition of to-day, if I may use the expression, of a *limit* among the chief upholders of the doctrine of limits is the following. The limit of a variable, x, is a value, c, such that for every positive quantity, ϵ, sufficiently small, a value N can be assigned to a number n, making mod $(x_n - c) < \epsilon$ for every value of n exceeding N. Here in n you have the vague (variable) to which I refer becoming infinite at the limit. You will perceive therefore that the fact that you could not comprehend what I meant did not suffice to relegate me to the class of those whose ideas are imperfectly coordinated.

My definition departs from the fashionable one in two ways. First, it removes the unnecessary restriction that n must vary by discrete steps; since I regard the conception of *limit*, when understood in the way it must be understood to make it serve its purposes, as well as in the thought of most of those who employ it, as involving or, at least, not *excluding* the idea of continuity. In the second place, in the interest of co-ordination of ideas, I regard infinity, just as in projective geometry, as a value to be "passed through." But you must not suppose that when I thus vaguely describe my reason for treating the matter as I have done that my reason itself is vague. On the contrary, it rests on exact algebraic discussion of the reasoning concerning finite and infinite quantities which cannot be successfully controverted. To say of views so founded that they "savor of notions current a century ago" suffices to demolish them just as much as kicking the great pyramid would bring that down.

Having given my own definition of limit, I state the common one which I reject. I have to put this into popular language, and I use an expression which has often been used by the advocates of the way of limits. I admit that in the bare statement, without seeing how it is used, it is not *evident* to a person not very well up in the subject that the statement in popular language is open to the charge of representing the vague variable as varying by discrete steps. Such however is the peculiar import of the word *never*.

I admit this definition is somewhat wanting in perspicuity. The vague variable requires explanation and the nature of the objection to the common definition needs elucidation.

I am unable to see however that you have shown my views to be antiquated or confused. It is you who do not seem to be aware of modern ideas.

We come now to the definition of the *doctrine of limits*. You say "I suppose there is a method of reasoning in mathematics called the method of limits . . . which method was employed by Euclid." This involves an inaccuracy in regard to the phrase which is serious as regards any criticism of my definition. Euclid uses in the 6th book a method of reasoning which Archimedes developed into the method of exhaustions. Supposing this method of reasoning were the essence of the method of limits, it would be far from true that Euclid used the method of limits. The difference is this: the method of exhaustions is a method of direct geometrical reasoning; the method of limits is a method of establishing the first propositions of the differential calculus. This is the language generally recognized by careful writers. The method of limits is a method of establishing the calculus without the use of true infinitesimals nor series by reasoning concerning the limits of finites. (. . .) such, it has several varieties. The one which you use, and which you would have me exclusively recognize, is not generally used, and is the least demonstrative of any.

You endeavor to make a distinction between the *method* and *doctrine* of limits, which I have treated as synonymous, by making the *doctrine* the "principles" of the *method*. But the distinction is not tenable. In establishing the propositions of the calculus it is necessary to enumerate the principles and there is none but a hair-splitting distinction between the method and the doctrine of limits.

You also think the latter "somewhat incorrectly" called the doctrine. I don't know what you have got in your head. It is like saying Ptolemy's Almagest discusses no "problems." If I know anything of the terminology of logic, the definition of *doctrine* in the Century Dictionary is correct: "the principles of any branch of knowledge; anything held for true."

In undertaking to state the essential characters of the doctrine of limits, I begin by saying that it rests on the idea that infinitesimals in the strict sense "cannot be reasoned about mathematically." To this you object, because you never heard of anybody's holding such an opinion. It is, nevertheless, the general opinion of those who adhere to the doctrine of limits. I have twice asked you whether you really mean to say that you hold that infinitesimals can be correctly reasoned about mathematically. To that you will not reply, because it is an argumentum ad hominem. To show, however, that I assume that dx is *not* infinitesimal (the very opposite of what I do assume) you say that I make it "subject to laws of multiplication." Now, this argument has no validity, unless I deny that infinitesimals are subject to the laws of algebra. So it appears that in your view those who hold the doctrine of limits as well as everybody else you ever heard of admit that infinitesimals are subject to mathematical reasoning, while those who adhere to the method of infinitesimals do not regard them as the subjects of such reasoning! You are all mixed up here. Those who consider that infinitesimals can be reasoned about mathematically have no need of resorting, and do not resort, to any adjurant theory for the differential calculus such as the doctrine of limits.

You say that I "object to the way in which" the doctrine of limits "has been defined in such a way as would lead the reader to suppose that the whole method was fallacious." This is a singularly hasty statement which will not bear examination, as you shall see. In the first place, I have raised no objection in the dictionary to any definition of the doctrine of limits. I have objected to a definition frequently given to "limit." But that does not lead any rational person to suppose the whole method of limits fallacious. To the doctrine of limits, I have made two criticisms. The first is, that the notion that we (cannot) reason directly about infinitesimals is unfounded. No sane person could conclude from that remark that indirect reasoning about them was fallacious. The second remark is that the method of infinitesimals "harmonizes better with recent advances in mathematics." If a reader is so thoughtless as to suppose this means the method of limits is in downright conflict with recent discoveries in mathematics, he will fall into the error you mention. I don't think there is much danger of that, but I will change to "is more in the spirit of modern mathematical philosophy," in another edition.

You say you "cannot gather whether" I "admit the validity of the method of limits or deny it." This I regard as high praise. I hope I may have treated all controverted doctrines in such a way that it is not easy to gather on which side of the controversy I range myself.

But I will mention that, in fact, if a *limit* is correctly defined, so that the approach to it may be continuous, and the great (distin . . .) of the discrete and continuous infinite be not lost from sight, the reasoning being like that concerning the line at infinity in projective geometry, then I see no objection to the method of limits except its unnecessary circumbendibus.

In the form which you would give to it, the error is not serious, but I do not think it is perfectly demonstrative. To say that the error of a statement is less than any assignable magnitude is certainly not to say there is no error: Euclid himself would admit that.

You say, I give no adequate definition of what the method is. But the truth is you are mistaken as to what the method of limits is. It does not consist in reasoning in a special way, and were I to select two forms of the method as illustrations, thus greatly lengthening the definition, neither would be that which you wish me to give exclusively.

On the whole, I do not think your original judgment of the three definitions has been shown to be manifestly just.

Yours very faithfully
C. S. Peirce

Peirce refers constantly in his writings to the influence of Lobachewsky, Clifford, Cayley, Cantor, and Klein on the direction of his mathematical thought. Historically, the idea that our space might be non-Euclidean with a scientifically verifiable curvature had been developed along with the non-Euclidean geometries shortly before Peirce penned the next letters.[45] It is curious that he should in these letters cling to the theory of negative curvature when the computations gave him the "reverse of what" he wished, for he was an old hand at the theory and practice of testing scientific hypotheses. He was most certainly acquainted with Riemann's celebrated paper, which in Clifford's translation becomes, *On the Hypotheses which Lie at the Bases of Geometry*. Apparently the strong Lobachewskian influence, which is so evident in his mathematical manuscripts, prevailed at this time.

My dear Newcomb:

I want to get into circumstances in which I can pursue certain researches. I want you to do certain things to aid me, and to that end, I want you first to remark how encouraging the figures look in regard to my attempt to make out a negative curvature of space.

1.—The small number of negative parallaxes, considering the errors to which such work is subject, is an argument that their real values are not very small. I think infinitely distant stars may for aught we know have parallaxes of $0''.10$.

2.—When we look at the numbers of stars of different magnitudes, we are struck first with the fact that they increase with the magnitude in a pretty regular way, and second that they do not increase nearly so fast as equable distribution requires, especially for the fainter stars.

Now the surface of a sphere (or the reciprocal of the light) is proportional to $[\sin h(r/2k)]^2$.

The volume of a sphere is proportional to $[\sin h(r/k) - (r/k)]$.

In the D M the number of stars of magnitude
4.8 and brighter is 468 = (2.670) of magnitude
8.8 and brighter is 70197 = (4.846)

This gives the ratio of increase per magnitude (0.544) instead of (0.600) according to the Euclidean Theory. I cannot now go further, as I want the latest photometric measures of DM stars.

3.—It is well-known that the proper motions of faint stars are not much smaller than those of brighter ones. Some years ago I compared the photometric measures of Seidel with Mädler's proper motions and found as follows

Mag	Distance from Proper Motion	Square Root Distance from Brightness
1	1.00	1.00
2	1.32	1.35
3	1.62	1.62
4	2.00	1.91
5	2.45	2.29
6	2.56	2.88

Now a proper motion has two parts, a parallactic part, inversely proportional to $\tan h(r/2k)$ and a stellar part, inversely proportional to $\sin h(r/2k)$. The component directed toward the apex of the sun's motion is in the mean entirely parallactic, the component perpendicular to that is entirely stellar. These two are in the mean not far from equal.

Hence, the total proper motion is in the mean proportional to

$$\sqrt{\frac{1}{\sin h^2(r/2k)} + \frac{A}{\tan h^2(r/2k)}}$$

where A is such that the terms are generally nearly equal. Now $\tan h(r/2k)$ is nearly constant for large values of r; so that this is not very remote from

$$\sqrt{\frac{1}{\sin h(r/2k)}}$$

[45] No date, Newcomb Collection, Library of Congress. The first page of a draft is in the Peirce Collection, Box VB2b, at Widener Library.

Dec. 21, 1891, Newcomb Collection, Library of Congress. (The number sheet mentioned is not in the folder in the files.)

Dec. 21, 1891, draft in Peirce Collection, Box VB2b, Widener Library.

Dec. 23, 1891, *ibid.*

No date, *ibid.*

The writer has found in the archives of the Smithsonian Institution a complete manuscript by Peirce entitled: On two map-projections of the Lobatschewskian plane. It reached Langley too late to be read at the Academy of Science meeting for which it had been intended.

To show this, the ratio of Distance from brightness to Distance from proper motion is (multiplied by a constant)

Mag	Observed Ratio	$\sqrt{1 + 2 \cos h^2(r/2k)}$	Where $2k$ = Dist of 5th mag stars.
1	1.11		The reason I multiply $\cos h^2 (r/2k)$
2	1.53	1.78	by 2 is to make
3	1.80	1.85	the two terms
4	2.02	2.02	about their actual
5	2.38	2.40	relative values.
6	3.60	3.05	

There were only a few 6th magnitude stars in Seidel's list.

4.—But the question is whether the component of the proper motion perpendicular to the great circle from the star to the apex of the sun's motion really diminishes (as $r/2k$ increases) relatively to the component in the line to the apex.

According to preliminary tests it really does so. Thus, I take all the stars of your catalogue from $5^h\ 30^m$ to $6^h\ 30^m$ and assume the meridian through each star to be the line through the sun's apex (a rather violent assumption). There are 53 stars. Of these, 7 are moving *towards* the apex. These I reject and the 7 that are moving most rapidly *away* to balance the first. I then find for each star the ratio of motion in *arc* of a great circle perpendicular to the meridian to its motion in the meridian. I divide them in the stars brighter than 6 and fainter than 6 mag. The lists are nearly equal. From each list I reject 3 sporadic very high values of the ratio, and the three lowest values to balance them. For the rest I find the mean ratio 0.92 for the bright stars and 0.68 for the faint ones. The difference is, no doubt, excessive.

I try the same for 18^h. But here the stars do not happen to be so favorable. The same treatment gives

for 10 bright stars 1.12
for 6 faint stars 0.91

All these results are favorable to the hypothesis; and can only be otherwise explained by *four* different suppositions. The argument appears to me strong enough to call for the *closest examination.*

The hypothesis is capable of being tested in *many ways.* It seems to me that an exhaustive discussion of it is called for.

The discovery that space has a curvature would be more than a striking one; it would be epoch-making. It would do more than anything to break up the belief in the immutable character of mechanical law, and would thus lead to a conception of the universe in which mechanical law should not be the head and centre of the whole.

It would contribute to the improving respect paid to American science, were this made out here. I should like to have a whack at it myself; and as I have found out what I have, I think I am entitled to that whack.

A month or six weeks' work might show how the thing was promising. But to discuss the matter as it should be discussed, from six to nine months would not be too long. The question is, can some appropriation be made, or some millionaire be found, to pay \$3000 for this, \$2000 for six months of my work and \$1000 for an assistant? Will you and other men, say Langley, King, Powell, Rood, John Fiske, be willing to express the opinion that it is a piece of work most desirable to have done?

In my mind, this is part of a general theory of the universe, of which I have traced many consequences, —some true and others undiscovered,—and of which many more can be deduced; and with one striking success, I trust there would be little difficulty in getting other deductions tested. It is certain that the theory if true is of great moment.

What interest would you take in the matter?

Very faithfully
C. S. Peirce

I wonder whether a professorship of logic in Stanford's University would be beyond hope for me.

My dear Newcomb:

Since writing you I have taken 63 faint stars from your catalogue and as near each as I could a bright one and for these have calculated the proper motions away from the apex and in the perpendicular direction. The result is most decidedly the reverse of what I wished; and this shows itself in several ways. I enclose some numbers.

My computations are without check; but errors to affect the result are impossible, I think. The apex was taken at $\alpha = 270° \ \delta = +30°$ for 1850.

Yours very faithfully,
C. S. Peirce

It still seems to me the subject should be pursued.
Milford Pa 1891 Dec. 21

Milford Pa 1891 Dec. 21

My dear Newcomb:

One thing is clear. It is that those figures I sent you lend no support whatever to the idea of a positive curvature, but are rather against it. The striking non decrement of the *peculiar motion* with the brightness needs to be investigated. But the formula for the surface of the sphere and for the peculiar motion equally involve $\sin h(r/2k)$ or $\sin(r/2k)$.

Very faithfully
C. S. Peirce

I have given a whole day to this. I cannot any longer afford the luxury of unremunerative work but very little.

Milford 1891 Dec. 23

My dear Newcomb:

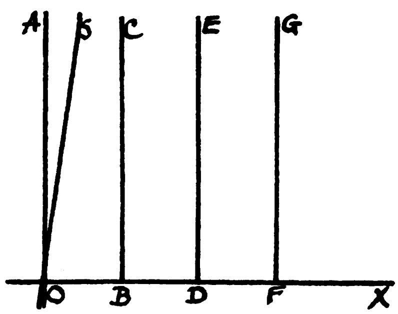
I have for the present given up the idea that anything can be concluded with considerable probability concerning the curvature of space. The best argument I could make was as follows. By *long* motion, I mean the component of proper motion directly away from the apex of the sun's motion. By *cross* motion the motion at right angles to that. Now I found the cross motion and brightness had remarkably little to do with one another. Separating the stars (I mean the 126 I wrote to you of) into two equal sets according to the magnitude of either of these quantities, I find the other quantity nearly equal for both sets. In point of fact, the intrinsically brighter stars have in the mean probably a greater mass and therefore by the law of action and reaction a smaller absolute motion. Consequently, I said, the geometric mean of the distance as deduced from the brightness and from the cross motion will probably be remarkably near the . . . .

incomplete

New York 7 West 43 St.

My dear Newcomb

The following question recently put to me seems to involve fundamental points relating to infinity. I should like to see how you would answer it.



Upon the straight line OX erect an infinite series of equidistant perpendiculars OA, BC, DE, FG, etc. and draw the oblique line OS. Then the space included between AOX and the circle at infinity bears some finite ratio to the space included in AOS and the circle at infinity. But it bears an infinite ratio to the space included between two successive perpendiculars, the base line, and the circle at infinity. Hence the diagonal OS must cross all the perpendiculars, contrary to Lobatchewsky. Where is the fallacy?

Yours very truly,
C. S. Peirce

Two of these letters from Peirce drew from Newcomb a reply[46] which, in its discussion of the possible curvature of space, anticipates the subject of his Presidential address before the American Mathematical Society on December 29, 1897.[47] That address was entitled "The Philosophy of Hyper-Space." Although it cannot be reproduced *in toto* here, a few excerpts will serve to clarify Newcomb's position and to highlight his scientific conservatism in refusing to entertain a hypothesis which had not yet come completely unscathed through the acid test of experiment.

His address reads, in part,

I cannot but fear that some confusion on this subject is caused by the tendency among both geometers and psychologists to talk of space as an entity in itself. . . . For us the limits of space are simply the limits to which we can suppose a body to move. Hence when space itself is spoken of as having possible curvatures, hills and hollows, it seems to me that this should be regarded only as a curvature, if I may use the term, of the laws of position of material bodies in space. Clifford has set forth, with acuteness and great plausibility, that the minute spaces occupied by the ultimate atoms of matter may, in this respect, have properties different from the larger space which alone makes itself known to our conceptions. If so, we should only regard this as expressive of some different law of motion, or, since motion is only a change of position, of some different law of position among the molecules of bodies.

Newcomb continues with the observation that "This consideration leads us to a possible form of space relations distinct from those of our Euclidean geometry, and from the hypothesis of space of more than three dimensions. I refer to what is commonly known as 'curved space.'" He speaks of the two independent substitutes which had now been made for the Euclidean axiom and the need of testing any curved space hypothesis by experience, and of the narrow limits of the two extremes of the earth's orbit within which "the measures of stellar parallax give no indication that the sum of the angles of a triangle in stellar space differs from two right angles." Moreover, he adds,

The wise man is one who admits an infinity of possibilities outside the range of his experience, but

[46] *Ibid.*

[47] *Science*, n.s., **7**: 1–7, Jan. 7, 1898. See also address delivered at meeting of the N. Y. Math. Society, Dec. 28, 1893, (*N. Y. Math. Soc. Bull.*, 95–107), entitled: Modern mathematical thought.

who in considering actualities is not decoyed by the temptation to strain the facts of experience in order to make them accord with glittering possibilities. . . . We are justified by experience in saying that the space relations which we gather from observation around us are valid for the greatest distances which separate us from the most distant stars. We have no right to extend the conclusion further than this. We must leave it to our posterity to determine whether, in either way, the hypothesis of hyperspace can be used as an explanation of observed phenomena.

Newcomb's reply to Peirce now follows.

Nautical Almanac Office

Bureau of Equipment and Recruiting
Navy Department
Washington Dec. 24, 1891.

My dear Peirce:

I received your two letters, each in due time, but am so crowded with work that I cannot give the subject the study which it deserves. I can therefore only refer to the different points in a general way.

First, does not the fact that all recent determinations of parallax are relative, prevent us drawing any conclusion as to a limit of ultimate parallax? It seems to me it does. That is to say the relative parallaxes of distant stars will converge towards zero on either hypothesis.

Second, in drawing conclusions from statistics of stellar magnitudes, the number of disposable hypotheses seems to me so great that we can scarcely test them even on the supposition of homaloidal space; much less, then, can we draw a conclusion as to the curvature of space. The fact is, it seems to me doubtful whether we can derive any law of relation between an absolute magnitude of a star and the number of stars having that magnitude.

Third, the proposition that the proper motions of faint stars are not much smaller than those of bright ones, seems to me not established, except in this sense; that, given a proper motion of two or three seconds or more in a century, the number of stars having it range through a wide degree of brightness. But you would thus exhaust the bright stars much quicker than you would the faint ones.

As to getting a grant of money for the purpose you mention, it seems to me the difficulties are insuperable. In the first place, the task of getting the scientific world to accept any proof now possible that space is not homoloidal, is hopeless, and you could have no other satisfaction than that of doing a work for posterity. In the next place, it is, I believe, unusual if not unprecedented, to pay an investigator to do a work of his own out of trust funds for the advancement of science, at least among us. I do not know where to look for funds to do this with.

As for the Stanford University, I have never been in any way consulted respecting it, and in fact know nothing about it, except what I have seen in print. I do not therefore feel able to do anything in that direction.

Yours very truly,
Simon Newcomb

Prof. Chas. S. Peirce
Milford
Penna.

Newcomb's extreme conservatism with regard to mathematical matters in the first of the following letters[48] is very surprising. Peirce was, without doubt, the more daring intellectual of the two. For example, he later corresponded enthusiastically with Langley on the development of the aeroplane[49] while Newcomb believed not at all in its potential, practical value.

Discussing the power of the "infinitesimal" approach to the calculus[50] Peirce gives an illustration from the work of Fermat,[51] and concludes with the sentence, "The method of indivisibles had recognized that infinitely large numbers may have definite ratios, so that the division is applicable to them." In a footnote to this sentence, Peirce remarks that "Newcomb errs in saying (Johnson's *Cyclopedia*, 1894, IV, 576) this method is 'medieval,' and his description of it is not very characteristic. He is also wrong (Funk's *Dictionary*, *indivisible*) in calling it an application of the method of limits."

The second and third letters[52] are self-explanatory.

Nautical Almanac Office
Navy Dept.—Wash. D. C.
Mar. 9th. 1892

My dear Peirce:

Your last letter seems decisive in favor of a proposition which I have often been inclined to maintain, to wit, that all philosophical and logical discussion is useless. If there is any one question which illustrates the correctness of the doctrine of infinities, always maintained by me, it is the very one suggested by the demonstration you and Halstead sent me. I have always held that infinity,

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[48] Peirce Collection, Widener Library. Box V B2b.

[49] Peirce-Langley Correspondence at the Smithsonian Institution.

[50] *The collected papers of Charles S. Peirce*, 4.151.

[51] Peirce could have referred to his father's approach to the derivative in B. Peirce's book entitled *An elementary treatise on curves, functions and forces.*

[52] Peirce Collection. Widener Library. Box VB2b.

considered in itself, could not be treated as a mathematical quantity, and that it is pure nonsense to talk about one infinity being greater or less than another. The ground for this view I think I mentioned in our correspondence a year ago; the very meaning of the word infinity is something without bounds. But we can compare two magnitudes only by comparing their bounds. Therefore I say the reasoning in question is baseless. What more can I say?

Yours very truly,
S. Newcomb

Mr. Charles S. Peirce
The Century Club
New York, N. Y.

Washington, D. C.
April 23, 1895

My dear Peirce:

I was puzzled by something in the Century Dictionary, which can, I think, be explained by no one but yourself. I refer to the definition of an oddly odd number. As no odd number can have any except odd factors, I do not see what the definition amounts to. It is I know a very old one, but did it originate in anything but a blunder of some ancient editor?

Yours very sincerely
S. Newcomb

Mr. Charles S. Peirce

Nautical Almanac Office,
Navy Department
Washington May 11, 1895.

My dear Peirce:

The enclosed paper has been offered to the Amer. Journal of Math. Before deciding what to do with it I would be much pleased to have you examine it, and point out any unsoundness that you may find. I have not yet looked at it carefully, as I can better do so after having your examination of the subject before me.

You may address your remarks to Dr. Craig, who is now responsible editor, especially as I may be away when they arrive.

Yours very truly,
S. Newcomb

Professor C. S. Peirce
Milford, Pa.

Thanks for your reply to
my question about "oddly odd."

The mathematical content of a number of the letters already quoted reflects Peirce's need for tenacity in the pursuit of a clarification of the meaning of each term. For the mathematical concepts of infinity, continuity, and probability as well, were to be found among the deepest foundation stones of his system of philosophy.

During the period in which the next letter[53] was penned, Peirce was preparing the lectures on the history of science which he delivered at the Lowell Institute that year. His familiarity with the old star names was to be expected after his notable work two decades earlier in the preparation of the historical portions of the *Photometric Researches.*

Milford Pa. 1892 July 28

Simon Newcomb LL.D.
Sup't. Nautical Almanac
Dear Sir:

In observing for time, I find the old star-names, where they exist more convenient than Bayer's designations. No doubt, many other observers would be of my mind. I suggest these names be inserted in the Almanac list, or at least, the following of them.

α Andromedae	Alpheratz
β Cassiepeae	Chaaph
α "	Shedir
β Ceti	Diphda
β Andromedae	Mirach

I do not think the second word in the name of a constellation should begin with a capital.

Yours respectfully
C. S. Peirce

(Only five of the ninety-eight names given by Peirce are listed here)

Without academic or government affiliation after 1891, Peirce decided to bring his materials together for publication in a set of twelve volumes to be entitled "The Principles of Philosophy: Or, Logic, Physics, and Psychics, considered as a unity, in the Light of the Nineteenth Century." A prospectus[54] was prepared by Peirce and subscriptions at two dollars and fifty cents per volume were solicited in advance. Peirce's synopsis reads as follows:

Vol. I—Review of the Leading Ideas of the Nineteenth Century.
Vol. 11–The Theory of Demonstrative Reasoning.
Vol. III—The Philosophy of Probability.
Vol. IV—Plato's World: An Elucidation of the Ideas of Modern Mathematics.
Vol. V—Scientific Metaphysics.

[53] Peirce Collection. Widener Library. Draft. Box VB2b.

[54] There is an extant copy of the synopsis in the Harvard University Archives section of the Widener Memorial building. The writer is indebted to Mr. Robert Haynes, Assistant Librarian, for securing permission to publish it.

Vol. VI—Soul and Body.
Vol. VII—Evolutionary Chemistry. (The title may probably be changed.)
Vol. VIII—Continuity in the Psychical and Moral Sciences.
Vol. IX—Studies in Comparative Biography.
Vol. X—The Regeneration of the Church.
Vol. XI—A Philosophical Encyclopedia.
Vol. XII—*Index* raisonné of *Ideas and Words.*

Peirce could not find a publisher to support the project. Henry Holt rejected it in a letter dated December 2, 1893.[55]

In the letter[55a] to Peirce about the proposed volumes, Newcomb's remark about "inverse probability" is surprising since he had written papers on the theory of probabilities for Runkle's *Mathematical Monthly* in the period 1859–1861.

Washington D. C.
Jan. 3rd, 1894

Dear Sir:

I am persuaded that whatever you might write on the subject of scientific philosophy would be provocative of thought and discussion, and therefore interesting, whether one accepted your conclusions or not. You may therefore put me down as a subscriber to your proposed volumes.

Your 2nd, 3rd, and 4th vols. are those which I should think would have the most scientific value, and which I would therefore rather see come out first.

I am curious to know what the doctrine of inverse probabilities is, as I see you propose to refute it.

I am sorry to see that you repeat the implication that somebody holds the dogma that we cannot reason mathematically about infinity. That we cannot correctly reason about infinity as if it were a magnitude is a proposition which I think no one ought to dispute; but if you do dispute it, I am sure you, as a logician, ought to put the proposition into the shape in which your opponents uphold it.

I am sorry to say that you greatly overestimate the value of any expression from me on your subject. My experience leads me to believe that people have very little confidence in my views on subjects outside of mathematics and astronomy. The general subject of the greater number of your volumes is one on which people already have their minds made up.

I could make a number of criticisms both on the expressions in your circular, and the descriptions of your volumes, but as it seems to be printed in its final form it is not worth while to do so.

Yours very sincerely,
S. Newcomb.

Mr. C. S. Peirce
Milford
Penna

The Thomas Bayes mentioned in the first of the next two letters[56] developed a basic theorem in the subject of inverse probabilities which was published posthumously in the *London Philosophical Transactions*, vols. 53 and 54 for the years 1763 and 1764. According to R. A. Fischer,[57] Bayes was the first to use mathematical probability inductively, "that is, for arguing from the particular to the general, or from the sample to the population." With his abiding interest in all possible forms of logical inference and his own logic grounded in the trilogy of inductive, deductive, and abductive inference, Peirce would of necessity be deeply concerned with the validity of conclusions so drawn.

Washington, D. C.
January 16, 1894.

My dear Peirce:

There seems no immediate occasion for me to do more than merely acknowledge yours of Jan. 6. I quite coincide with your expression of the spirit in which you treat the subject, although I fear my philosophy would diverge a good deal from yours. Your prospectus is well fitted to excite curiosity, and yet I fancy that the last paragraph, and possibly several of the preceding paragraphs, will not attract an audience. I do not mean by this to imply that there is anything objectionable or open to criticism in the paragraphs above referred to. But experience has taught me that there are some subjects on which nobody wants to be really informed.

As I do not know exactly what Bayes' theorem is, I am still in the dark as to your objection to inverse probabilities.

In your letter you say, "You begin by finding fault with a sentence quoted verbatim from yourself." I am curious to know what that sentence is, and where it was uttered.

Yours very truly,
Simon Newcomb

Mr. C. S. Peirce
Milford, Penna

Washington, D. C.
February 7, 1894.

My dear Peirce:—

Excuse my inattention to your letters. I am overrun with work, the result of having ten years' computations of about six or eight computors to get

[55] Peirce Collection. Publishers' Correspondence. Widener Library.
[55a] Peirce Collection. Widener Library. Box VB2b.
[56] Peirce Collection. Widener Library. Box V B2b.
[57] *Statistical methods for research workers*, Edinburgh, 1938. Also E. T. Bell, *Development of mathematics.*

into shape and weave in with astronomical theory. But for this you would find me paying much more attention to your projects.

I am greatly obliged for your explanation about the inverse probabilities. The problem you allude to is of course one in which an element is lacking. But your programme gave the idea that you meant to subvert one of the best grounded theories of mathematics.

Your second letter puzzles me. In the first place if you do not agree to my conclusions by all means say so in the plainest English you can use. I would a good deal rather be killed by a rattling attack than "todgeschweigen," as the Germans say. But I am utterly unconscious of having said that there has been no great advance in mathematics since Euler and La Grange. On the contrary I thought my whole address was devoted to showing the spirit of that advance.

Yours very truly,
S. Newcomb.

One of Peirce's meager sources of income during these years was as a reviewer of books for Garrison's *Nation*. The passage in the review referred to in the next letter[58] is the opening paragraph which reads as follows:

Many good people fancy that the advances of mathematics, like those of jurisprudence, become manifest only when the state of things in one generation is compared with that in another; and that they are merely in the nature of extensions of old methods to new cases. . . . We are speaking of pure mathematics, not celestial mechanics.

Dear Newcomb,

Some months ago I wrote for the *Nation* a notice of some books on the Theory of Functions. The *Nation* is not exactly ravenously hungry for that sort of thing and the proofs only reached me this week. I then saw that you might fancy I meant to attack your address, which of course I had not seen when I wrote what I did. I added a sentence to soften the contradiction. But really I think you are wrong. You might as well say that Chemistry has made no progress since Geber; because we can no more transmute the metals now than we could then.

You must take mathematics to mean pure mathematics and conceive its problems as they are conceived today. On that point of view, the advance of mathematics seems to me wonderfully rapid and accelerating.

However, my purpose is not to *tackle* you but only to explain that what may look like a reply to you was really written long before your address.

Very truly
C. S. Peirce

Their overlap in interests would account for many of the mutual scientific associations of Peirce and Newcomb. Among the most eminent of these was the British astronomer Sir Joseph Norman Lockyer. He had headed the British Eclipse Expedition to the Mediterranean in 1870 and was closely associated with the American team in Sicily. Peirce's first wife, Melusina Fay, was a working member of the American group and in her official report to the Coast Survey she refers to Lockyer's general helpfulness.[59]

Peirce himself often expressed his great respect for Lockyer's work and the quality of his reasoning. In later years he reviewed *Lockyer's Inorganic Evolution as Studied by Spectrum Analysis* and paid him the following tribute[60] at that time. He said,

That the relations among the chemical elements are to be explained by some sort of evolutionary process is the only idea we can at present entertain. We ought to begin then, with trying how the hypothesis of the simplest kind of evolution that could answer the purpose will fit the facts, and adhere to that until it is refuted. Lockyer's seems to be that simplest hypothesis. . . .

In the Newcomb letter[61] which follows, Peirce's approach to many of the problems in the history of science stands revealed. Using the tools of the modern astronomer he checked mathematically the assumptions on which Lockyer had based his historical theories in the *Dawn of Astronomy*. Peirce's review[62] was based on these computations and exposed Lockyer's incompetence in this field. He questioned Lockyer's claim that "Egyptian temples were generally oriented to the risings and settings of stars." Some of the argument against the hypothesis is given in the second letter[63] following.

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[58] Newcomb Collection. Library of Congress. The books reviewed were *Theory of functions of a complex variable* by Forsyth; *A treatise on the theory of functions* by Harkness and Morley; and *Traité d'analyse* by E. Picard, *The Nation* **58** (1498): 197, Mar. 15, 1894.

[59] *U. S. Coast and Survey expedition to the Mediterranean for observing the eclipse of 1870.* Appendix No. 16 of U. S. C. S. Report for 1870 (Dec. 22, 1870).

[60] *The Nation* **70** (1819), May 10, 1900.

[61] Peirce Collection. Widener Library. Box V B26.

[62] *The Nation* **58** (1500): 234, March 29, 1894.

[63] Newcomb Collection, Library of Congress. Peirce's approach to problems in the history of science was emphasized by the writer in a paper which will appear in the *Proceedings* of the Eighth International Congress for the History of Science held at Florence, Italy, September,

Nautical Almanac Office, Naval OB
Georgetown Heights, D. C. Feb 13, 1894

Dear Mr. Peirce:

Mr. Garrison desires me to send you certain data respecting the obliquity of the ecliptic and the positions of the fixed stars for epochs of ten thousand years back, which will be within a few minutes of arc. I do not know of any formulae which will in themselves serve your purpose. Of course the ordinary formulae for precession do not apply, and the data for the rigorous trigonometric reduction have not, so far as I am aware, ever been carefully computed. So the best I can do is to send you the data for making the computation.

Having the path of the pole of the ecliptic during the past 10000 years, as shown by the numbers on the diagram, the motion of the pole of the earth can be computed by mechanical quadratures, within one or two minutes of arc. It would seem from the diagram that the obliquity has been diminishing during almost the entire period, or, more exactly, about eight thousand years, so that it was probably nearly one degree greater ten thousand years ago than it is now. With the former position of the pole the places of the stars can be computed by the trigonometric reduction.

I am very glad you are ready to take up that book of Lockyer's. If he has really produced a sound scientific work, by all means let Sir Joseph have the credit of it. But if it is of a piece with much of his other work, let him be shown up.

Yours very sincerely,
S. Newcomb

Peirce
Milford Penna

Milford Pa 1894 Feb. 20

My dear Newcomb:

I enclose you a circular showing what use I am putting your letter to.

In regard to Lockyer's Theory, I think, in view of its *possible* importance if true, in setting early Egyptian chronology, which I think has a bearing on our whole conception of man's development, that he has made out sufficient case to warrant the expenditure of time and money to collect further facts. At the same time, I think there is much against the theory.

As for the circular zodiac of Denderat, it is possible to locate upon it with some probable accuracy,—aside from any *measurements* made upon it,—the positions of about 40 stars. Then measuring the places of those stars, we find they agree in R. A. with a polar projection made about 700 B. C. and then *radii vectores* will not suit any projection I can think of but one from the vertex of the cone tangent to the sphere on the circle of perpetual occultation at 35° to 40°. Nothing in Egypt will answer at all. Now 719 B. C. Egypt was defeated in a great battle by Sargon whose capital was Nineveh, lat 36°, and about that time Assyria began to be more influential intellectually in Egypt. I am therefore inclined strongly to think that the original from which after many copies and changes this "zodiac," or rather planisphere was taken was made on such a projection (or some projection practically equivalent) at Nineveh about that time. For if made in Egypt how could it fail to show Canopus, and in short all stars, (but one, apparently) of lower declination than −48° which at Denderat have an altitude at culmination of 16°? Now since the planisphere was put up about A. D. 14, it follows that the Egyptians had made no observations of the stars to speak of for 700 years; and that quite accords with my opinion of their scientific nullity. The idea of expecting to find such a people orienting their temples with any sufficient accuracy to settle chronology, I think is difficult. Doubtless if we could find temples of the pyramid epoch, or thereabout, that could be shown by independent evidence to be oriented to stars, then the case might be very different. Perhaps Lockyer may do so in the future. I should be glad to see him go to work. So far his evidence is slender.

Very truly
C. S. Peirce

Peirce made a last great effort in 1899 to return to the Coast Survey as Inspector, Bureau of Weights and Measures. His earlier Coast Survey work had given him a particular competence for this position as the first of the next two letters[64] shows. He was not successful, however, in his quest for the job. Among his sponsors were Asaph Hall, Seth Chandler, and George A. Plimpton. The second letter[65] was apparently written by Newcomb while on one of his favorite walking trips in Switzerland.

Milford Pa 1899 June 10

My dear Newcomb:

I am an applicant for Civil Service Examination for the place of "Inspector of Standards," Office of Weights and Measures, U.S.C. and G. Survey. Salary $3000, the same I formerly had. When I shall have been accepted as a candidate for examination, the first question put to me, I am informed, will be, Name five persons who will answer questions concerning your "scientific administrative qualifica-

1956. A more general statement about his activities in that area was made in a report on his work which appeared in *Year Book Amer. Philos. Soc.* for 1954: 353–358, 1955.

[64] *Ibid.*

[65] Peirce Collection. Widener Archives. Box V b5.

tions and experience." I naturally apply to my colleagues on the Academy Committee on Weights and Measures of which you are a member.

Allow me to remind you that you have known about the following work of mine pertinent to the question:

1st, that in 1874 I first undertook to "weigh the earth" by setting up a balance over a deep shaft and comparing the weight of 10 kilos above and below. I did not succeed because I could only work when the machinery at the Hoosac tunnel was stopped and that could only be stopped for two hours on successive Sundays. Now there were difficulties about oscillations of the wire and about moisture which could not be overcome in that short time. But the method, which I first proposed, was soon after applied successfully by others.
2nd, that my determinations of the absolute value of gravity were superior to those which immediately preceded them; if not to all previous ones.
3rd, that that work involved my bringing to America the first authoritative line-metre.
4th, that my work of comparing the length of a bar with the mean ruling of a glitter-plate, with a view to obtaining a check on secular changes in the lengths of bars, had many merits. My measurements of the deviation of the ray were more accurate perhaps than any measurements of a large angle ever made. The peculiar comparator I invented and used enabled me to build up from a double centimeter with a probable error of less than a millionth part, which was quite a feat. True, Rowland found a break in my glitter which vitiated my value of the wave length of light. But that does not, I believe, affect my *main purpose*, as long as that plate can be used again.
5th, that I proposed and used the method of two reversible pendulums in order to compare the lengths of the yard and the metre, a new idea.
6th, that I was for some months in charge of the Office of Weights and Measures under Hilgard as Superintendent; and only left because his physical condition was such as to cause me embarrassment which I thought required me to quit Washington.

May I name you as one of the five persons who will answer questions as desired, and may I hope that those answers will be favorable?

Yours very truly
C. S. Peirce

Maloja, Switzerland
July 21, 1899

My dear Peirce:

When your letter of June 10 reached me I was at a mountain recess where correspondence was very difficult. So I have postponed answering it and other letters till I could settle down.

I do not see how I could say anything of real value about your "scientific administrative qualifications & experience." Your work was done for the Coast Survey and its records are there, and tell their own story. The Superintendent has at his command all the data for reaching a conclusion and nothing that I could say could add to his knowledge of the subject. It is very clear to me that the persons to whom the C. C. Comm. should apply are those who have made a more careful study than I have of your work or have had occasion to examine it. These are but to be found among the C. S. and other pendulum experts.

Sincerely yours,
S. Newcomb

Early in 1904, Garrison[66] requested Peirce to give priority in the selection of his reviewing assignments to Newcomb's *Reminiscences of an Astronomer*. Peirce's review[67] was highly complimentary to Newcomb as a person and as a scientist. Garrison observed in a letter to Peirce dated March 10, 1904, that it would please Newcomb.

Apparently Peirce had included in the original version of the review an extended description of the succession to the seat to which Newcomb had been elected in the French Academy of Sciences in 1895.[68] The next letter[69] reveals one possible source of Peirce's information regarding it. Since the original review was too long, Garrison deleted the discussion of the succession and, at Peirce's suggestion, submitted it to the *Evening Post*. The *Post* printed that part of the "disembowelled Newcomb" as the leading article in the Saturday Supplement on March 5 of the same year.

Peirce called the article "French Academy of Sciences" and in it he traced the history of the eight seats of the *associés étrangers* created in 1699. Since five of these places had not existed previously, they were filled at the time by the election of Hartsoeker, Newton, Jacob and John Bernoulli, and Viviani, all more or less mathematicians. Peirce mentions the fact that Franklin, Rumford, and Newcomb were the only Americans honored by election and then, man by man, shows the succession of Newcomb to New-

[66] *Ibid.* Garrison file of letters. Jan. 8.
[67] *The Nation* **78** (2021): 237, March 24, 1904.
[68] Peirce knew something about the Academy at first hand. He had lectured there on June 14, 1880 on the value of gravity.
[69] Newcomb Collection. Library of Congress.

ton's seat. Indeed, this must have pleased Newcomb very much.

Milford Pa 1904 Jan. 15.

My dear Newcomb:

Living here so far away from libraries, when I do not know what book to borrow in order to get a given piece of information, I have to ask the good offices of some person up in the subject.

Now when you received the extraordinary honor of election as Foreign Associate of the French Academy of Sciences, I think you must have had the curiosity to know, 1st, when the rule that there should be five was made and what previous number there had been, if any; and the whole history of the institution of five members. 2nd, I would like to see the complete list of all there ever have been down to today. 3rd any curious anecdotes connected with the matter.

I remember a book by one of the de Candolles which I think gave the names of most of them. But I may be mistaken and at any rate the book is thirty years old.

If you know of such a book, I wish you would kindly write to Dr. Herbert Putnam and tell him that is the book I am after. I will write to him that I would like him to send me De Candolle[']s book and also a book or book[s] for the title or titles of which I have asked you to send to him. Then you will have no responsibility in the matter but will merely furnish a piece of information to me to whom everything relating to the history of science is interesting.

I enclose some pages of a Syllabus that may possibly interest you. It certainly would had I been able to print the whole. But though I limited the copies to 100, the money gave out when the printer got so far. The whole would have been about a hundred and fifty such pages. Logic is a subject which does not amount to a row of pins unless it is treated systematically and it is necessary to push through a mass of stuff like the greater part of what I send and then through matter of extreme subtlety and difficulty of comprehension of another kind, in the way of logical analysis, before one can treat in a solid way of the part of the matter that is generally interesting. I have a great quantity ready for the press which I think, more keenly the more experience I gain, is of really great importance. But there is no hope of its ever being printed. It seems a fearful piece of egotism to study so deeply for myself alone. But it is not my fault.

Very truly
C. S. Peirce

Among the computers on Newcomb's staff in the Nautical Almanac Office, none was so highly prized by Newcomb as the mathematician G. W. Hill. Newcomb praised his work on every possible occasion and paid particular tribute to him in his *Reminiscences of an Astronomer*[70] with a statement opening with the words, "Perhaps the most eminent and interesting man associated with me during this period was Mr. George W. Hill, who will easily rank as the greatest master of mathematical astronomy during the last quarter of the nineteenth century."

Peirce wrote a note on "Mr. G. W. Hill's Moon Theory" in the *Nation*[71] on October 19, 1905. He lauded Hill's achievements in celestial mechanics. He speaks of Hill's "staggering conception of an infinite determinant" and of his success in "virtually solving a differential equation of an infinite order." In doing so Hill probably did not "perceive that he was applying Baconian reasoning to mathematics." Peirce speaks of Vol. I of the *Collected Mathematical Works of George William Hill* as bringing the "oldtime glow of exultant American feeling."

By October 17, 1907, in a review[72] of Vol. IV of the *Collected Works*, Peirce speaks less glowingly of Hill's work and begins the second paragraph with the statement that

> This science . . . is reduced to an art of performing excessively intricate calculations. It must be a peculiar mind that can devote a lifetime to it; and with less devotion there is no chance of being able to improve it.

This review brought a fiery retort from Newcomb in a letter written on October 17 to the Editor of the *Nation* and which appeared in the issue of October 31, 1907.[73] Newcomb goes on to say,

> Now if you will slightly change your wording, and say that through the labors of a series of investigators from the time of Newton to that of Hill the theory in question is being reduced to an art of performing intricate calculations, you will hit the truth. What gives significance to the work of Hill and those in the same field is not their patience in performing calculations, but their ability to show how it is possible, by calculations within the power of one man, to reach results which would have required the labor of many lives if the methods had not been invented. Any good computer, under capable supervision, can make the intricate calculations. It is the method that costs.

[70] Pp. 218–223.
[71] **81** (2103): 321.
[72] **85** (2207): 355.
[73] **85** (2209): 396.

Appended to the above is a note which reads as follows:

By calculations, we did not mean numerical computations. Professor Newcomb expresses, as his own dictum, what we intended to say. We have already done justice to Dr. Hill's mathematical invention; but there is little of that in the fourth volume, which we have had under examination.

The Reviewer.

The following letter[74] needs no further comment.

P. O. Milford Pa. 1907 Oct. 31

My dear Newcomb:

The *Nation*, which comes to me today, brings to my mind your anger at my notice of the fourth volume of Hill's works. I am sure I like you no less for being angered and think it very natural. At the same time,—well, it is the facts of the situation and my stating those facts which irritate you. You cannot put your finger upon anything I said that is not perfectly true, and this is shown by your saying that *if* I *had* said so and so, I should have been right. Now that is just what I did say in substance. That the science of celestial mechanics by its own perfectionment is now reduced to calculation. Of course, I did not mean numerical computation, but just that sort of art that there is in Delaunay's method. It is not a method for finding out any substantially new truth, but is a method for calculating a result according to well known principles.

I never cast any slur on the men who do this sort of thing. I said they must have peculiar minds; but in some measure, this is true of any specialist. That you and Hill and other theoretical astronomers find in the afternoon of life that their own successes have rendered their science uninteresting to most people,—even to most mathematicians is distressing; but it is a fact. Meantime, for those who keep on I for one have an especial admiration, and the less interesting from any broad standpoint their work has been rendered, the more they deserve to be applauded; especially, since there is ground for hope that something important may eventually come of the work.

In short, if you will reread what I said coolly, you will see, that I was neither mistaken nor was I wanting in esteem for the theoretical astronomers, and that that which my article contained that was disagreeable was due to my expressing truths that may be unpleasant to a man like you, but are truths just the same for all that.

In short you read into my article a *tone* which really was not there. Your position is such that men do not like to tell you to your face that you are wrong. I don't like to do so myself; though it is not because offending you would inconvenience me otherwise than by sadness. You may be sure that nobody outside your group entertains a greater intelligent admiration for Hill and you than I do.

C. S. Peirce

The last letter[75] in this collection emphasizes the psychological as well as the optical factors of which the astronomer must be cognizant when drawing conclusions from telescopic observations. The problem is as old as the telescope itself and became a major issue in the controversies following the work of Lowell[76] and of Schiaparelli on the canals of Mars.

Newcomb expressed his opinion[77] that, despite Lowell's investigations of these basic factors as they related to his researches on Mars, further research was necessary. He mentions especially the "psychology of vision, that branch of the subject which relates to accuracy of conception and estimate" and which is "an almost virgin field." Newcomb had devised certain experiments on visibility and visual interpretation which, in their approach to the problem, were quite different from those of Lowell. He did not question the subjective reality of the canal system but he felt that the proof of its objective reality would be incomplete until further research on the process of visual inference had been made.

These remarks were refuted point by point by Lowell in a later issue of the journal[78] which had publicized Newcomb's criticisms. Lowell refers to the results of investigations made by H. Dennis Taylor which were "entirely opposite in conclusion from what is supposed to exist by Professor Newcomb." He points to Newcomb's use of transmitted light rather than reflected light in his experiments and holds rigidly to his own original position.

A one-page "Note on Preceding Paper"[79] by Newcomb follows the Lowell article, and a one-page "Reply to Professor Newcomb's Note,"[80] gives Lowell the last thrust in this joust.

In the letter which follows, Peirce refers to the results of the psychological experiments he performed in collaboration with J. Jastrow and

[74] Newcomb Collection. Library of Congress.

[75] *Ibid.*

[76] Mars and its canals by Lowell, *Annals of the Lowell Observatory* **3**: 268–277.

[77] *Astrophysical Journal* **26** (1): 1–17, July, 1907.

[78] *Ibid.* **26** (3): 131–141, Oct., 1907.

[79] *Ibid.* **26** (3): 141, Oct., 1907.

[80] *Ibid.* **26** (3): 142, Oct., 1907.

which he reported in a paper entitled "On Small Differences in Sensation."[81]

P. O. Milford Pa 1908 Jan. 7

My dear Newcomb:

Needless to say that I have read your paper on the canals of Mars with great interest. For though I have always thought that granting the reality of what Lowell has observed it is far from proving the work of intelligent inhabitants, yet I have worked enough with a longish equatorial, that of Cambridge, for near 3 years constantly, and have worked on stellar photometry still more, and on other photometry and chromatics very much more and on allied psychological problems to have great interest in anything such a man as you puts forward on the subject.

I have no doubt that the utterance of these words of caution from a man of such authority will do good to Lowell himself, not to speak of such men as Morse and Story who on the basis of no more than a few weeks in Lowell's observatory, put forward opinions and arguments of no weight at all. Yet I do not think your investigation is up to your standard; and I will mention one or two points which seem to me weak, besides the general objection that it is all too narrow and does not bring light upon the psychological laws involved.

In the first place I note that you accept as *established* the dictum of Gustav Theodor Fechner that the least sensible ratio of light is 101/100. If you will look in volume III Mem. of the U. S. Nat. Acad. of Sci. you will find a paper by me and my then student in logic Joseph Jastrow devoted to the question whether there is or is not such a thing as a "Differenz-Schwelle" or least perceptible difference of sensation; and of course, our conclusions being negative, can only be founded on that weakest form of induction, which consists in inferring that a given phenomenon does not exist because it has not appeared in a certain run of experience,—a form of inference that though *per se* very weak we are obliged to resort to in almost or quite every science. One does not believe in ghosts, or in meteorites, because critical investigation has never found them.

Now you know that I am *au fait* at experimental psychology; and I have not an exalted opinion of psychologists as scientific observers. Jastrow and I began with sensations of pressure and for a reason I will shortly mention we ended there. At once, using such precautions as any astronomer would use in observing faint nebulas, without any practice we found that if there were any least perceptible ratio of pressure, it was twenty or thirty times nearer unity than the psychologists had made it to be. We afterward tried to do the same thing for light; but were stopped by the utter impossibility of getting a piece of Bristol board containing a square inch of uniform luminosity. No doubt this might have been overcome. But Jastrow and I were severally pressed with other work and we dropped the investigation—contenting ourselves with what we had done. I hope you will scrutinize the tables of comparisons and the precautions which were more careful and studied and elaborate than the memoir states.

I became perfectly satisfied by the run of the curve that it is merely a question of a sufficient *molition* (my term for a volition prescinded from all purposiveness) of direct attention, voluntary or *involuntary*, to enable a person to perceive the difference between any two feelings, or sensations, of different intensities or of different energies of excitation under the same internal conditions. Our tables will enable you to form your own opinion as to this. But whatever conclusion you come to, let me tell you that Jastrow without any previous training except about two months experimentation with me upon pressure sensations, as soon as he took up photometry got as his average perceptible ratio of luminosity (with no great exertion of attention, though not in the fashion of the psychologist,) about 301/300 as well as I remember now. Certainly far less than had been said. Now if this principle,—a very broad psychological principle it is, with a thousand important consequences,—be correct, there is a fundamental weakness in your work in that you give the reader no assurance that you were worked up to the pitch of attention that Lowell and his assistants without doubt have been.

Another small criticism, pointing the opposite way is that you admit without qualification that some canals there are; and for no better reason than that they have been photographed. But what proof is there that the leading causes of illusion in telescopic observation are absent in examining the doubtless nearly invisible lines on the photograph?

In the present indispensableness of large expenditures for astronomy we ought to give a little extra praise, to be understood as a tactful recognition of the force of *character* involved in the conquest of *needles'-eye* difficulty, to the work of a young man of great wealth who shows a real devotion to the science, beyond what we should to the young fellow who is presumably working for an assistant-professorship in some college; and we cannot help admiring Lowell's work. He and all should be made to feel that it is bound to advance astronomy and *all science* not a little, whether his observations turn out to be observations of the real Mars, or whether they turn out to be observations of illusions partly based on instrumental imperfections; in the former case infusing into human veins a new motive for pursuing astronomical study, and in the latter case enforcing the lesson that *percepts* are not by any means as Karl Pearson calls them, the "first impressions of sense," but are

[81] *National Academy of Science Memoirs* **3**: 75–83. Read on Oct. 17, 1884.

results of "Schluss-Verfahrungen" of our deeper lying consciousness, closely and in much detail, analogous to the different varieties of critical induction, yet having certain general characters of their own, which must be brought to light before we can make the best use of the finer kinds of observations.

I confess that your reasoning about the visibility of a line appears to me to involve a momentary forgetfulness of the fact that not merely what we call "illusions," that is, perceptual inferences that are refuted by others, but also *all* results of perception, have processes behind them which have all the characters of reasoning except that of being conscious and, thereby self-controlled, and thereby being *critical*. You seem to me tacitly to assume that the process of perceiving a line is considerably simpler than my observations have led me to surmise that it is, (and in some measure to test my surmise). However, I must not put much stress on that, since I have no leisure to expound my theory.

By the way, since Garrison's death, an important item of bread and butter has been almost wholly cut off from me by my no longer being invited to write for the *Nation*. There is a person on the staff who I believe thinks himself a very superior philosopher,—as most students of the subject severally rate themselves. It is very singular and seems to show that their methods of determining their valuations are at fault, since there is no room for half of them to be right. I thought it likely, too, that your letter about my remarks on Celestial Dynamics increased their indisposition to trust to me. The whole difference between us seems to me to have consisted in my using the word "calculation" in a well-established sense considerably broader than your interpretation of what I said. I meant that there was no present prospect of important general physical discoveries, such as those of Keppler and Newton flowing from the work (unless perhaps centuries hence) nor even of any mathematical theory comparable for instance with Laplace's functions in importance. If anything of the kind is to come from your study of the movement of *Mars*, *tant mieux*! Well, I only introduce the matter to say that if you are disposed to do me such a good turn, a word dropped in conversation with Lamont might do me and those to whom I may be of any use more service than you would think.

Very faithfully
C. S. Peirce

These letters are not only a valuable addition to scientific Americana of their period but serve also to deepen our acquaintance with two outstanding personalities of that epoch. That these men left an indelible imprint on the intellectual life of America cannot be denied. They came by their intellectual powers and earned personal distinction in such strikingly different ways that one is reminded again of the criterion by which Peirce would judge a man's greatness. It was formulated to read,

Who, for instance, shall we say are the great men of science? . . . Some hold that they are fashioned of the most ordinary clay, and that only their rearing and environment, conjoined with fortunate opportunities, make them what they are. The heaviest weight, intellectually, among these writers, maintains, on the other hand, that circumstances are as powerless to suppress the great man as they would be to subject a human being to a nation of dogs. But it was only the blundering Malvolio who got the notion that some are born great; the sentence of the astute Maria was: "Some are become great, some atcheeves greatnesse, and some have greatnesse thrust uppon em." . . . My opinion will, I fear, be set down by some intellectual men as foolishness, though it has not been lightly formed, nor without long years of experimentation—that the way to judge of whether a man was great or not is to put aside all analysis, to contemplate attentively his life and works, and then to look into one's heart and estimate the impression one finds to have been made. . . . The great man is the impressive personality; and the question whether he is great is a question of impression.[82]

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[82] The century's great men in science, *The Evening Post*, Jan. 12, 1901.