Writings of Charles S. Peirce

A CHRONOLOGICAL EDITION

Volume 4
1879–1884

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"Original Faculty of Philosophy" mural by Leon Kroll (1956), in Shriver Hall, The Johns Hopkins University. From left to right: A. Marshall Elliott, Charles S. Peirce, William Hand Browne, George S. Morris, Henry Wood, Henry A. Rowland, Basil L. Gildersleeve, Ira Remsen, H. Newell Martin, J. J. Sylvester, C. D. Morris, William K. Brooks, Simon Newcomb, Herbert Baxter Adams, G. Stanley Hall, George Huntington Williams, Paul Haupt. Courtesy of Ferdinand Hamburger, Jr. Archives of The Johns Hopkins University.

Writings of

CHARLES S. PEIRCE

A CHRONOLOGICAL EDITION

Volume 4
1879–1884

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Preface

Editions differ in what they select and how they arrange and edit their texts. Our selecting, arranging, and editing in the *Writings of Charles S. Peirce: A Chronological Edition* are guided by the belief that Peirce's writings are, as he said of Plato's, "worthy of being viewed as the record of the entire development of thought of a great thinker" and that the development of his thought is eminently worth studying; for Peirce contributed to an exceptionally wide range of disciplines—in mathematics, the natural and social sciences, and the humanities—while aiming always at eventual synthesis, with a primary focus in logic, more and more broadly conceived.

The need for a comprehensive, chronologically arranged edition of Peirce's writings began to be acutely felt after Murray Murphey's *The Development of Peirce's Philosophy* appeared in 1961. At the "Arisbe Conference" in Milford, Pennsylvania, in October 1973, some twenty-five Peirce scholars discussed the relative merits of several alternative plans for such an edition, and settled on a selected but strictly chronological one. Indiana University assumed responsibility for the preparation of the new edition in 1975, and the Peirce Edition Project was established at its Indianapolis campus, or Indiana University–Purdue University at Indianapolis. Supporting grants from the National Endowment for the Humanities and the National Science Foundation began in July 1976, and the Project got underway with a full-time staff of three. An Advisory Board and a group of Contributing Editors were appointed and, after a meeting with the former in November 1977, general policies and procedures were adopted. In the meantime, copies of most of Peirce's lifetime publications and of his manuscripts deposited in the Houghton Library of Harvard University had been acquired—and materials from other depositories were added later. Since 1984, the Project has had a full-time staff of six.

When work toward the new edition began in 1975, the only edition of Peirce's writings in more than one volume was the eight-volume *Collected Papers* (1931–35, 1958). But in 1976 there appeared the four volumes (in five) of *The New Elements of Mathematics.* By that time the first part of Peirce's *Contributions to THE NATION* had been published; parts 2, 3, and 4 followed in 1978, 1979, and 1987. In 1977 there appeared the *Complete Published Works,* a 149-microfiche edition accompanied by a printed *Comprehensive Bibliography* (revised and enlarged by 12 fiches in 1986). And in 1985, Carolyn Eisele published two volumes of *Historical Perspectives on Peirce's Logic of Science.* These are all valuable editions, but none is critical and none conveys a comprehensive sense of Peirce's entire work. His known writings, published and unpublished, would fill over a hundred volumes if all manuscript drafts and versions and the several thousand manuscript pages of discarded computations and scratch-sheet calculations were included. But any edition in fewer than sixty volumes might fairly be called "Selected Writings."

The present *critical* edition will consist of thirty volumes. It will include every philosophical and logical article that Peirce published during his lifetime; those scientific and mathematical articles that shed particular light on the development of his thought and remind us of the immediate scientific and mathematical background of the work he was doing in philosophy and logic; and those of his papers—no matter what their field or subject-matter—that are most important to an understanding of the development of his thought and of his work as a whole. The most distinctive feature of our edition is that Peirce's writings are arranged in a single chronological order: those he published as of their dates of publication (or oral presentation), those he did not publish as of their dates of composition. But to allow the reader to discern the degree of coherence and unity of Peirce's thought during a given period, every series of papers is presented complete and uninterrupted, as of the date of the first paper in the series. About one-half of the writings included in our edition will be from hitherto unpublished manuscripts. Even what is not new will often seem new by virtue of the fresh context provided for it by the chronological sequence. In all cases, even when we repeat what has appeared before, we have returned to the original manuscripts and publications and have edited them anew. We also include in each volume a few of Peirce's letters, published or unpublished, that are relevant to his work during the period. (Two volumes of correspondence are planned as supplements to the edition proper.) Except in the case of some long technical-scientific papers, we publish only complete selections in the text; related excerpts are given as notes in the editorial apparatus.

Recently a growing number of readers of Peirce have come to him from semiotics, the theory and study of signs, and they regard him as one of the founders of that discipline. From the beginning, Peirce conceived of logic as coming entirely within the scope of the general theory of signs, and all of his work in philosophical logic was done within that framework. At first he considered logic a branch of semeiotic (his preferred spelling), but he later distinguished between a narrow and a broad sense of logic; in the broad sense it was coextensive with semeiotic. Eventually he abandoned the narrow sense, and the comprehensive treatise on which he was working during the last decade of his life was entitled "A System of Logic, considered as Semeiotic."

Our edition will facilitate the tracing of this and of other developments of Peirce's thought, and it may yield answers to questions that have so far been difficult to pursue. Who were the thinkers whose writings Peirce studied most intensively, in what order, and at what stages of the development of his own thought? What were the questions with which he began, and what others did he take up and when? To what questions did his answers change, and what was the sequence of changes? When and to what extent were his philosophic views modified by his own original researches in mathematics and the sciences, and by the major scientific discoveries of his time? In each distinguishable period, to what degree did he bring his thought to systematic completeness? Did he have a single system from beginning to end, with only occasional internal adjustments? To encourage the pursuit of questions like these and to enable the reader to trace the whole development of Peirce's thought—and to trace that thought as articulated in critically edited and reliable texts: those are the primary goals of our edition.

Each volume contains several distinct sections. The largest and most important, the text of Peirce's writings, is preceded by a Chronology, which lists the most significant dates and events in his life and work, and by an Introduction, which provides the biographical and historical background for the writings. The editorial apparatus is continued, following the text, with Notes, a Bibliography of Peirce's References, and a Chronological List of every paper he is known to have written, whether published or not, during the period covered by the volume. The Introduction and Chronological List thus frame the writings that appear between them, and they provide a comprehensive sense of Peirce's work in mathematics, the sciences, philosophy and logic, and the other areas to which he contributed. Then follows the Essay on Editorial Method, which explains our editing policies and principles, and a section called Symbols, which defines all symbols and abbreviations used both in the text and in the Textual Apparatus. The latter provides, for each item in the text, a record of the textual decisions that have been made: a record consisting of (untitled) headnotes, Textual Notes, Emendations, Line-End Hyphenation, and Rejected Substantive Variants. (The last is derived from the historical collation list, which is prepared early in the editing process and which also contains variants in accidentals. A list of Peirce's alterations in the manuscripts is similarly prepared. Although neither is included in our volumes, both lists are available to interested persons for the cost of photocopying.) The Textual Apparatus is followed by Line-End Hyphenation in the Edition Text, which lists hyphenated compounds that must retain their hyphens when transcribed or quoted from the critical text of the given volume, and by an Index. (A comprehensive index and bibliography is planned for a separate later volume.)

The writings included in the edition have been prepared according to the standards of the Modern Language Association's Committee on Scholarly Editions, and they appear in "clear" text: that is, excepting a few editorial symbols that represent physical problems in the manuscripts or omitted text in publications, everything in the main section is Peirce's own, including the footnotes. In some instances, we have supplied titles. Titles of published items are printed in italic type, those of unpublished items in roman. Each title is followed by a source note or identifying number—published items are identified by P number and the bibliographic information listed in the *Comprehensive Bibliography;* unpublished items by MS number and the date of composition. (Further information concerning manuscript or publication appears in the headnote for each item in the Textual Apparatus.) MS numbers refer not to the Harvard arrangement (as given in Richard S. Robin's *Annotated Catalogue of the Charles S. Peirce Papers*) but to the new arrangement of Peirce's writings established in Indianapolis, which also includes those known to exist in depositories other than the Houghton Library. Reassembling the thousands of scattered pages and sequences of pages that were formerly in "fragment folders," and arranging all manuscripts chronologically (Peirce himself having dated only about a fourth of them), has involved a great deal of preliminary work and will continue until shortly before publication of the final volume. If further papers turn up too late to appear in their chronological places, they may be included in later supplements.

Finally, it must be said that restraint and accuracy have been the guiding principles in our editing and that our critical text represents what Peirce wrote, not what we think he should have written. This is true of published as well as unpublished writings, although the published item is, because of editorial and compositorial interference in the printing process, more likely to be emended than the latter, and at times even regularized. It should also be mentioned that, among unpublished writings, we distinguish between public and private documents (the latter including diaries, notebooks, and letters or drafts of letters) and that such private documents are reproduced almost without change. In any case, in our editing we correct typographical errors, but retain Peirce's inconsistencies in spelling and punctuation when they reflect acceptable nineteenth-century standards and practices. We make other changes only when some evidence suggests that Peirce's intention warrants them—and all such changes are listed, by page and line numbers, in the Textual Apparatus.

As a further aid to the reader of Peirce's text, we have introduced five sets of symbols into the critical text. Supplied titles appear in italic brackets; italic brackets enclosing three ellipsis points indicate one or more lost manuscript pages; italic brackets enclosing a blank indicate that an incomplete discussion occurs before the end of the manuscript page; italic brackets enclosing page and line numbers before and after three ellipsis points indicate omitted published material; and sets of double slashes mark the beginning and end of Peirce's undecided alternate readings, with the single slash dividing the original from the alternative inscription.

Acknowledgments

We are indebted to Indiana University and the National Endowment for the Humanities for their support of the Peirce Edition Project; to the Harvard University Department of Philosophy for permission to use the original manuscripts, and to the officers of the Houghton Library, especially Melanie Wisner, for their cooperation regarding the Charles S. Peirce Papers; to Webb Dordick for his research assistance in the Harvard libraries; to Bernard R. Crystal of the Butler Library of Columbia University; to Sharon Thibodeau, Lee Johnson, and Richard E. Wood of the National Archives; to Grace C. Sollers and Martha Rouse of the National Oceanic and Atmospheric Administration; to three members of the Advisory Board who have been especially helpful with this volume: Jo Ann Boydston, Don L. Cook, and William A. Stanley; to Irving H. Anellis, Thomas C. Cadwallader, I. Bernard Cohen, Joseph W. Dauben, Tomis Kapitan, Angus Kerr-Lawson, Larbi Oukada, and all other scholars who have given us expert help at various points; to André De Tienne for invaluable service as a contributing editor and visiting research associate in the Project; to the other eight contributing editors of this volume; to the three members of our editorial and administrative support staff: Cathy Clark, indefatigable transcriber and investigator, Janine Beckley, resourceful archivist and researcher, and the incomparable Barbara Shields, who almost single-handedly devised our computer system and taught us how to use it; and to Lynn A. Ziegler, our former Textual Editor, who was instrumental in the earlier stages of preparation of this volume.

We are also indebted to the Interlibrary Loan department of Indiana University-Purdue University at Indianapolis for continued good service; to the Princeton University Library for permission to publish a paper from the Allan Marquand Collection; and to the Texas Tech University Institute for Studies in Pragmaticism for permission to use duplicates of its annotated electroprint copy of the Harvard Peirce Papers. A final note of thanks goes to three administrative officers of Indiana University: Chancellor and Vice President Gerald L. Bepko, Executive Vice Chancellor and Dean of the Faculties William M. Plater, and Dean John D. Barlow of the School of Liberal Arts.

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Chronology

1839 Born on 10 Sept. in Cambridge, MA, to Benjamin and Sarah Hunt (Mills) Peirce
1855 Entered Harvard College
1859 Graduated (A.B.) from Harvard
Temporary aide in U.S. Coast Survey, fall to spring '60
1860 Studied classification with Agassiz, summer-fall
1861 Entered Lawrence Scientific School at Harvard
Appointed regular aide in Coast Survey, 1 July
1862 Married to Harriet Melusina Fay, 16 Oct.
1863 Graduated *summa cum laude* (Sc.B.) in chemistry from Lawrence Scientific School
1865 Harvard lectures on "The Logic of Science," spring
Began Logic Notebook, 12 Nov.; last entry in Nov. '09
1866 Lowell Institute lectures on "The Logic of Science; or Induction and Hypothesis," 24 Oct.–1 Dec.
1867 Elected to American Academy of Arts and Sciences, 30 Jan.
1869 First of ca. 300 *Nation* reviews, in Mar.; last in Dec. '08
Assistant at Harvard Observatory, Oct. '69–Dec. '72
Harvard lectures on "British Logicians," Dec.–Jan.
1870 First Survey assignment in Europe: 18 June–7 Mar. '71
1872 Founding member of Cambridge Metaphysical Club, Jan.
In charge of Survey office, spring-summer
Put in charge of pendulum experiments, beginning in Nov.
Promoted to rank of Assistant in the Survey, 1 Dec.
1875 Second Survey assignment in Europe: Apr. '75–Aug. '76
Served as first official American delegate to International Geodetic Association in Paris, 20–29 Sept.
1876 Separated from Melusina in Oct.
1877 Elected to National Academy of Sciences, 20 Apr.
Third Survey assignment in Europe: 13 Sept.–18 Nov.
Represented U.S. at International Geodetic Association conference in Stuttgart, 27 Sept.–2 Oct.
1878 *Photometric Researches* published in Aug.
1879 Lecturer in Logic (till '84) at Johns Hopkins University
First meeting of JHU Metaphysical Club, 28 Oct.

1880 Elected to London Mathematical Society, 11 Mar.
Fourth Survey assignment in Europe: Apr.–Aug.
French Academy address on value of gravity, 14 June
1881 Elected to American Association for the Advancement of Science in Aug.
1883 *Studies in Logic* published in spring
Divorced from Melusina, 24 Apr.
Married to Juliette Froissy (Pourtalès), 30 Apr.
Fifth and final Survey assignment in Europe: May–Sept.
1884 In charge of Office of Weights and Measures, Oct.–22 Feb. '85
1888 Purchased "Arisbe," outside Milford, PA
1889 Contributor to *Century Dictionary*
1891 Resigned from Coast and Geodetic Survey, 31 Dec.
1892 Lowell lectures on "The History of Science," 28 Nov.–5 Jan.
1893 *Petrus Peregrinus* announced; prospectus only published
"Search for a Method" announced by Open Court; not completed
1894 "The Principles of Philosophy" (in 12 vols.) announced by Henry Holt Co.; not completed
"How to Reason" rejected by both Macmillan and Ginn Co.
1895 "New Elements of Mathematics" rejected by Open Court
1896 Consulting chemical engineer (till '02), St. Lawrence Power Co.
1898 Cambridge lectures on "Reasoning and the Logic of Things," 10 Feb.–7 Mar.
"The History of Science" announced by G. P. Putnam's; not completed
1901 Contributor to *Dictionary of Philosophy and Psychology*
1902 Grant application for "Proposed Memoirs on Minute Logic" rejected by Carnegie Institution
1903 Harvard lectures on "Pragmatism," 26 Mar.–17 May
Lowell lectures on "Some Topics of Logic," 23 Nov.–17 Dec.
1907 Harvard Philosophy Club lectures on "Logical Methodeutic," 8–13 Apr.
1909 Last published article, "Some Amazing Mazes"
1914 Died on 19 April

Introduction

The years 1879–84 were perhaps the most fulfilling and disappointing in the life of Charles Sanders Peirce. He saw the promise of a long hoped for academic career, established important academic contacts and had remarkable successes as a teacher, and gained international prominence as a scientist. But in 1884 his academic career ended in disgrace, and his scientific reputation was soon to suffer a serious assault. His purpose and sense of direction would be so battered that he would retreat to the seclusion of a country house to spend the rest of his life with his second wife, Juliette. However, during these years, amid the turmoil of personal victories and private calamities, Peirce worked at a fever pitch and produced some of his most important writings.[1]

The most momentous and consequential event during these years was the death of his father on 6 October 1880. Born in 1809, Benjamin Peirce was Harvard's Perkins Professor of Astronomy and Mathematics for nearly forty years, and America's leading mathematician. He was largely responsible for introducing mathematics as a subject for research in American institutions, and he is known especially for his contributions to analytic mechanics and linear associative algebra. He helped organize the Smithsonian

[1]I thank Professor Max H. Fisch, without whose guidance this introduction could not have been written. I have drawn freely from his published writings and extensive files. For further information about Peirce's time in Baltimore, see his "Peirce at the Johns Hopkins," included in the collection of some of his papers: *Peirce, Semeiotic, and Pragmatism*, ed. Kenneth L. Ketner and Christian J. W. Kloesel, (Bloomington: Indiana University Press, 1986); for information about Peirce's scientific and mathematical work, see the published writings of Victor Lenzen and Carolyn Eisele.

To reduce the number of footnotes, I do not give references for items that can be easily located by keeping the following in mind: all manuscript references (according to either the Peirce Edition Project or Robin arrangement) are to the Peirce Papers at Harvard University which also contain the correspondence between Peirce and his parents; Daniel C. Gilman's correspondence, as well as the Metaphysical Club minute book, is in the Milton S. Eisenhower Library at Johns Hopkins University; Allan Marquand's lecture notes (the main source for information about Peirce's logic courses) are in the Princeton University Library; correspondence with employees of the Coast Survey is in Record Group 23 in the National Archives; Max Fisch's correspondence is at the Peirce Edition Project in Indianapolis.

Institution, and from 1867 to 1874 served as superintendent of the United States Coast Survey. Benjamin Peirce was widely regarded as the most powerful mind so far produced in the United States.[2]

At the time of Benjamin's death, it was thought that of his four surviving children (Benjamin Mills had died in 1870) the one most endowed with his intellectual powers was Charles, who was expected to carry on his father's work. Benjamin himself appears to have expected as much, for at the close of his remarks on the impossible in mathematics before the Boston Radical Club near the end of his life, he "observed that his son Charles was now engaged in carrying on his investigations in the same line to which he had specially applied himself; and it was a great gratification to him to know that his son would prosecute the work to which he had devoted the latter part of his own life."[3]

There is no doubt that his father had greater influence on Charles's intellectual development than did anyone else. Early on he had recognized his son's powers and had taken a continuing interest in his education and career. He got Charles started at the Coast Survey with a salaried position in 1867, and by putting him in charge of pendulum operations set the course for Charles's scientific work for the remainder of his career with the Survey. When in 1870 they traveled home to Cambridge from Michigan with the body of Benjamin Mills, he advised Charles against trying to make a career of logic; it would be better, he said, to stick with science. When his father died in 1880, Charles may well have remembered this advice, for he soon announced that he would quit logic and philosophy.

The full impact on Charles of his father's final illness and death can only be guessed at. The emotional toll is manifested in his impulsive decision to quit logic and philosophy and sell his library, a decision he soon came to regret, and in a general malaise that settled over him. Upon returning to Baltimore after the funeral in Cambridge, Peirce wrote to his mother in late October: "I have had a fog resting on my spirit ever since I have been back, so that I have not been working very successfully but I hope it is clearing up. It has been just like a steamer forging through a fog." That image may well have been vivid for Peirce. Because of his father's grave condition a few months earlier, in late July, he had been called home from Europe where he had been on assignment for the Coast Survey, and it is likely that he returned aboard the French steamer *St. Laurent* which arrived in New York on 4 August "after a passage in which it had strong W. gales and fog most of the time."[4]

The effect of his father's death on the direction of Peirce's work was immediate. Along with his brother, James Mills, Charles turned to Benjamin's writings, hoping to get more of them into print. He spent much of the

[2]See "Reminiscences of Peirce," in *Benjamin Peirce 1809–1880; Biographical Sketch and Bibliography,* ed. Raymond Clare Archibald (Oberlin, OH: Mathematical Association of America, 1925; reprinted New York: Arno Press, 1980).

[3]*Sketches and Reminiscences of the Radical Club,* ed. Mrs. John T. Sargent (Boston: James R. Osgood & Co. 1880), pp. 379–80.

[4]Victor Lenzen to Max H. Fisch, 3 March 1963.

next year editing and annotating his father's privately printed *Linear Associative Algebra* of 1870. Mathematical topics began to occupy him more frequently than ever before, although this was also due to the influence of the mathematical community in which he found himself at Johns Hopkins University. But Peirce was already a talented mathematician who had accomplished enough to be included in the small group of scientific men in America who were capable of contributing to sciences that were laden with mathematical theory. Other men in this group, mainly mathematical astronomers, included Simon Newcomb, Asaph Hall, and George William Hill.[5]

Probably the greatest effect of Benjamin's death on Charles was the loss of the influence and protection that his father's reputation had provided. Benjamin had been so highly regarded in scientific and academic circles, and his opinions and interests had carried such weight, that Charles almost always received special consideration. After his father's death this protective influence ended and Charles was left to make his own way.

The loss of his father was soon followed by the death of Carlile P. Patterson, who in 1874 had succeeded Benjamin Peirce as superintendent of the Coast Survey. Patterson's death on 15 August 1881 brought to an end the golden age of the Coast Survey, a time when pure research was much esteemed and the daily course of activity was governed by a desire to learn and discover as much as by the need to achieve practical results for a technologically oriented and sometimes shortsighted Congress. Patterson had been an ideal employer and it appears from Peirce's eulogy (P 264) that he feared a change for the worse:

> His superintendency was marked by . . . great practical achievements. . . . Yet, although he was not professedly a scientific man, under none of the eminent geodesists who had preceded him was more stress laid upon the scientific branches of the work–to their extension, and to the precision of their execution.
>
> No one was so earnest as he to secure to the Survey the labors of men of purely scientific, and especially mathematical, attainments and abilities.
>
> . . . I feel that in Patterson's death the science of the country has lost a staunch ally.

As it turned out, Peirce had good reason to fear the worst. No sooner had Julius Hilgard taken over as superintendent, something Benjamin had sought to prevent, than Peirce was put on notice that his reports would have to be more timely. In this way Hilgard let it be known that he did not have Patterson's patience for Peirce's exacting and time-consuming methods nor, perhaps, for delays caused by his recent commitment to Johns Hopkins. So began a period of disaffection that in 1891, after thirty-one years of service, led to Peirce's forced resignation. In the meantime Hilgard had led the Survey into a public scandal and, after his dismissal in 1885, the Survey fell for the first time into the hands of a bureaucrat with no training in science, F. M. Thorn.

Nearly as consequential for Peirce as his father's death was his divorce from his first wife, Harriet Melusina (Zina) Fay Peirce, on 24 April 1883,

[5]John W. Servos, "Mathematics and the Physical Sciences in America, 1880–1930," *Isis* 77 (1986): 611–29. These men had all been students of Benjamin Peirce.

and his marriage to Juliette Annette Froissy Pourtalai (or de Pourtalès) just six days later. Peirce and Zina had married on 16 October 1862 and they lived together until Zina refused to accompany Peirce when he moved from Cambridge to New York City in October 1876. Although her reasons have never been fully disclosed, it is clear that Zina was unwilling to live the itinerant life that Peirce thought essential for his work with the Survey. She never remarried and in later years expressed regret that she had not stayed with Peirce.

Sometime during his separation from Zina, perhaps within the first year, Peirce met Juliette, said to be the widow of a Count Pourtalai and the sister of a diplomat brother who had, it seems, been known to George Bancroft while he was ambassador to Prussia. Bancroft is said to have recognized Juliette in America from her resemblance to her brother. She was generally thought to be a Frenchwoman, but she actively suppressed all accounts of her origin and her identity remains uncertain.

Peirce probably met her at the Brevoort House, a European-styled hotel located on Fifth Avenue near Washington Square, where he usually stayed when in New York City. He was well known to the manager of the Brevoort, who reportedly introduced Peirce and Juliette on the occasion of a great ball.[6] Thus began an affair conducted far less discreetly than the times demanded, an affair for which Peirce and Juliette would suffer greatly. Rumors of their romantic indiscretions would ultimately cut Peirce off from university life and Juliette from society.

Of those outside Baltimore who knew of Peirce during the period covered by the present volume, most thought of him as a scientist in the service of the Coast Survey.[7] His association with the Survey began in 1859 and in July 1861 he was appointed a regular aide. In 1867, less than five months after Benjamin had become superintendent, Charles was promoted to a salaried position and began his rise to prominence in science. His primary field of scientific endeavor became geodesy, especially after 1872 when his father promoted him to assistant, the rank immediately below that of superintendent, and put him in charge of pendulum experiments. The two main aims of Peirce's geodetic operations were to determine the force of gravity at various locations in the United States and abroad and, from these results, determine the figure of the earth.

Peirce's scientific work extended far beyond geodetic operations. For example, he made notable contributions to metrology. Determinations of gravity require exact measurements of the length of the pendulums employed, and exact measurements depend on precision comparisons with standards. Consequently, Peirce spent a good deal of time comparing the

[6]Henry James to Henry S. Leonard, 2 Oct. 1936.

[7]The Coast Survey became the Coast and Geodetic Survey in 1878, thereby signifying official recognition that geodesy was now the regular business of the Survey. Peirce's father had played an important rôle in bringing about this expansion of its responsibilities. For convenience, I use the older and shorter name.

lengths of Coast Survey pendulums with recognized European standards. This work led to a more generalized interest in standards, and for several months in 1884–85 he was in charge of the U.S. Office of Weights and Measures.

It seems natural that extensive work with pendulums should have led an inquiring mind like Peirce's to reflect on the methodology of pendulum experimentation and on the adequacy of the instruments themselves. To some extent such reflection was part of the job, for it was essential that the data of observations be "corrected" to eliminate the effect of systematic sources of error. But Peirce was adept at this work. In addition to establishing that the flexure of the stand of a popular pendulum (the Repsold compound pendulum) was an important source of error, which demonstrated the need for corrections to many of the gravity determinations of leading European scientists, Peirce conducted numerous experiments to determine additional sources of error. These included the effect of the wearing of the knife-edge (the thin blade on which the compound pendulum oscillates), the effect of using steel cylinders instead of knives, the effect of the oscillation of the walls of the receiver (the container in which the pendulum swings), and the effect of temperature on the length of the pendulum. Peirce also invented two styles of pendulum (only one of which was constructed) as well as a new kind of pendulum stand.

At the beginning of the period 1879–84 Peirce was involved with the U.S. Treasury Department in a matter that may have planted seeds of disaffection with the Survey. Late in 1878 he had requested an increase in his salary from $2870 to $3500, and was so determined to have his raise that he was prepared to submit his resignation should it be refused. "I prefer working for somebody who will consider the character of my work," he wrote to his father on 14 January 1879. (Peirce may have had Daniel C. Gilman in mind, the president of Johns Hopkins University, with whom he had been in correspondence for more than a year about the possibility of an academic appointment.) By 8 July Superintendent Patterson sent the request to John Sherman, Secretary of the Treasury, with the following supporting argument:

> Mr. Peirce is forty years of age, has been employed on the Survey for eighteen years, and on account of his exceptional ability for special investigations, was during eleven years service rapidly advanced to his present pay in 1873. Since that date Mr. Peirce has made extraordinary advances in Pendulum observations of a very original character, exciting the deepest interest in this important scientific subject on the part of all physicists, both in this country and abroad, and leading to a complete revision of all past observations at the main initial points for Pendulum observations in Europe. In fact Mr. Peirce is the first person in this country who has with any success attacked this problem, the subject having remained in abeyance for many years, awaiting a truly scientific observer. Mr. Peirce has also succeeded in comparing the accepted standard unit of length (the meter) with a permanent (so far as now known) length in nature, a wave length of light, a task hitherto never attempted on account of the inherent difficulties of the case, over which after many discouragements and failures he has at last triumphed. These results of Mr. Peirce's work have greatly advanced the science of Geodesy, the scientific reputation of the Survey, and therefore that of the Country.

The enclosed extracts from letters of eminent American Scientists offer the best evidence of the value of Mr. Peirce's work.[8]

The eminent scientists were Alfred M. Mayer, professor of physics at Stevens Institute of Technology; Wolcott Gibbs, Rumford Professor at Harvard University; Ogden N. Rood, professor of physics at Columbia College; and Benjamin Peirce.

Mayer reported that the results of Peirce's work already "are of the highest importance to the advancement of science and to the interest of the U.S. Coast Survey. Mr. Peirce's methods are original, and of an accuracy and refinement which are unsurpassed"; he added that "Mr. Peirce deserves well of his countrymen, for his work has added much to the scientific reputation of the U.S. Coast Survey among European nations." Gibbs discussed the spectroscopic apparatus that Peirce used in his experiments with light waves. "I have carefully examined the apparatus," he said, "and am of opinion that it is admirable both in design and in workmanship. In fact I do not hesitate to say that both the spectroscope and spectrometer are the most perfect instruments of the kind in existence, and I have been both delighted and instructed by a critical examination of the refinements introduced in their construction." Rood addressed Peirce's general merit and the "very high estimation in which Mr. Peirce's contributions are held by the scientific men of this country and of Europe," and he claimed that "it would be difficult to find another scientist having similar qualifications with Mr. Peirce either in the special education required, or in natural ability. I certainly know of no one in this country who would be at all qualified to take the position which he now holds in your Survey." Finally, Benjamin Peirce, whose relation to Charles may have somewhat weakened the impact of his remarks, wrote of his son's work in establishing a wave-length of light as a standard of length:

It is a most remarkable achievement to have thus determined the length of the meter from the wave-length of light, which is the shortest length which has ever been measured; and the only sure determination of the meter, by which it could be recovered if it were lost to science. It will certainly secure for the Survey the applause of all scientific men.

When combined with Mr. Peirce's admirable measures of the pendulum, which have justly been regarded by the savans of Europe as adding a new era to this most difficult branch of observation, it places him among the great masters of astronomical and geodetic research, and it would be most unfortunate, i[f] such grand strides in science were not suitably acknowledged.

But Peirce did not get his raise. In Patterson's letter conveying Sherman's decision, he assured Peirce that he would do anything in his power to advance his interests outside the Survey, but said that it would be difficult to replace him. By the time Peirce heard of Sherman's denial he had received his part-time appointment at Johns Hopkins and concluded that with

[8]This extract from Patterson's letter to Sherman, and the extracts referred to therein, are included in Patterson's 8 August 1879 letter to Peirce (L 91).

his combined salaries he was sufficiently well off. Besides, he felt that Patterson, who had admitted that he was not adequately paid, might be "more or less indulgent" of his connection with Johns Hopkins—a recognition of the potential difficulty of pursuing two careers at once.

William James had recommended Peirce to Gilman for the professorship of logic and mental science in 1875, and Benjamin Peirce had later recommended him for the professorship of physics. By 13 January 1878 Peirce had informed Gilman of his strong interest in being "called" to Johns Hopkins and had set out in detail his projected program for the physics department. Peirce emphasized that he was a logician and had learned physics as part of his study of logic; for "the data for the generalizations of logic are the special methods of the different sciences. To penetrate these methods the logician has to study various sciences rather profoundly." He then described his view of logic and remarked on the importance of his work:

> In Logic, I am the exponent of a particular tendency, that of physical sciences. I make the pretension to being the most thorough going and fundamental representative of that element who has yet appeared. I believe that my system of logic (which is a philosophical method to which mathematical algebra only affords aid in a particular part of it) must stand, or else the whole spirit of the physical sciences must be revolutionized. If this is to happen, it cannot be brought about in any way so quickly as by the philosophical formularization of it and the carrying of it to its furthest logical consequences. If on the other hand it is to abide, its general statement will be of consequence for mankind. I have measured my powers against those of other men; I know what they are. It is my part to announce with modest confidence what I can do. My system has been sketched out but not so that its bearings can be appreciated. If the world thinks it worth developing, they have only to give me the means of doing it. But if not, I shall follow another path, with perfect contentment.

Gilman inquired on 23 January whether Peirce would accept a half-time appointment as lecturer of logic, while retaining his position with the Survey. Peirce replied on 12 March that he would.

> The truth is that the great difficulty I had in reaching a decision was that if I were to be your professor of logic, my whole energy and being would be absorbed in that occupation. Right reasoning is in my opinion the next thing in practical importance to right feeling; and the man who has to teach it to young minds has such a tremendous responsibility, that the idea of giving $\frac{1}{2}$ his activity to such a business seems shocking. All the more so, that students have hitherto been fed with such wretched bran under the name of logic. That name now rests under a just opprobrium from which, if I should become your professor, it would be the purpose of my life to redeem it, first in the eyes of those who had been my pupils, and next before the world; for I should think that I had failed if my pupils did not carry into after-life a more distinct idea of what they had learned from me than of most of the subjects of their study and did not feel that the study of reasoning had been of great advantage to them.

But the trustees had already decided not to make any further appointments that year.

Gilman inquired again the following year, and though Peirce now set certain conditions, he again replied affirmatively (on 6 June 1879). He wanted to have sole charge of instruction in logic and the assurance that the position would eventually be full-time. Furthermore, he advised Gilman that he would be on Coast Survey business in Europe until after the beginning of the fall term. As for the teaching of logic, Peirce's views were much the same as he had expressed the previous year.

> There are two things to be done; one, to communicate the *logica utens*, and to make expert reasoners of the pupils, able to form clear ideas, to avoid fallacies & to see in what quarters to look for evidence; the other, to familiarize them with the logical ideas which have percolated through all our language & common sense, & to show their significance & what they are worth. Special branches of logic may of course be taught in special cases; such as logical algebra, the history of logic, etc. etc.

On 13 June 1879 Gilman made an offer which Peirce accepted on 20 June, the day after he received it.

So it happened that for most of the period 1879–84 Peirce pursued two careers: as a scientist in the most prestigious scientific agency in America and as a teacher and scholar in the most advanced American school for graduate studies. Peirce was a regular commuter on the B & O railroad between Washington and Baltimore during these years. He tried to do well in both jobs, but that was a formidable predicament and, as it turned out, a near impossibility. Given the demands of his position in the Coast Survey, which included frequent travel and sustained periods of research and experimentation, and the pressures of a new career in teaching with the excitement of his longed-for interaction with brilliant students, it is not surprising that Peirce's health began to break. He struck an alarming note toward the end of his first term of teaching when in a curious letter to Gilman, written on Christmas Day 1879, he said:

> I have an odd thing to say to you which is to be perfectly confidential unless something unexpected should occur. In consequence of certain symptoms, I yesterday went to see my physician in New York, & he after calling in an eminent practitioner in consultation, informed me that he considered the state of my brain rather alarming. Not that he particularly feared regular insanity, but he did fear something of that sort; and he must insist on my being some little time in New York and he could not promise that I should go back on January 5th. For my own part, I do not think the matter so serious as he thinks. The intense interest I have had in the University and in my lectures, combined with my solitary life there, & with the state of my physical health, has undoubtedly thrown me into a state of dangerous cerebral activity & excitement. But I feel convinced that I shall surprise the doctors with the rapidity with which I regain my balance. I don't think the matter of any particular importance. However, I think it best to say to you as much as I do say; both that you may understand why I may possibly not be on hand Jan 5, and also because the matter might turn out worse than I anticipate, and I might do some absurd thing. I have said nothing to anybody else than you; & I beg you will not let me see that it is in your mind when I go back; for I shall not go back until it is quite over.

The matter was apparently no more serious than Peirce had thought; nevertheless, for the next several years he suffered from ill health.

It is amid the events and circumstances so far described that Peirce's writings of this period were created. Although much of his work exhibits his dual preoccupations—his scientific work is reflected in his academic work, and vice versa—his writings generally concern one or the other of his pursuits. These pursuits are distinct enough to be treated separately, though it is well to keep in mind the parallel unfolding of the events described in the following two sections.

The Coast and Geodetic Survey

The decade preceding 1879–84 has sometimes been regarded as Peirce's most "intensely scientific period," but he seems to have lost little intensity during the present period. A review of his scientific undertakings and accomplishments reveals that his productivity remained on a par with that of the previous decade, although his reliability, especially with regard to the preparation of his field reports, did decline somewhat. With his part-time employment at Johns Hopkins Peirce could not be so single-mindedly directed toward scientific undertakings as he had been during the 1870s. But Peirce's commitment to teaching did not keep him from carrying on a full life of science.

From 1879 to 1884 Peirce was in charge of half a dozen major pendulum observation parties at several sites in Pennsylvania and at St. Augustine, Savannah, Fortress Monroe in Virginia, and the Smithsonian. Extensive experiments were also conducted in Baltimore and Cambridge and at Stevens Institute in Hoboken, New Jersey. Besides these domestic occupations Peirce led an observation party to Montreal in 1882, and in the summers of 1880 and 1883 he made the final two of his five sojourns to Europe on assignment for the Coast Survey. The fieldwork for these assignments resulted in perhaps as many as two hundred field books of experimental data, and it generated over a linear foot of detailed correspondence (most of which is deposited in Record Group 23 in the National Archives). In addition to these major assignments Peirce performed the regular functions of his office and carried out a number of other experiments at the Washington headquarters. He conducted experiments with his spectrum meter in his attempt to establish a wave of sodium light as a unit of length and oversaw the construction of four pendulums of his own design (Peirce Pendulums 1–4). Throughout these years Peirce was always at work on the reduction of the data of his field notes and on the preparation of reports for publication, primarily for the superintendent's annual reports. He saw more than a dozen scientific papers into print and he contributed at least as many papers and reports to scientific associations, most notably the National Academy of Sciences.

In 1879 Peirce's initial concern was to get fieldwork underway in accordance with his assignment to determine the disturbing effect of the Appalachian mountains on geodetic operations. Early in January he occupied the Allegheny Observatory in Pennsylvania[9] and began to take measurements of gravity. Peirce had coordinated the pendulum operations there so that he could supervise the Observatory while its director, Samuel P. Langley, was off to Mt. Etna during the first part of the year. Before Langley's departure Peirce had visited the Observatory to "get the hang of it." (In later years,

[9]Allegheny was a city in Allegheny County, Pennsylvania, which later amalgamated with Pittsburgh.

when he was director of the Smithsonian Institution, Langley provided much-needed employment for Peirce as a translator of French and German scientific publications).

Peirce's fieldwork was completed at Allegheny in March and resumed at Cresson in July and at Ebensburgh in mid-August. Field operations at these Pennsylvania stations were concerned with the determination of gravity but also with sources of error resulting from the nature of the pendulum apparatus itself. Peirce had worked on the latter since 1875 when he had surprised Europe's leading geodesists at a Paris conference with his claim that the stand of the Repsold pendulum was unstable and thus a systematic source of error.

Peirce had acquired a Repsold pendulum during his second European assignment in 1875, and had made a series of determinations at selected European locations (or "initial stations") in order to relate American to European results. In his report on these determinations he emphasized that "The value of gravity-determinations depends upon their being bound together, each with all the others which have been made anywhere upon the earth." He had made determinations in Berlin, Geneva, Paris, and Kew, and had met such leading figures as James Clerk Maxwell of Cambridge, Johann Jakob Baeyer of Berlin, and Emile Plantamour of Geneva.

It was during this time that General Baeyer had first raised the suspicion that the Repsold stand might be unstable. Peirce examined the stand in Geneva and worked out an approximate value of the error due to its swaying, which he presented at the Paris conference. If Peirce was right, all of the results published in Europe during the previous ten years would be vitiated. Although Peirce's claim drew little response, Hervé Faye suggested that such an error might be overcome by setting up two pendulums on the same stand and by swinging them simultaneously in opposite directions. The following year at a meeting in Brussels, which Peirce did not attend, it was concluded that he was mistaken. Peirce resolved to defend his claim at the next meeting of the European Geodetical Association in 1877 in Stuttgart. With abundant experimental data in hand and with the mathematical theory well worked out, Peirce won the day. He later reported that "from that time I was acknowledged as the head of that small branch or twig of science."[10]

The results of Peirce's geodetic work in Europe, and some subsequent work in the United States, were set forth in the extensive monograph entitled "Measurements of Gravity at Initial Stations in America and Europe" (item 13), which is regarded as one of the classics of geodesy and the first notable American contribution to gravity research. It was specially noted at the Munich meetings of the International Geodetical Association in 1880 and it is listed as a basic monograph on the pendulum in the 1904 *Encyklopädie der mathematischen Wissenschaften.* The results of Peirce's work on flexure were presented in April 1879 at a meeting of the National Academy of Sciences (P 152) and appeared not long after in the *American Journal of Science and Arts* as "On a method of swinging Pendulums for the determina-

[10]Peirce to J. H. Kehler, 22 June 1911 (L 231).

tion of Gravity, proposed by M. Faye" (item 5), which shows the theoretical soundness of Faye's method for avoiding error due to flexure.

Three more papers that Peirce read to the Academy in April indicate his other scientific endeavors. His "Comparison of the meter with wave lengths" (P 154) detailed his efforts to establish wave-lengths of light as a standard of length; a different version of the paper (P 133) was presented by his father to the American Academy of Arts and Sciences in Boston. Although summary reports of this work were published in various scientific journals—as in items 2 and 4, or in his "Mutual Attraction of Spectral Lines" in *Nature* (P 156)—no major study was ever published. By 1886 Peirce had several times revised his report on the spectrum meter but the finished monograph has been lost. Item 37 is what remains of an 1882 version.

In his spectrum meter experiments, Peirce compared wave-lengths of light with the breadth of a diffraction-plate. He used a machine called a comparator, a spectrometer he himself designed, and a diffraction-plate designed by Lewis M. Rutherfurd. These experiments led him to the discovery of hitherto unknown diffraction phenomena called "ghosts," which provided the topic for his third paper to the National Academy (P 153) and the published paper "On the Ghosts in Rutherfurd's Diffraction-Spectra" (item 10).

Peirce's fourth paper, "On the projections of the sphere which preserve the angles" (P 151), was the first public presentation of his quincuncial projection; it was later published in the *American Journal of Mathematics* (item 11) and in the 1876 Coast Survey *Report* (P 183; see also P 238). The quincuncial projection allowed for repetition of the whole sphere in transposed positions on the map so that any location might be viewed as occupying a central position relative to the rest of the earth. It was used during World War II for charting international air routes. Peirce had completed most of the work on the projection by 1879 and the first quincuncial map appeared in May 1879 in an appendix to the *Proceedings of the American Metrological Society,* but there it was only a convenient map for showing the date-line from pole to pole, not a new projection with supporting mathematical theory.

Another classic paper in the 1876 *Report* (in addition to item 13) is Peirce's "Note on the Theory of the Economy of Research" (item 12). The theory developed in this paper was intended to guide scientific researchers in their efforts to balance the benefit of advancing knowledge against the costs of the research. The main problem of the doctrine of economy is "how, with a given expenditure of money, time, and energy, to obtain the most valuable addition to our knowledge," a problem that concerned Peirce even in his later years. This paper has been reprinted as recently as 1967 in *Operations Research.*

Two other papers published in 1879 illustrate the scope of Peirce's scientific interests during the period 1879–84. The 16 October issue of the *Nation* contained his review of Ogden Rood's *Modern Chromatics* (item 9), which makes several references to Peirce's own experimental work on color, and the 1876 Coast Survey *Report* contained yet a third paper, entitled "A

Catalogue of Stars for observation of latitude" (P 159). This catalogue, which was intended to supersede the list published in the 1873 *Report* (P 95), does not appear under Peirce's name, but J. E. Hilgard's preface indicates that "the list was selected under the direction of Assistant C. S. Peirce, and the names of the stars were assigned by him."

Peirce concluded his fieldwork for the determination of the disturbing effects of the Allegheny mountains with a three-month occupation of a station at York, Pennsylvania, in 1880. Henry Farquhar conducted the operations, which continued until mid-June, under Peirce's direction. In addition to measurements of gravity, observations were made for the detection of flexure, and experiments were conducted in which the standard pendulum knife was replaced by small steel cylinders that acted as bearings. This method had been proposed by both Peirce and Yvon Villarceau in order to avoid the effects of the blunting of the knife-edge, but Peirce eventually showed that the cylinders increased rather than reduced friction.

In April Peirce sailed on his fourth Coast Survey assignment to Europe. Although his previous gravity determinations in Paris varied significantly from the accepted measures of Borda and Biot, he demonstrated that, when corrected for errors not suspected at the time of their observations, their work came into line with his. His paper "On the Value of Gravity at Paris" (item 15) is a translation of the paper he presented to the French Academy of Science and published in the Academy's *Comptes Rendus* (P 171). Peirce intended to report on his pendulum work and his spectrum meter at the International Geodetical Association meeting in September in Munich but, as mentioned earlier, he was called home when his father became seriously ill. He sent an abbreviated report in the form of a letter to Hervé Faye, which was published in the Association's proceedings (item 17).

After his return from Europe in 1880, Peirce did not take up any new projects right away. He provisionally completed his comparison of the meter with a wave-length (although he soon resumed that study), pursued his investigations of the effect of the walls of the receiver on the period of oscillation, and labored to improve the related mathematical theory. In mid-November he read a paper "On the ellipticity of the earth as deduced from pendulum experiments" to the National Academy of Sciences in New York City; it was later published in the 1881 Coast Survey *Report* (item 76).

Several more of Peirce's scientific writings appeared in print in 1880. In July, *Nature* published "On the Colours of Double Stars" (item 18), and "The quincuncial projection" was reprinted in the 1876 Coast Survey *Report.* A summary of the "Measurements of Gravity at Initial Stations" appeared as "Results of Pendulum Experiments" in the October issue of the *American Journal of Science and Arts* (item 21, which was reprinted in the November issue of *The London, Edinburgh, and Dublin Philosophical Magazine and Journal of Science*), and a report of his pendulum operations was included in the 1879 report of the Commission der Europaeischen Gradmessung (P 184).

Much of Peirce's Coast Survey work during the first half of 1881 focused on the construction of his four invariable reversible pendulums which, according to Hilgard, had their surfaces "as nearly as convenient in the form of an elongated ellipsoid." Peirce had invented the pendulums so that the effects of viscosity could be theoretically ascertained. Three of them (Nos. 1, 2, and 4) were meter pendulums, and one (No. 3) was a yard pendulum. Peirce No. 1 was used in the Arctic, in Franklin Bay above the 80th Parallel, by an expeditionary party led by Lieutenant Adolphus W. Greely. Greely's party was one of two U.S. parties assembled as a result of a meeting in Hamburg in 1879 where eleven nations[11] had agreed to man polar stations for one year (1882–83) to conduct scientific observations and pool their results. Included in Greely's assignment was pendulum work for the determination of gravity, which Peirce had carefully planned. He had personally instructed Greely's astronomer, Sergeant Edward Israel, in the conduct of the experiments. Greely's party suffered terrible hardships and eighteen of his twenty-five men, including Israel, died during the course of the next three years. Although Israel and Greely had meticulously recorded and maintained the records of their pendulum experiments, Peirce at first concluded from the data that some accident must have befallen the pendulum. When he recorded this opinion in his official report of the experiments (P 369), it occasioned a mild dispute. It ended amicably when Peirce assured Greely that "there has been no failure, but this determination is far more reliable than any other which has ever been made within the arctic circle, and this will be, take my assurance of it, the ultimate judgment of experts".[12]

In mid-April 1881, Peirce delivered a report "On the progress of pendulum work" to the National Academy of Sciences (P 199), and in June he made gravity determinations at Washington, in late July and August at Baltimore, and in September at Cambridge. He continued his investigations of error due to the flexure of the stand and the receiver and he experimented with the Faye-Peirce plan of swinging two pendulums from the same support. In July he sent his letter to Faye (item 17) and saw the publication of his "Width of Mr. Rutherfurd's Rulings" in *Nature* (item 29), and in August he attended the thirtieth meeting of the American Association for the Advancement of Science in Cincinnati where he read a paper entitled "Comparison Between the Yard and Metre by Means of the Reversible Pendulum" (P 186). On 30 August he was elected to membership in the Association and was appointed to the standing committee on weights and measures.

Although Peirce's reputation as a geodesist was strong as 1882 got underway, he was beginning to be a source of irritation inside the Survey, primarily because of his growing tendency toward tardiness in reducing the results of his fieldwork and the preparation of reports for publication. In April he was urged by Richard D. Cutts, assistant in charge of the Survey office, to send in his appendices for the 1881 *Report* and, on 6 July, Superintendent Hilgard informed him that it had gone to press without his four appendices

[11]Austria, Denmark, England, Finland, France, Germany, Holland, Norway, Russia, Sweden, and the United States.

[12]Peirce to Greely, 27 November 1888 (L 174).

("Determination of Force of Gravity at points in Penn.," "Variation of Gravity with the Latitude," "Flexure of Pendulum Supports," and "Oscillation Period of the Walls of the Receiver"). He implored Peirce to concentrate on what could be finished by 20 July, the final day for submission, and to let him know at once which papers could not be finished so that reference to them might be struck from the report. Surprisingly, the 1881 *Report* appeared with four appendices by Peirce, although some from the above list were not included.

In April 1882 Major John Herschel of the India Survey arrived in the United States to conduct gravity operations at selected stations in order to connect British with American pendulum work. Peirce helped him get set up at Hoboken and frequently assisted him during his year-long stay. In May Herschel was invited to participate in an informal conference on the future of pendulum work and the efficiency and accuracy of the methods employed, a conference no doubt occasioned by his presence in the United States and by his prominence in the field of pendulum operations. Those in attendance besides Peirce and Herschel were Simon Newcomb of the *Nautical Almanac* and Superintendent J. E. Hilgard, as well as George Davidson and C. A. Schott of the Coast Survey. J. W. Powell, director of the United States Geological Survey, was unable to attend. Peirce edited the proceedings of the conference, which were published in the 1882 *Report* (items 48–55).

In addition to the considerable attention he gave to Herschel's pendulum operations throughout the year and to his supervision of the construction of the Peirce pendulums (work on No. 4 was still underway in May), Peirce conducted extensive fieldwork of his own. From May through September he made gravity determinations and other pendulum observations in Washington, Baltimore, Hoboken, Montreal, and Albany. Although he continued to swing the Repsold pendulum in order to coordinate his operations with those of Herschel and others, he also made observations with his new invariable reversible pendulums. By the end of 1883, Peirce Pendulums 2 and 3 had been swung at Washington, Hoboken, Montreal, Albany, and St. Augustine.

Peirce traveled to Montreal in August to make a series of pendulum experiments at the McGill College Observatory and to attend the thirty-first meeting of the American Association for the Advancement of Science. The work was very demanding and beset with complications due to equipment problems. Peirce was beginning to feel the strain of overcommitment. On 29 August he wrote to his mother:

> For a long time I have been so driven with work that I have had no time to write the smallest line except in the way of business. . . . I have prepared an enormous quantity of matter for the press of late,—almost enough to make a volume of the Coast Survey Reports. . . . I have also been very active in the line of experiments, frequently working all night. Hilgard is a regular task-master. My assistants and I have been nearly killed with overwork.

Yet Peirce managed, somehow, to take in some of his surroundings. His letter continues: "I am charmed with Montreal. It is a most lovely site, much of the architecture is fine, there is very little that is utterly dreary, and the admixture of the French element contributes something very pleasing."

Peirce had not traveled to Montreal alone. Juliette had accompanied him on the train and may have stayed with him for a short time in Montreal before traveling with her maid to Quebec City. When Peirce left on 10 September, after completing his work in Montreal, Juliette again accompanied him. They stopped over in Albany where Peirce visited the Dudley Observatory, and they stayed at the same hotel. Peirce's openness in his relations with Juliette did not go unnoticed, especially by Superintendent Hilgard.

Operations at the Fort Marion station in St. Augustine were occasioned by a field party sent by the French government to observe the 6 December transit of Venus. Peirce was assigned to assist the French party by determining the longitude of the station, which he did with the assistance of E. D. Preston, at a station in Savannah, Georgia, and Captain Desforges at Fort Marion. Although he does not seem to have spent much time in Florida in December, he did oversee the setting-up of the station, and he wrote the 21 December letter to Mitchell with a graphical notation for the logic of relatives (item 60) from St. Augustine.

Peirce read four papers to the National Academy of Sciences in 1882, two based on his work at Johns Hopkins and two on his work for the Survey. The practice of reading papers based on his work at Johns Hopkins seems to have begun in November 1881 when he read a version of his "Logic of Number" (item 38). In April he presented "On a fallacy of induction" (P 233), which he had read five months earlier to the Johns Hopkins Scientific Association (P 211). At the November meetings, he presented "On the logic of relatives" (P 235), which was probably a version of his soon to be published "Note B" in *Studies in Logic* (item 66). He also read two papers resulting from his Coast Survey work, "On the determination of the figure of the earth by the variations of gravity" (P 234) and "On Ptolemy's catalogue of stars" (P 236). The first paper may have been a version of what he had read to the Johns Hopkins Scientific Association in 1881 (P 210) and would publish in 1883 as "On the Deduction of the Ellipticity of the Earth from Pendulum Experiments" (item 76).

Three of Peirce's scientific papers appeared in print in 1882. "On Irregularities in the Amplitude of Oscillation of Pendulums" was published in October in the *American Journal of Science and Arts* (item 58); it was a response to remarks made by O. T. Sherman in an earlier issue of the *Journal* (24:176). Volume 13 of the *Annals of the Astronomical Observatory of Harvard College,* entitled *Micrometric Measurements* and published in 1882, contains the results of extensive observations made under the direction of Joseph Winlock and Edward C. Pickering during the years 1866–81. Peirce was one of the principal observers during many of those years and much of his work is represented in the volume (P 219). The third publication (P 238), an extract from the "Quincuncial Projection" (item 11), appeared in Thomas Craig's *A Treatise on Projections.*

Peirce's Coast Survey work for the first four months of 1883 consisted primarily of fieldwork at the Smithsonian and at Hoboken. From late December 1882 through most of January 1883 he was at the Smithsonian; in February he reoccupied the Stevens Institute at Hoboken, swinging Pendulums 2 and 3 in order to compare the yard with the meter; in March and April he was back at the Smithsonian.

April 1883 was an important month in Peirce's personal life. Emotions were running high in a dispute about a reference Peirce had inserted into a paper by J. J. Sylvester. But probably of greater concern to Peirce was the fact that his divorce from Zina was drawing near. The final decree was issued on 24 April and six days later he married Juliette. On the day of his divorce he had written to Gilman that something had gone wrong at the Survey, that he could not make his afternoon class and that it might be best to bring his lectures that term to a close. It was surely not coincidental that Superintendent Hilgard had issued instructions on the 23rd directing him to go to Europe to help connect English and American pendulum work. Peirce's fifth, and last, European assignment gave him and Juliette the opportunity to honeymoon away from the reproachful societies of Baltimore and Washington. Yet Peirce was diligent in executing his duties during his four months in Europe. He compared the Survey's standard yard No. 57 with the imperial yard No. 1 and with the iron yard No. 58 at the British Standards Office in London (where he also visited the library of the Royal Society). At the Kew Observatory in Surrey he measured the flexure of the pendulum base used for his 1878 experiments, which he had been unable to measure in 1878, and in Geneva he measured the flexure of the table he had used for the pendulum base in his 1875 experiments.

Part of Peirce's European assignment was to obtain special pendulum apparatus from Gautier, world-renowned manufacturer of precision instruments in Paris (where, at the Bibliothèque Nationale, Peirce made a thorough study of Paris MS. No. 7378, the Epistle of Petrus Peregrinus on the lodestone). He had known for some time that the four pendulums made at the Coast Survey Office were sufficiently defective to diminish the accuracy of measurements and he was much pleased with the prospect of having Gautier construct new pendulums, which he intended to take back with him in September. But during some preliminary experiments at the Gautier workshop he discovered a new source of error, the result of the flexure of the pendulum staff due to cuts about the knife-edges. He designed an improved staff to eliminate this flexure and received permission to have the pendulums redesigned. Unfortunately, manufacturing delays and the necessity for continued experimentation during the manufacturing process resulted in Peirce's return to America without the new pendulums. He unsuccessfully sought to obtain them after his return and was forced to continue using the faulty Peirce pendulums, thus depending on theoretically derived correction formulas. His disappointment over the Gautier pendulums contributed to Peirce's embitterment and growing estrangement from the Survey.

Having settled in Baltimore with Juliette after his return from Europe in September 1883, Peirce resumed the direction of pendulum work for the Survey and was soon conducting experiments at the Washington Office and at the Smithsonian Institution. Probably due to his lengthy stay in Europe, Peirce did not make any presentations to scientific associations during the year, although a number of his scientific papers appeared in print. *Nature* published his "Note on Peirce's comparison of U. S. Yard No. 57 with British Yard No. 1" (P 249), and the 1881 Coast Survey *Report,* published in 1883, contained his "Flexure of Pendulum Supports" (item 75), "Deduction of the Ellipticity of the Earth" (item 76), "Method of Observing the Coincidence of Vibration of Two Pendulums" (item 77), and "Value of Gravity at Paris" (item 15). Peirce's fieldwork was, as usual, detailed in the *Report*'s "Pendulum observations" (P 252). The 1882 *Report* was also published in 1883 and it contained the "Report of a Conference on Gravity Determinations, held at Washington, D. C., in May 1882" (items 48–55), which Peirce had edited and to which he contributed his "Six Reasons for the Prosecution of Pendulum Experiments" (item 51) and the "Opinions" section (item 54).

1884 was probably the worst year of Peirce's life. On 26 January he was informed of a resolution of the Johns Hopkins Executive Committee that led to his dismissal a few months later. For several weeks, even months, Peirce was in a near state of shock over the realization that his university life might be over. Except for pendulum operations at the Smithsonian that continued under his direction through April, Peirce seems to have taken up no new Survey work until July when he received instructions from Hilgard to proceed to Fortress Monroe for gravity determinations and then to reconnoiter for one or two more stations in the mountains of Virginia, West Virginia, and North Carolina. Peirce was pleased with the results of his work at Fortress Monroe but he did not succeed in finding any new gravity stations. When Peirce returned to Washington he was put in charge of the Office of Weights and Measures.

Peirce finished the year with what seems to have been a burst of energy. Having resigned himself to a non-academic life, perhaps he was settling into his life as a scientist. He occupied the Smithsonian through February 1885 and measured (by comparing with standards) all four of the Peirce pendulums. As head of the Office of Weights and Measures, he traveled to Boston, Providence, Hartford, New York, and Philadelphia and met with electricians and manufacturers of gauges and machinery to determine how to meet the need for standards of measure as set out in resolutions passed at the United States Electric Conference. At the October meetings of the National Academy of Sciences in Newport he read three papers: "On Gravitation Survey" (P 281), "On Minimum Differences of Sensibility" (P 282), co-authored with Joseph Jastrow, and "On the Algebra of Logic" (P 283). He also discussed Wolcott Gibbs's paper "On the Theory of Atomic Volumes" and R. Pumpelly's paper "On an Experimental Composite Photograph of the Members of the Academy."

On 30 December he attended the American Metrological Society meeting at Columbia College, where he read a paper on the determination of gravity (P 270) and gave an account of his measures of the Old Stone Mill at Newport. A short article on the Mill had appeared in the 5 December issue of *Science* (P 293). In a discussion of the adequacy of the standards of weights and measures in the United States, Peirce informed the Society of some of the deficiencies of the current system. As a consequence, the Society passed a resolution calling for the appointment of a committee to persuade Congress and the Secretary of the Treasury of the need for establishing an efficient national bureau of weights and measures.

Possibly the most important of Peirce's scientific writings of 1884 was his "Determinations of Gravity at Allegheny, Ebensburgh, and York, Pa., in 1879 and 1880" (P 290), which appeared as Appendix 19 of the 1883 *Report.* His *Photometric Researches* of 1878 (*W*3: item 69) figured prominently in volume 14 of the *Annals of the Astronomical Observatory of Harvard College,* entitled *Observations with the Meridian Photometer,* by Edward C. Pickering (P 271). And in November 1884, he published a paper on "The Numerical Measure of the Success of Predictions" (P 292) in *Science,* which illustrates that his interest in finding suitable means for quantifying even the evaluative elements of scientific work continued after his earlier work on the economy of research.

In bringing the picture of Peirce's scientific activities to the end of 1884 we have gone somewhat beyond the period of the present volume. Yet it should be noted that as the present period ends and as Peirce came to accept the end of his academic career, he experienced a surge of enthusiasm for experimental science. For a few months, until scandal shook the Survey, he may have thought that goodwill toward him might be restored. But, as will be seen in the introduction to the next volume, that was not to be.

The Johns Hopkins

Though Peirce's decision to teach logic at the Johns Hopkins was a diversion from the scientific path he had been following so successfully, it did not set him on a new path of inquiry. As he had clearly shown in his January 1878 letter where he had set down his views on how the physics department should be organized, logic had long been his abiding research interest. Some of his earliest writings were about logic, broadly conceived to include the study of scientific method as well as the more formal investigations of the syllogism and the algebra of logic. His first major series of lectures, the Harvard Lectures of 1865, was on the *logic* of science, and by the following year he had begun chapter 1 of a treatise on logic where he had pointed out that, although formal logic may seem trivial, it has in fact such a deep significance that "the commonest and most indispensible conceptions are nothing but objectifications of logical forms" (*W*1:351). Six years later, spurred on by the seminal deliberations of the Cambridge Metaphysical Club, Peirce was at work on his 1872–73 logic book, with "logic" now defined as "the doctrine of truth, its nature and the manner in which it is to be discovered" (*W*3:14). Although his focus had shifted somewhat from the formal to the pragmatic aspects of inquiry, his general interest still was logic.

There is good reason to believe that his famous "Illustrations of the Logic of Science" of 1877–78 was the fruition of the 1872–73 work. Peirce expected to finish the "Illustrations" as the period of the present volume got underway and to publish them in book form in the International Scientific Series. The sixth paper had appeared in the *Popular Science Monthly* in August 1878 and the French version of the second paper appeared in January 1879. As late as 1881 he wrote to his mother that he was thinking of writing more papers for the series and in early 1882 he wrote, in the front of a diary listing his expectations for the year, that he intended "to write my book on logic." With this in mind, and remembering his 1867 American Academy Series (*W*2: items 2–6) and his pioneering 1870 "Description of a Notation for the Logic of Relatives" (*W*2: item 39), it is clear that when Peirce took up logic at Johns Hopkins, he was continuing a line of research he had long pursued. Already, W. K. Clifford had declared Peirce to be "the greatest living logician, and the second man since Aristotle who has added to the subject something material."[13]

But in January 1879, even with further "Illustrations" planned, neither logic nor philosophy in general was much on Peirce's mind. He was hard at work on his spectrum meter experiments and plans for his extensive Pennsylvania fieldwork for the Survey, and he was under considerable pressure to finish his report on gravity at initial stations (item 13) and some other field reports. Almost all of Peirce's 1879 writings, until he took up his position at Johns Hopkins in the fall, reflect these scientific interests. The only exceptions are his short review of Read's *Theory of Logic* (item 1) and his lecture on logic and philosophy (item 3) which he may have delivered to the Harvard Philosophical Club in May. But this soon changed. On 27 July, he wrote to President Gilman that he was preparing his first lectures—"You would be amused if I were to say that they were very fine"—and soon afterwards he was deeply engaged in some of his most original logical researches. Not for several years—not until after his dismissal from Johns Hopkins—did his philosophical research extend once again beyond logic to phenomenology and metaphysics.

Before turning to a chronological account of Peirce's life at Johns Hopkins, a few general historical remarks should be made. The Johns Hopkins University opened in 1876, financed by a bequest of the Baltimore philanthropist who gave the university its name. On the advice of the presidents of some leading universities, the trustees decided to focus on the establishment of professional schools and to emphasize research and graduate education. Daniel C. Gilman had been appointed president the year before, and he began to put together his faculty according to the trustees' plan. He was so successful that Peirce could announce, in his Fourth of July address to Americans in Paris in 1880 (item 16), that Johns Hopkins was unique among American universities in that "it has here alone been recognized that the function of a university is the production of knowledge, and that teaching is only a necessary means to that end." In its first four years, the published

[13]Quoted in Fisch (1986), p. 129.

results of research done at Johns Hopkins nearly equaled the total research output of all American universities for the preceding twenty years.

Eighty-nine students were enrolled in 1876 and, three years later, when Peirce took up his appointment, enrollment reached 159. Many of the early students had already taken degrees from other universities, and at Hopkins they sought advanced degrees. Johns Hopkins was the first university in America to offer the doctorate and, from the beginning, it attracted many brilliant students. Of the fifty or so who studied with Peirce, some who stand out include John Dewey, Fabian Franklin, Benjamin Ives Gilman, Joseph Jastrow, Christine Ladd (Franklin), Allan Marquand, Oscar Howard Mitchell, and Thorstein Veblen. Christine Ladd, with whom Peirce kept in touch throughout his life, was among the most gifted of his students. The admission of a woman for an advanced degree was remarkable for the times, and it required pressure from Sylvester and Benjamin Peirce. But when time came to confer Ladd's doctorate, the trustees refused to do so because she was a woman, and it was not until many years later that she received her degree.

The Johns Hopkins was an intimate community during this period, for besides the students, the number of professors, lecturers, associates, and instructors ran to only about forty. Peirce stood out in these circumstances. In his life of Gilman, Fabian Franklin remarks that "the singular genius of Charles S. Peirce was made a source of remarkable intellectual stimulation in the University"[14] and Christine Ladd reported that in the classroom "Peirce . . . had all the air . . . of the typical philosopher who is engaged, at the moment, in bringing fresh truth by divination out of some inexhaustible well."[15] When Sylvester asked one of his students to tell him about Peirce's lectures, he was informed that they "were always substantial, often very subtle, never trite, not easy to follow, frequently so lacking in clearness that the hearers were quite unable to understand"; but the student added that "there can be no question that Mr. Peirce is a man of genius." "Well," Sylvester replied, "if he is a genius, isn't that enough? Isn't it men of genius that we want here?"[16]

Sylvester, too, was a man of genius and the most distinguished professor during the university's early years. Although he had been shut out of university life in England, his reputation as a mathematician was of the first order. He had once held a post at the University of Virginia but had been forced to resign after an unfortunate incident with a violent student. Benjamin Peirce, perhaps the only mathematician in America who truly comprehended Sylvester's greatness, had urged Gilman to appoint him. Gilman had hesitated because he thought that Sylvester might be "hard to get on with"[17] but came to realize that he was precisely the kind of stimulating intellect needed to ignite the minds of advanced students. Sylvester was on

[14] *Life of Daniel Coit Gilman* (New York: Dodd, Mead & Co., 1910), p. 239.

[15] "Charles S. Peirce at the Johns Hopkins," *Journal of Philosophy* 26 (1916): 716.

[16] Reported by Cassius J. Keyser in "Charles Sanders Peirce as a Pioneer," *Galois Lectures* (*Scripta Mathematica* Library: No. 5, 1941), p. 94.

[17] Daniel C. Gilman, *The Launching of a University* (New York: Dodd, Mead & Co., 1906), p. 66.

the faculty when classes began in 1876. When he left seven years later to become Savilian Professor at Oxford, Gilman was probably beginning to reach his limit with the difficult natures of men of genius. He had just seen Sylvester and Peirce through a troublesome public quarrel and he now had to deal with the revelations and deliberations that would lead to Peirce's dismissal not long after.

Sylvester fully lived up to Gilman's expectations. Under his leadership Hopkins became the center of mathematical research in America; in fact, it might be said that American mathematics, as a true contender on the world stage, was born there during Sylvester's tenure. (Earlier, perhaps only the work of Benjamin Peirce had gained international respect.) Although it may have been in the classroom that Sylvester sowed the seeds for the mathematical harvest that would follow, it was his founding of the *American Journal of Mathematics* (again with the help of Benjamin Peirce) that quickly put Johns Hopkins at the center of mathematical thought. With the very first issue in 1878 the *Journal* became the forum for original mathematical research in America, and it served to connect American work with work from abroad.

Although it was Sylvester who galvanized the mathematical community at Hopkins, he was by no means the only creative force. Sylvester had helped persuade Peirce and Thomas Craig to stay on at Hopkins—as Coast Survey employees they were finding it difficult to fulfill the duties of two offices—and in March 1881 he wrote to Gilman:

> Allow me to express the great satisfaction I feel in the interest of the University at the measures adopted by the Trustees to secure the continuance of Craig and Peirce. We now form a corps of no less than eight working mathematicians—actual producers and investigators—real working men: Story, Craig, Sylvester, Franklin, Mitchell, Ladd, Rowland, Peirce; which I think all the world must admit to be a pretty strong team.

And when Professor Arthur Cayley of Cambridge University came as a visiting lecturer from January to June 1882, it is doubtful that as much sheer mathematical power was so concentrated anywhere else.

The other Hopkins professors during Peirce's time were Basil L. Gildersleeve (Greek), Newell Martin (biology), Charles D. Morris (Latin and Greek), Ira Remsen (chemistry), and Henry A. Rowland (physics). Peirce seems to have had little interaction with Gildersleeve, Martin, Morris, and Remsen, although all except Morris read papers to the Metaphysical Club, which Peirce presided over for several terms. In the spring of 1880, Gildersleeve travelled to Europe with Sylvester and Peirce, and on 15 July wrote to Gilman from Paris that he had been seeing a good deal of Peirce, who "has been kind to me in his way, and if he were always as he can be sometimes, he would be a charming companion." But apparently no regular friendship developed. Relations were much closer with Rowland, chairman of the Physics Department, the position Peirce had sought in January 1878. Peirce often saw Rowland at the meetings of the Johns Hopkins Scientific Association and the Mathematical Seminary and he frequented and probably used Rowland's laboratory. When Rowland undertook to map the solar spectrum he used the results of Peirce's work on the wave-length of light, which, combined with the results of Ångström and Louis Bell (Rowland's assistant), gave him his table of solar spectrum wave-lengths that served as the world standard for a generation.[18]

Three lecturers at Johns Hopkins must be mentioned as influential in Peirce's career: G. Stanley Hall, George S. Morris, and Simon Newcomb. The first two were on the philosophy faculty and taught in alternate half years. Morris taught ethics and the history of philosophy and Hall taught courses in psychology and developed the psychological laboratory. Although Morris, Hall, and Peirce were rivals for the philosophy professorship, there seems to have been no animosity among them, and Peirce's relations with Hall, who for a time lived just across the street from him, were quite friendly. They both had an active interest in experimental psychology and they appreciated each other's work. In an 1879 article in *Mind* on "Philosophy in the United States," Hall had praised Peirce as "a distinguished mathematician" whose *Popular Science Monthly* "Illustrations" promised to be "one of the most important of American contributions to philosophy."[19] In 1884, when Hall was chosen over Peirce and Morris (and also William James) for the philosophy professorship, he expressed surprise: "Each of the three was older and abler than I. Why the appointment, for which all of them had been considered, fell to me I was never able to understand unless it was because my standpoint was thought to be a little more accordant with the ideals which then prevailed there."[20] Hall went on, in 1889, to become president of Clark University which he modeled after the Johns Hopkins. Peirce visited him there at least twice.

Simon Newcomb, a protégé and friend of Benjamin Peirce, was well-known to Charles. Their paths had often crossed, in and out of the Peirce home, and would continue to cross for years. They corresponded for over thirty years, with Peirce's last letter to Newcomb dated 7 January 1908.[21] But more often than one might expect of a presumed friend, and more often than anyone realized, Newcomb took actions that damaged Peirce. Three incidents stand out. The first concerns Newcomb's role in the events leading to Peirce's dismissal, which will be discussed later. The second occurred after Peirce's dismissal when Newcomb had succeeded Sylvester as editor of the *American Journal of Mathematics.* The first part of Peirce's "Algebra of Logic" (P 296), which had been accepted for publication by Sylvester, appeared in the *Journal* in 1885, and part 2 was to follow in the next issue. Confident that it would be published, Peirce had duly submitted it, but Newcomb rejected it on the ground that its subject was not mathematics. Given that in the first part Peirce had introduced quantifiers into his system

[18]Fisch (1986), pp. 63–64.

[19]"Philosophy in the United States," *Mind* 4 (1879): 101f.

[20]*Life and Confessions of a Psychologist* (New York: D. Appleton Co., 1923), p. 226.

[21]See "The Correspondence with Simon Newcomb," in *Studies in the Scientific and Mathematical Philosophy of Charles S. Peirce*, ed. R. M. Martin (The Hague, Paris, New York: Mouton, 1979). For Peirce's last letter to Newcomb see pp. 86–88.

of logic, as well as truth function analysis, Newcomb's rejection can only be seen as a great misfortune for Peirce and for logic. The third incident occurred years later when Newcomb was asked to review a scientific monograph that Peirce had prepared for publication for the Coast Survey—the report on gravity at the pendulum stations Peirce began occupying in 1885. He had spent years reducing his data and writing this report and he expected it to be a major contribution. But two of three reviewers recommended that it not be published, with Newcomb's negative appraisal perhaps the deciding one. The rejection of Peirce's report contributed to the decision to ask for his resignation from the Coast Survey. It is ironic that in his last letter to Newcomb, Peirce asked that he put in a good word for him at the *Nation,* which had long been an important source of income for Peirce, "if you are disposed to do me such a good turn."

In his five years at Johns Hopkins, Peirce taught logic courses each semester, often both elementary and advanced courses. He also taught special courses on the logic of relatives, medieval logic, philosophical terminology, and probabilities, as well as a course on the psychology of great men. Never before in America—nor anywhere else, save perhaps at Aristotle's Academy in Athens—had a logician of such power developed a program of research with such capable students. It seemed certain that Gilman would see the results he had hoped for when he took a chance with Peirce. That expectation was widespread. According to John Venn:

> Mr. C. S. Peirce's name is so well known to those who take an interest in the development of the Boolian or symbolic treatment of Logic that the knowledge that he was engaged in lecturing upon the subject to advanced classes at the Johns Hopkins University will have been an assurance that some interesting contributions to the subject might soon be looked for.[22]

Venn was reviewing the 1883 *Studies in Logic,* of which he said that "such assurance is justified in the volume under notice, which seems to me to contain a greater quantity of novel and suggestive matter than any other recent work on the same or allied subjects which has happened to come under my notice."

Peirce's involvement in the life of the university extended far beyond the classroom. He attended the meetings of the Mathematical Seminary and the Scientific Association and occasionally contributed papers. Not long after he had arrived at Johns Hopkins, he instigated the founding of the Metaphysical Club, perhaps inspired by his memory of the old Cambridge Metaphysical Club. He had conceived it, according to Christine Ladd, in this way: "So devious and unpredictable was his course that he once, to the delight of his students, proposed at the end of his lecture, that we should form (for greater freedom of discussion) a Metaphysical Club, though he had begun the lecture by defining metaphysics to be 'the science of unclear thinking'."[23] At the first meeting, on 28 October 1879,

[22]*Mind* 8 (1883): 594–603.
[23]"Charles S. Peirce at the Johns Hopkins," 717.

Peirce was elected president and Allan Marquand secretary, and six papers were read and discussed. According to the minutes of the second meeting, the club was to meet each month during the academic year, and the standard order of business was to be as follows:

1. Reading of Minutes.
2. Reading & discussion of a *Principal* Paper, the delivery of which shall not exceed forty-five (45) minutes.
3. Papers deferred from previous meetings.
4. Reading & discussion of *Minor* Communications, the delivery of which shall not exceed twenty (20) minutes.
5. Reviews of books & magazines.
6. Transaction of business.
7. Adjournment

Peirce served as president for about half the club's life, the other half being divided between Hall and Morris. He attended nearly two-thirds of the meetings and as late as 13 May 1884, long after it was known that his contract would not be renewed, he presided over the thirty-ninth meeting in the absence of Hall. He delivered his final paper to the club at its fortieth meeting on 18 November. By this time Hall had been appointed to fill the philosophy position as professor of psychology and pedagogy, and he recommended at the fortieth meeting that the Metaphysical Club should be reorganized to reflect the changes in the philosophy program. The club met only three more times, expiring with the forty-third meeting of 3 March 1885, not long after Peirce's departure.

It is not surprising that most of Peirce's research during the period of this volume, except for science, closely follows the paths marked out by his Hopkins courses and activities. Even the impact of his father's death on his program of research was influenced by Sylvester, who urged him to edit *Linear Associative Algebra* for publication in the *Journal.* Peirce's interest in carrying on some of his father's mathematical work became much intertwined with interests related to the mathematical community at Hopkins, which included some of his best logic students.

During his first semester he taught a general logic course that met three times a week for three months and a course in medieval logic which met only once a week. Fourteen students took general logic, including three who would make contributions to *Studies in Logic:* B. I. Gilman, Ladd, and Marquand. It was the lectures for this course that Peirce was preparing when on 27 July he wrote to D. C. Gilman that "you would be amused if I were to say that they were very fine." Earlier in the letter, Peirce had expressed some anxiety about the coming term:

> I have a good deal of confidence & a good deal of diffidence about my instruction in Logic. The former about the ultimate result if I succeed in pleasing you the first year, the latter about the first year. Logic is peculiar in this respect that it is not so much a body of information as it is knowing how to use the mind. That is why the Socratic method ought to be followed as much as possible. But then it is extremely difficult to make that method work right.

From lecture notes and course descriptions, and from class notes taken by Allan Marquand and other students, we can get a fairly clear picture of what Peirce's courses were like and what he was like as a teacher. Christine Ladd-Franklin speaks of the eagerness of Peirce's students for intellectual adventure and their receptiveness "to the inspiration to be had from one more master mind."

He sat when he addressed his handful of students (who turned out afterwards, however, to be a not unimportant handful) and he . . . got his effect not by anything that could be called an inspiring personality, in the usual sense of the term, but rather by creating the impression that we had before us a profound, original, dispassionate and impassioned seeker of truth.[24]

Joseph Jastrow reports that "Peirce's courses in logic gave me my first real experience of intellectual muscle." He goes on to speak of Peirce's "fertile suggestiveness" and then of his personality.

Mr. Peirce's personality was affected by a superficial reticence often associated with the scientific temperament. He readily gave the impression of being unsocial, possibly cold, more truly retiring. At bottom the trait was in the nature of a refined shyness, an embarrassment in the presence of the small talk and introductory salutations intruded by convention to start one's mind. His nature was generously hospitable; he was an intellectual host. In that respect he was eminently fitted to become the leader of a select band of disciples. Under more fortunate circumstances, his academic usefulness might have been vastly extended. For he had the pedagogic gift to an unusual degree. . . .

The young men in my group who were admitted to his circle found him a most agreeable companion. The terms of equality upon which he met us were not in the way of flattery, for they were too spontaneous and sincere. We were members of his "scientific" fraternity; greetings were brief, and we proceeded to the business that brought us together, in which he and we found more pleasure than in anything else.[25]

In reflecting on the courses she had taken with Peirce, Christine Ladd-Franklin remarked that "His lectures on philosophical logic we should doubtless have followed to much greater advantage if he had recommended to us to read his masterly series of articles on the subject which had already appeared in the *Popular Science Monthly.*"[26] But Marquand's notes of Peirce's first classes show that, even if his "Illustrations" were not required reading, he often referred to them and spent his first three lectures discussing such topics as doubt and belief, methods of fixing belief, and degrees of clearness of ideas. This was the early part of the course Peirce called prolegomena, which continued through the eleventh class on 3 November. The final four paragraphs of Marquand's notes on lecture 11 show Peirce's concluding emphasis for this part of the course:

[24]*Ibid.*, 716.

[25]Joseph Jastrow, "Charles S. Peirce as a Teacher," *Journal of Philosophy* 26 (1916): 724–25.

[26]"Charles S. Peirce at the Johns Hopkins," 717.

Various forms of investigation of the same subject converge to one result. Eg on velocity of light. This gives a real significance—a finality to truth. It is no (made up) figment, but a reality.

We do not *make Reality independent of thought altogether*, but only of the *opinion* of you I or any other man. We may adopt a false opinion, this only delays the approach of the true.

Truth we may call a *predestinate opinion*—sure to come true. Fatalism proper assumes events as sure to come to pass, no matter what we do about it. But our reaching this opinion tomorrow or next year does depend upon what we do. Its characters nevertheless are independent of our opinion.

To say that real things influence our minds & that opinion will finally become settled—one & same. No explanation to say we come to same conclusion because real things influence our minds. We come to this final opinion by a *process*. What is that process, is the problem of Logic which we now consider.

Peirce continued the course with a lecture and a half on his theory of signs, taken mainly from his *Journal of Speculative Philosophy* series of 1868 (*W*2), and then he took up formal logic, which he divided into syllogistic, the theory of logical extension and comprehension, quantification of the predicate, and the algebra of logic. The first three topics took Peirce to the end of the term (of thirty lectures). The algebra of logic was reserved for the second term.

Peirce's lectures on formal logic were based in part on his 1867 American Academy series (*W*2), but many new issues were developed which helped set the course for future work. For example, in order to examine reasoning in the theory of numbers, Peirce developed an axiomatic treatment of elementary number theory. In his 17 December lecture he gave the following seven premises:

1. Every number by process of increase by 1 produces a number.
2. The no. 1 is not so produced.
3. Every other number is so produced.
4. The producing & produced nos. are different.
5. In whatever transitive relation every no. so produced stands to that which produces it, in that relation every no. other than 1 stands to 1.
6. What is so produced from any individual no. is an individual no.
7. What so produces any individual no. is an individual no.

Then, after specifying his notation and defining the relations "greater than" and "not greater than," he went on to develop examples. Items 24 and 38 show that Peirce continued to refine his basis for natural numbers.

Marquand's notes illustrate that Peirce used his classes to work through material that he was preparing for publication—or that what he prepared for his courses ended up in print. Several of Peirce's important writings on logic from this period correspond to the content of his courses. This is true of "On the Logic of Number" (item 38) as well as of "On the Algebra of Logic" (item 19). When Peirce began the second half of his first logic course on 12 January 1880, he indicated that he would be dealing with Boole's and Schröder's work and with his improvements on Boole. He also mentioned work by Leslie Ellis and his own "Logic of Relatives" and De Morgan's 1860 paper on the syllogism. But the material Peirce discussed in his winter 1880 classes was developed quite beyond his algebras of 1867 and 1870. In the fall or winter of 1879, Peirce had worked out a systematic treatment of the algebra of logic entitled "On the Algebraic Principles of Formal Logic" (item 6). Although this work is fragmentary, it suggests a systematic presentation of the algebra of logic that may have served both as an outline for his class lectures and for item 19. Even though item 6 is no doubt an early version of item 19, it is of interest to look at some of the differences. In item 6 Peirce still employed his 1870 notation, using the claw ($\prec$) as his sign for general inclusion, "$+\!\!,$" for logical addition, and "," for logical multiplication. In item 19, however, he has replaced the "$+\!\!,$" with the simpler "$+$" and the "," (for logical multiplication) with "$\times$" (or with mere conjunction) though he retains his claw, as he will for the rest of his life (except in his graphical and iconic notations). The most powerful rule in the earlier system is a principle of duality that permits the assertion of a dual form for every well-formed expression. This rule is not present in the item 19 system but in its place is a new, more powerful (and considerably more important) rule, related to the deduction theorem, that permits the assertion of inferences as inclusions and vice versa. As a general expression of this powerful rule Peirce asserted the identity of the relation expressed by the copula with that of illation, and said that this identification gives us the principle of identity ($x \prec x$) and shows that the two inferences

$$\begin{array}{l} x \\ y \\ \therefore z \end{array} \qquad \text{and} \qquad \begin{array}{l} x \\ \therefore y \prec z \end{array}$$

are of the same validity. By this rule modus ponens and conditional proofs are legitimized in item 19, but they are no part of the earlier work. Otherwise the systems bear marked similarities.

Item 19 did not appear in print until September 1880, though Peirce had completed it by April when he left for Europe on assignment for the Coast Survey. Thus within a few months' time, six at most, his system had evolved in the ways indicated above. Notably, what came in between was his first course in logic. We know from Marquand's notes that as early as 12 November 1879 Peirce had asserted that "the *Copula* expresses a *transitive* relation" and that on 3 December he pointed out that "later in theory than Syllogistic—springs also as all Logic, from transitiveness of Copula" and "we have already identified the illative sign with the transitiveness of the copula. $A \therefore B$ & $A \prec B$. The resemblance more important than the difference." Although it is impossible to say how much Peirce's interaction with his students influenced his writings, the example of item 19 (which is one of several that could have been given) is very suggestive of the sort of synergism that one might expect between a good teacher and good students.

Another topic that occupied Peirce during the winter of 1879 was the relationship between thinking and cerebration (or logic and physiology in his first logic course). Two versions of a paper on the subject, included in the present volume, are first chapters of a work on logic, perhaps the book he was preparing from his "Illustrations." This is suggested by the fact that one version of the paper (item 7) moves into a discussion of the settlement of opinion that is taken almost verbatim from the first "Illustration" (*W*3:242–57), although both papers appear to be early versions of the first section of item 19. Perhaps Peirce had it in mind to somehow combine his "Illustrations" with his 1879–80 work on the algebra of logic and to make that his logic book for the International Scientific Series.[27] It should also be noted that Peirce began his first logic course with a discussion of the connection between logic and physiology.

Five students were enrolled in Peirce's course in medieval logic, described in the Hopkins *Circulars* as "A course of lectures on Medieval Logic, designed to show the spirit and leading doctrines of the logic of the Middle Ages." Peirce had made a thorough study of the history of logic and was probably the most knowledgeable American in medieval logic, and his collection of medieval logic texts was unsurpassed in America. While he was teaching medieval logic, he also directed Marquand's study of Epicurean logic, especially of the Herculaneum papyrus of Philodemus's "On Methods of Inference." On Peirce's recommendation Marquand made the first English translation, which he submitted along with a commentary, as his doctoral dissertation. A paper by Marquand on Epicurean logic, possibly the commentary part of his (lost) dissertation, was included in *Studies in Logic.* Peirce's own study of Epicureanism, in guiding Marquand, may have planted the seed that a few years later, fed by his developing evolutionism, grew into his paper on "Design and Chance," the seed being the Epicurean doctrine of absolute chance, the view that a place for freedom was afforded by the uncaused swerve of atoms.[28]

During the same term Peirce gave a paper to the Metaphysical Club on 11 November on "Questions Concerning Certain Faculties Claimed for Man" and, on 3 December he spoke to the Scientific Association on the four-color problem (he is reported to have suggested improvements to the method of demonstration employed by A. B. Kempe).[29] Before the year was out, he reviewed Vol. 2, No. 3, of the *American Journal of Mathematics* for the *Nation.* He remarked that Hall's discovery (at Johns Hopkins) of the effect of a magnetic field on electric current (the Hall effect) could hardly be overestimated, and he took special note of Sylvester's stress on the importance of observation for the discovery of mathematical laws by saying that "there has been, perhaps, no other great mathematician in whose works this is so continually illustrated."

At the end of his first term Peirce wrote the 25 December letter to Gilman about his "state of dangerous cerebral activity & excitement." He returned in January to begin a very unsettling year, albeit one of remarkable achievement. While confined to his quarters with bronchitis during the first months of 1880, he wrote "On the Algebra of Logic (item 19)," in which he

[27]Edward L. Youmans, editor of the *Popular Science Monthly,* where Peirce's six "Illustrations" appeared, hoped to combine them with additional illustrations in a book for his International Scientific Series.

[28]Fisch (1986), pp. 235–37.

[29]*Johns Hopkins University Circulars* 1 (1880): 16.

produced a system of logic that with only slight augmentation provides a complete basis for logic.[30] This paper is part of a series in which Peirce set out his logic of relatives and, as Tarski noticed, "laid the foundation for the theory of relations as a deductive discipline."[31] It is in this paper that Peirce begins to loosen the ties between his logic of relatives and mathematical analysis. Item 19 also gives Peirce a place in the development of the mathematical theory of lattices, although his rôle in the foundation of lattice theory is somewhat controversial (it cannot be said unequivocally that Peirce ever fully comprehended the idea of a lattice). The controversy stems from Peirce's claim that the full law of distribution could be proved within his 1880 system with the implication that all lattices are distributive. Peirce declined to give his proof on the ground that it was too "tedious." Schröder and others (Voigt, Lüroth, Korselt, and Dedekind)[32] countered with proofs of the independence of the law of distribution but, after at first conceding, Peirce came back in 1903–04 in support of his original claim. His distributivity proof, first written out in his logic notebook (Robin MS 339, p. 437) on 31 January 1902, was published by Edward V. Huntington in 1904[33] and later by C. I. Lewis in his *Survey of Symbolic Logic.*[34] There is disagreement about the desirability of modifying the basis of Boolian algebra as Peirce did in order for his proof to go through.[35] An earlier proof given in Peirce's 1879 "Algebraic Principles" (item 6) has not yet been examined by mathematicians.

Also in 1880 he wrote his short "A Boolian Algebra with One Constant" (item 23), in which he anticipated H. M. Sheffer's paper of 1913 that introduced the stroke function.[36] He also continued his work on number theory and in the winter following his father's death began working in earnest on associative algebras. By the end of the year Peirce had sketched out his proof that, in the words of Eric Bell, "the only linear associative algebras in which the coordinates are real numbers, and in which a product vanishes if and only if one factor is zero, are the field of real numbers, the field of ordinary complex numbers, and the algebra of quaternions with real coefficients."[37] The proof appeared as an appendix to his edition of *Linear Associative Algebra* (item 42).

[30]See Arthur N. Prior, "The Algebra of the Copula," in *Studies in the Philosophy of Charles Sanders Peirce,* 2nd series, ed. Edward C. Moore and Richard S. Robin (Amherst: University of Massachusetts Press, 1964), pp. 79–94; especially pp. 88–92.

[31]Alfred Tarski, "On the Calculus of Relations," *Journal of Symbolic Logic* 6 (1941): 73.

[32]V. N. Saliĭ, *Lattices with Unique Complements,* tr. G. A. Kandall (Providence, RI: American Mathematical Society, 1988), p. vii.

[33]"Sets of Independent Postulates for the Algebra of Logic," *Transactions of the American Mathematical Society* 5 (1904): 288–309.

[34]*Survey of Symbolic Logic* (Berkeley: University of California Press, 1918).

[35]See Saliĭ (1988), pp. 36ff.

[36]"A Set of Five Independent Postulates for Boolean Algebras, with application to logical constants," *Transactions of the American Mathematical Society* 14 (1913): 481–88.

[37]*The Development of Mathematics,* 2nd ed. (New York: McGraw-Hill, 1945), p. 250.

The Metaphysical Club was especially active during the first half of 1880 with about twenty presentations, and a special meeting was called in May for Josiah Royce's "On Purpose in Thought," read in his absence. On 9 March Peirce had presented "On Kant's *Critic of the Pure Reason* in the light of Modern Logic," which appears to be one of the few papers in this period focussing directly on the fundamental philosophical questions which Peirce had developed in his 1867 American Academy series but which he would not take up again for several years. The following abstract of the paper appeared in the April *Circular:*

> Mr. Peirce compared Kant's solution of the problem "How are synthetical judgments *à priori* possible?" with the solution given by modern logic of the problem "How are synthetical judgments in general possible?" He showed that the reply which Kant makes to the former question has its analogue with reference to the latter. This analogous answer to the second question is true, indeed, but is far from being a complete solution of the problem. On the other hand, the solution which modern logic gives of its question may be successfully applied to Kant's problem; but this does not enable us to discover the origin of the conceptions of space and time. The categories of Kant were next considered. The list given by him is built upon the basis of a formal logic which subsequent criticism has undermined and carried away. Nevertheless there really do exist relationships between some of those conceptions and logic on the one hand and time on the other. The explanation of these relationships in conformity with modern logic, though far more definite than that of Kant, is not altogether dissimilar to it.

An impressive record of the fertility of Peirce's mind in 1880 can be found in a notebook, probably written during the summer while he was in Paris. Entitled "Logic of Relatives," MS 364 contains a remarkable set of ideas and developments, including notes on alternative copulas where Peirce first set out the idea for his single connective Boolian algebra, some suggestive moves toward his quantifier notation, a new set of seven axioms of number based on the "greater than" relation, and notes on his relative of simple correspondence that he used for his treatment of finite collections (see item 38). It is possible that, like items 20 and 22, these are notes toward a continuation of item 19, a continuation that was sidetracked by his father's death and by Schröder's criticisms of his distribution claims. As might be expected, the ideas Peirce developed in the summer made their way into his logic classes in the fall.

Peirce had begun 1880 teaching the second half of his first general course on logic, as well as a two-month course in probabilities. In connection with the latter he probably wrote his notes entitled "A large number of repetitions of similar trials" (item 14). But his courses appear to have been cut short by the illness that gave him the opportunity to finish item 19 before leaving for Europe in April.

Peirce had returned by 5 August and remained in, or not far from, Cambridge until after his father's death on 6 October. Although he had originally thought to skip the fall term at Johns Hopkins (he had been authorized to stay in Europe until January), he now prepared for the full academic year. On 19 August he wrote to Gilman about his upcoming lectures:

I wish to extend them through the whole year if possible, & if Patterson consents. I expect to make two courses, one very elementary and practical, the other to take up first the algebra of logic, then probabilities, and finally inductive logic. I have this summer made a discovery in logic which seems to me to be really important. I shall develop it in an early number of the Journal of mathematics; and shall explain it in my lectures.

The "Logic of Relatives" notebook (MS 364) provides clues to what this discovery might have been: his successful axiomatization of the natural numbers or his definition of finite sets (item 38); his "A Boolian Algebra with One Constant" (item 23); or it might have involved quantification or truth values. Peirce continued his letter with a remark about "On the Algebra of Logic," which would soon be in print. "This paper which is appearing in the Journal will probably be in 3 parts and will cover over 100 pages. The first part appears in the number which is nearly ready. I think it would be well for me to put some of my copies on sale at Cushing & Baily's for the convenience of my students."

Peirce returned to Baltimore after his father's death to begin his two fall courses: elementary logic, which met twice a week with an enrollment of five, and advanced logic, which met three times a week with an enrollment of seven. Among the seven were all the contributors to *Studies in Logic* as well as Sylvester's favorite student, Fabian Franklin. The text for the first part of the advanced course (item 19) had been issued in September. One of Peirce's assignments appears to have been the preparation of class notes, or notes on the text, to be handed in for his scrutiny and comments. Christine Ladd's notes reveal an intensive study of item 19, especially with regard to his extension of De Morgan's eight propositional forms. Peirce had remarked that if we admit "particularity of the predicate," the system of propositions must be enlarged; but he did not say how many propositional forms there would be in the completed system. In one of the early classes in the fall term he showed that there are fifteen states of the universe for two terms; he did not yet consider the empty universe as a sixteenth state. Ladd made an elaborate study of this matter and struggled with the problematic empty universe. Taking a hint from Fabian Franklin's application of binary notation to logical formulae, she worked out binary numbers for all the value combinations for two terms. Though reluctant at first, she felt compelled for reasons of symmetry and completeness to include the null case. By the time she read an early version of her *Studies in Logic* paper to the Metaphysical Club in January 1881, she had fully overcome her reluctance to imagine an empty universe. A table in that paper gives "the sixteen possible constitutions of the universe with respect to two terms," which is in effect the complete truth-table for the sixteen binary connectives (probably making its first appearance in print[38]).

Peirce had resigned the presidency of the Metaphysical Club before

[38]See also, however, Mitchell's table on p. 75 of his paper in *Studies in Logic* ("On a New Algebra of Logic") and Peirce's table on p. 442 of his principal contribution ("A Theory of Probable Inference," item 64).

leaving for Europe, thinking he would be away until January, but he was reelected in the fall when he returned early. On 14 December 1880 he suffered from a severe headache and sent a note to be read in his absence at the meeting that evening. He reported that he had made contact with the secretary of the Leipzig Academical Philosophical Club, who sought to establish a "better acquaintance between the Clubs," and that he had "lately received papers from professors Wundt, Schröder, J. J. Murphy, Venn, Jevons, MacColl, and others on various logical and psychological subjects." With his fellows club members, Peirce was in the inner circle of logic.

Yet at the height of his success as a logician he had not settled on a career in logic. His success as a scientist, combined with the pressures of his duties for the Coast Survey, had something to do with his hesitation to commit himself to logic, as did his father's advice that he stick with science, but probably the main reason was his unhappiness with his part-time status at Johns Hopkins. On 18 December he wrote to Gilman that he intended to leave the university in the spring because of the difficulty with conducting two careers at once and that, given his "subordinate position" at Johns Hopkins, he was unwilling to modify his connection with the Coast Survey. He intended to abandon the study of logic and philosophy and offered to sell his library (on those subjects) to the university for $550. Before the week was out Gilman accepted Peirce's offer and, in his commencement day address on 22 February, he lauded Peirce and remarked on the importance of his collection.[39] But Peirce did not quit logic and philosophy and he soon deeply regretted the loss of his books. By November 1883 his efforts to secure special volumes for his research and his courses—most notably the Berlin *Aristotle*—and his attempt to buy back some of the books he had sold to the library had become a source of irritation to the library committee and of personal offense to Gilman.

When Peirce resumed teaching in January 1881 for his fourth term he expected it to be his last; for by 7 February the trustees had accepted his decision to leave. Had his elementary logic course with three students and his advanced course with six (including, again, B. I. Gilman, Ladd, and Marquand) been his last, he might have avoided the erosion of his welcome at Johns Hopkins as well as the scandal of his dismissal, which closed academic doors later on. But by the end of March, Sylvester had prevailed on Gilman to keep Peirce (and Craig) and the trustees had agreed to raise his salary from $1500 to $2500. Peirce agreed to stay on, and soon he was again deeply engaged in his logical researches.

1881 was a very productive year for Peirce, especially in logic. Probably in the spring, in connection with his advanced logic class, he wrote his paper on the theory of probable inference, which would later be included in *Studies in Logic* (item 64), and in the summer he wrote "On the Logic of Number" (item 38) where, several years before the equivalent axiomatizations of Dedekind and Peano,[40] he gave his successful axiomatization of the natural

[39]See Fisch (1986), pp. 52–53.
[40]Paul Shields, "Charles S. Peirce on the Logic of Number," Diss. Fordham 1981.

numbers. Near the end of the year he composed his "Proof of the Fundamental Proposition of Arithmetic" (item 36) in which he proved, using De Morgan's syllogism of transposed quantity to define a finite collection (as he had in item 38), that the sequential order of objects counted does not affect the count (the outcome of the counting). In November he read "On the logic of number" (P 200) before the National Academy of Sciences. In the meantime, he had continued work on his father's *Linear Associative Algebra,* with commentary and further developments of his own (items 26 and 27), including his proof that there are only three linear associative algebras in which division is unambiguous, which he presented to the Mathematical Seminary in January. He also continued his "Logic," in which he probably intended to include his "Illustrations," the papers on thinking as cerebration (items 7 and 8), and "On the Algebra of Logic" (item 19) and its projected continuation. Two other papers included here seem also to belong in this group: "Logic; and the Methods of Science" (item 30) and "Methods of Reasoning" (item 31), which provide an important link between item 19 and the 1885 article "On the Algebra of Logic."

An examination of the early volumes of the *American Journal of Mathematics* reveals that many of the contributions are entitled "Note on . . . " or simply "On . . . " and it is quite probable that many of Peirce's short manuscripts of this period that have such titles were written with the *Journal* in mind. A number of these pieces did appear there (items 10, 19, 38, 41, and 42) although at least three of them are more substantial than ordinary notes, and several others (items 5, 15, 18, and 44) appear elsewhere, though they too may originally have been written for Sylvester's *Journal.* Even Notes A and B in *Studies in Logic* may have been intended at first for the *Journal,* along with items 6, 32, and 33. But it is also possible that some of these papers were written for presentation at one or another of the Johns Hopkins clubs, for many of their presentations had such titles, including Peirce's "On Relations between Sensations" in April 1881 and Joseph Jastrow's "A Note on Mechanical Light" in April 1883.

Peirce was president of the Metaphysical Club for all of 1881 but was absent for two of its six meetings. At the meetings he attended he heard ten papers by, among others, Ladd, Franklin, Davis, Marquand, B. I. Gilman, and G. S. Morris. These were mainly on logic (three were on induction) and psychology, but one by Burt was on Hegel's *Philosophical Propaedeutic* and Morris's was on "English Deism and the Philosophy of Religion." In November Peirce gave a paper entitled "A Fallacy of Induction" before the Scientific Association in which he examined some of Priestley's inferences concerning atomic weights and specific heats.[41]

Peirce's courses in the fall of 1881 had unusually low enrollment with only three students both in elementary and advanced logic. (Thorstein Veblen was in the elementary course, and Davis, B. I. Gilman, and Mitchell in the advanced.) The courses were described in the July *Circular* as follows:

[41]Ellery W. Davis, "Charles Peirce at Johns Hopkins," *Mid-West Quarterly* 2 (1914): 48–56.

1. An elementary course on *General Logic,* deductive and inductive, including probabilities. This course will be designed to teach the main principles upon which correct and fruitful reasoning must proceed; and special attention will be paid to the discussion of the significance and validity of those logical conceptions and maxims which are current in literature and in law.
2. A course upon the methods of science. A sketch of deductive logic and the theory of relative terms will lead to the study of the methods of Mathematics. The theory of chances and errors will next be expounded. Lastly, after the development of the general doctrine of induction and hypothesis, the methods of reasoning in several of the physical and moral sciences will be examined in detail.

By the end of 1881 Peirce was again fully committed to logic both as investigator and teacher, and his reputation was now such that his work was noticed almost as soon as it appeared. To his Preface in his *Studies in Deductive Logic,* dated 3 October 1880, W. Stanley Jevons added the following paragraph:

> To the imperfect list of the most recent writings on Symbolical Logic, given in this preface, I am enabled to add at the last moment the important new memoir of Professor C. S. Peirce on the Algebra of Logic, the first part of which is printed in the *American Journal of Mathematics,* vol. iii (15th September, 1880). Professor Peirce adopts the relation of *inclusion,* instead of that of *equation,* as the basis of his system.[42]

Peirce's paper (item 19) had been out less than three weeks. John Venn noticed the same paper at the 6 December 1880 meeting of the Cambridge Philosophical Society, in particular Peirce's notation (which Venn placed just before Frege's).[43] But perhaps the most satisfying notice came in the 24 March 1881 issue of *Nature* where, in a piece entitled "Recent Mathematico-Logical Memoirs," Jevons claimed that: "The most elaborate recent contributions to mathematico-logical science, at least in the English language, are the memoirs of Prof. C. S. Peirce, the distinguished mathematician, now of the Johns Hopkins University, Baltimore."

Peirce's classes in the spring of 1882 were better enrolled, for he had five students in each of his two regular classes, elementary and advanced logic. (Mitchell took both, and B. I. Gilman and Ladd repeated the advanced course.) Peirce also taught a short course on the logic of relatives, where items 45 and 46 may have originated (as well as Note B of *Studies in Logic*). Perhaps the best indication of what Peirce covered in his short course is his "Brief Description of the Algebra of Relatives" (item 43) which he composed in very short order at the beginning of the term, inspired by what he heard from his advanced logic students who were taking Sylvester's new course of lectures on universal multiple algebra. Peirce was convinced that Sylvester's universal algebra was only a case, or interpretation, of his own logic of relatives, and he decided to write out his system in a way that would demonstrate this relationship. He especially wanted to present his logic of relatives in a manner that would interest Sylvester. Peirce's "Brief Description" is

[42] *Studies in Deductive Logic* (London: Macmillan, 1880), p. xxiii.

[43] "On the various notations adopted for expressing the common propositions of Logic," *Proceedings of the Cambridge Philosophical Society* 4 (1883): 36–47. Reprinted in *Symbolic Logic* (London: MacMillan and Co., 1881).

dated 7 January and he had proof sheets in hand by the middle of the month. Even as he was writing his brochure he was in correspondence with Sylvester about some of the points he hoped to demonstrate. But Sylvester seems not to have been convinced—and he was not anxious to see the paper in print, as is evident from Peirce's 6 January 1882 letter:

> I lay no more claim to your umbral notation than I do to the conception of a square block of quantities! What I lay claim to is the mode of multiplication by which as it appears to me this system of algebra is characterized. *This* claim I am quite sure that your own sense of justice will compel you sooner or later to acknowledge. Since you do not acknowledge it now, I shall avail myself of your recommendation to go into print with it. I have no doubt that your discoveries will give the algebra all the notice which I have always thought it merited and therefore I hope my new statement of its principles will be timely. I cannot see why I should wait until after the termination of your lectures before appearing with this, in which I have no intention of doing more than explaining my own system & of saying that so far as I am informed it appears to be substantially identical with your new algebra, & that it ought to be, for the reason that mine embraces every associative algebra, together with a large class–perhaps all–of those which are not entirely associative. I am sorry you seem to be vexed with me.

Just the day before Peirce had written to Sylvester trying to explain the "precise relationship of your algebra of matrices to my algebra of relatives." He concluded that "It, thus, appears to me just to say that the two algebras are identical, except that mine also extends to triple & other relatives which transcend two dimensions."

Arthur Cayley had arrived at Johns Hopkins in December and in January began his half-year tenure as visiting lecturer. On 18 January he, Sylvester, and Peirce had delivered a special program of lectures to the Mathematical Seminary in celebration of Cayley's visit. Peirce's paper, "On the Relative Forms of Quaternions" (item 44), was commented on favorably by Sylvester. On 16 January Peirce had added a note to his brochure, which was then in press, stating that on that day, for the first time, he had read Cayley's 1858 *Memoir on Matrices.* He had discovered that his algebra of dual relatives had there been substantially anticipated although, he pointed out, "many of his results are limited to the very exceptional cases in which division is a determinative process." Peirce began to fear that his brochure might somehow offend Sylvester and Cayley. So on 7 February, when his printed copies arrived, Peirce sent one to President Gilman along with the following note:

> It occurs to me that it is possible that (although I am unable to see it at all) there may be some just cause of offense in my references on the last page to Professors Sylvester and Cayley. Of course, you will see none at first glance; but will you see them and find out 1st whether they think they see anything out of the way and 2nd whether if so it is merely the systematic arrogance of these Britishers or whether it is just. I will keep back the issue until I hear from you.

There must have been some objection, for Peirce never did distribute his brochure. But he no doubt taught its content in his course on the logic of relatives, and he used it in his logic class in the fall.

However frustrated Peirce may have been—on 7 January he wrote "Sylvester is a cad" in his diary, and in later years he remembered that he had "felt squelched"[44]—his relations with Sylvester continued seemingly undamaged. On 5 March he again wrote to him: "I have a purely algebraical proof that any associative algebra of order n can be represented by a matrix of order $n + 1$ having one row of zeros, together with a rule for instantaneously writing down such a matrix." About the same time, Sylvester was seeing Peirce's "On the Logic of Number" and his edition of *Linear Associative Algebra* through the press. They appeared in the fourth volume of Sylvester's *Journal* with the *Linear Associative Algebra* stretching over two issues. In the second addendum to *LAA*, "On the Relative Forms of the Algebras" (item 41), Peirce inserted a reference to his problematic brochure, which suggests that he may have completed this addendum between 7 January, when he finished the brochure, and the middle of February, by which time he had decided not to distribute it.

Peirce's summer was almost completely taken up with his scientific endeavors, especially his work with John Herschel and the construction of his new pendulums but also with his spectrum meter experiments and with his reports for the superintendent. He was occupied, as well, with the legal preparations for his divorce from Zina. He commuted frequently from Baltimore to Washington and New York, and took occasional side trips on Coast Survey business, as well as the trip to Montreal and Albany when he traveled with Juliette.

Charles Darwin's death in April had rekindled discussions of the question of evolution. On 27 April T. H. Huxley had written for *Nature:*

> He found a great truth, trodden under foot, reviled by bigots, and ridiculed by all the world; he lived long enough to see it, chiefly by his own efforts, irrefragably established in science, inseparably incorporated with the common thought of men, and only hated and feared by those who would revile, but dare not.[45]

When Peirce returned to Baltimore in September to begin his fall classes, he gave a public lecture (item 56) designed to convey "the purpose of the study of logic" and "remove some prejudices." He gave a general outline of his fall course (to meet four times a week) and made a strong pitch for liberal education:

> But when new paths have to be struck out, a spinal cord is not enough; a brain is needed, and that brain an organ of mind, and that mind perfected by a liberal education. And a liberal education—so far as its relation to the understanding goes—means *logic.* That is indispensible to it, and no other one thing is.

Reflecting on Darwin's achievements, he attributed them largely to his method:

> The scientific specialists—pendulum swingers and the like—are doing a great and useful work; each one very little, but altogether something vast. But the higher places in science in the coming years are for those who succeed in adapting the methods of one science to the investigation of another. That is what the greatest progress of the passing generation has consisted in. Darwin adapted to biology the methods of Malthus and the economists. . . .

[44]Robin MS 302.
[45]*Nature* 25 (1882): 597.

After several other examples of men who had adapted the methods of one science to the investigation of another, Peirce went on:

> in order to adapt to his own science the method of another with which he is less familiar, and to properly modify it so as to suit it to its new use, an acquaintance with the principles upon which it depends will be of the greatest benefit. For that sort of work a man needs to be more than a mere specialist; he needs such a general training of his mind, and such knowledge as shall show him how to make his powers most effective in a new direction. That knowledge is logic.

Peirce was beginning to see his task as that of applying the methods of logic, especially induction and hypothesis, to philosophy and science. Over the coming months he would reflect on the statistical method that had been so fruitful for Darwin and would make the bold surmise that chance is an active player in the evolution of the universe and its laws. The Epicurean seed would bear fruit.

Peirce's logic class for the fall of 1882 (with fourteen students) and the spring of 1883 (with seven) was impressive. Jastrow stayed for both terms, and it is probably this course he was thinking of when he said that Peirce had given him his first real experience of intellectual muscle. Peirce considered the foundations and philosophy of logic, using his "Illustrations" as his text, and then took up modern formal logic and the algebra of logic, using as texts De Morgan's *Syllabus of Logic* and Schröder's *Operationskreis des Logikkalkuls,* with examples from many other sources. He then took up (1) the logic of relatives, using as texts his "Logic of Relatives" (item 39 in *W*2), "Algebra of Logic" (item 19), "Algebra of Relatives" (item 43), and his paper on the logic of relatives that would become Note B in the *Studies in Logic* (item 66); (2) mathematical reasoning, where he examined the nature of mathematical demonstration and studied "the methods of mathematical research" using the history of multiple algebra as his example; (3) the theory of probabilities, with Liagre's *Calcul des Probabilités,* Boole's *Calculus of Finite Differences,* and Ferrero's *Metodo dei Minimi Quadrati* as texts; (4) inductive reasoning, to which he devoted a large part of the course and for which he used his "Theory of Probable Inference" (item 64); (5) the nature of scientific reasoning, with Kepler's *De motibus stellae Martis;* (6) an inquiry into the validity of modern conceptions of the constitution of matter, with Meyer's *Kinetische Theorie der Gase;* and (7) in conclusion, he considered the relation of the new theory of logic to philosophical questions. This course was Peirce's most ambitious bid for a permanent position as professor of logic.

Peirce continued his active participation in the Johns Hopkins clubs. He presided over the Metaphysical Club until November and gave a paper on Mill's logic and a response (item 47) to B. I. Gilman's "On Propositions and the Syllogism." In October he read "On a Class of Multiple Algebras" (item 57) to the Mathematical Seminary. He also presented two papers on logic to the National Academy of Sciences, one in April "On a fallacy of induction" (P 233) and another in November "On the logic of relatives" (P 235). The first may be the paper he had presented to the Johns Hopkins Scientific Association in November 1881, and the second is probably what became Note B in *Studies in Logic.*

By the end of 1882 Peirce was experimenting with graphical systems of logic. He may have been stimulated by Sylvester's 1878 paper "On an Application of the New Atomic Theory to the Graphical Representation of the Invariants and Covariants of Binary Quantics," perhaps in conjunction with his study of the atomic theory of matter for his logic class. In this paper, in what Peirce saw as an anticipation of his reduction thesis (see item 20), Sylvester had put forward "one simple, clear and unifying hypothesis, which will in no wise interfere with any actually existing chemical constructions. It is this: leaving undisturbed the univalent atoms, let every other n-valent atom be regarded as constituted of an n-ad of *trivalent* atomicules arranged along the apices of a polygon of n sides." After explaining his theory further, and giving numerous diagrammatic examples, Sylvester remarked: "The beautiful theory of atomicity has its home in the attractive but somewhat misty border land lying between fancy and reality and cannot, I think, suffer from any not absolutely irrational guess which may assist the chemical enquirer to rise to a higher level of contemplation of the possibilities of his subject." Peirce's paper on junctures and fractures (item 59) and his 21 December letter to O. H. Mitchell (item 60) suggest that he may have been trying to apply some of the methods of chemistry, and perhaps the theory of atomicity, to logic.

Also in 1882, though perhaps already in the latter part of 1881, Peirce met Benjamin Eli Smith, who had come to Johns Hopkins as a graduate assistant. Although he seems not to have been a student in any of Peirce's courses, he presented two papers to the Metaphysical Club, one on "Wundt's Theory of Volition" in February 1882 and the other "On Brown's 'Metaphysics'" the following month. Smith was a member of the staff of the *Century Dictionary* (and soon became its managing editor) and he recruited Peirce to be a contributor. Peirce was given principal responsibility for terms in logic and philosophy, mathematics, mechanics and astronomy, weights and measures, and all words relating to universities. By 1883 Peirce had already begun working on definitions (see MSS 496 and 497) and in the fall of that year, with the dictionary project in mind, he added a new course on philosophical terminology. From this time onward—for after the first edition of 1889–91 he immediately set to work on a revised edition—Peirce had definitions, etymologies, and language groups (and other lexicographical matters) on his mind. This was a monumental project and Peirce's contribution was massive. Its impact on the evolution of his thought was surely very significant, though it has yet to be seriously examined. Peirce's difficulties with Sylvester had not ended with his decision to withhold his 1882 brochure. In the early weeks of 1883 a more severe and consequential dispute broke out. In August 1882 one of Sylvester's papers (an abstract of a paper on nonions which he had read in May to the Mathematical Society) appeared in the *Circulars* with the following sentence: "These forms can be derived from an algebra given by Mr. Charles S. Peirce (*Logic of Relatives,* 1870)." The sentence, as it turned out, had been written by Peirce. Apparently, Sylvester had entrusted Peirce with checking the proof-sheet of his paper for adequate reference to his own work. Peirce had expected that Sylvester would look over his changes before releasing the proof-sheet to the printer but, according to Sylvester, that did not happen. In reflecting on the episode in later years,[46] Peirce remembered that he had not made the insertion mark for the printer, but had only written out the sentence he thought Sylvester would want to insert. The February 1883 *Circular* carried an *Erratum* by Sylvester correcting the troublesome sentence to read "Mr. C. S. Peirce informs me that these forms can be derived from his Logic of Relatives, 1870." He went on to say:

I know nothing whatever of the fact of my own personal knowledge. I have not read the paper referred to, and am not acquainted with its contents. The mistake originated in my having left instructions for Mr. Peirce to be invited to supply in my final copy for the press, such references as he might think called for.

Peirce was incensed. Not only had he engaged in lengthy discussions with Sylvester about his logic of relatives and carried on at least a limited correspondence with him, but in April 1882 Sylvester had discussed Peirce's logic of relatives before the Mathematical Seminary and in the same month had stated specifically before the Scientific Association that Peirce's logic was tantamount to his Nonions. His remarks had been reported in the *Circulars* as follows:

Mr. Sylvester mentioned . . . that in his recent researches in Multiple Algebra he had come upon a system of Nonions, the exact analogues of the Hamiltonian Quaternions. . . . Mr. Charles S. Peirce, it should be stated, had to the certain knowledge of Mr. Sylvester arrived at the same result many years ago in connexion with his theory of the *logic of relatives.*[47]

Only a year earlier, in Sylvester's own journal, Peirce had published the addendum to his father's *Linear Associative Algebra* (item 41) in which he proved "that any associative algebra can be put into relative form, *i.e.* . . . that every such algebra may be represented by a matrix."

Peirce wrote out a full reply to the Erratum and sent it to Gilman. There followed much correspondence between Gilman, Peirce, and Sylvester and there were drafts of responses and responses to responses. At one point, on 29 March, Peirce wrote to Gilman:

I cannot consent to my statement being modified unless Professor Sylvester will say that my conduct was correct in regard to the proof-sheets. I have no objection to this being qualified by his saying that it was correct *if the oral message was delivered to me as I say it was;* but clearly if such a qualification is to be inserted, everything depends upon how it is put.

Peirce continued with detailed recommendations for emendation. At some point Gilman sent drafts of Peirce's reply and Sylvester's note to Peirce's reply to trustee G. W. Brown. Brown responded on 17 April: "After thinking over this annoying matter it appears to me that nothing is to be done but to publish the articles as they stand. This should however be the last of it and

46 Robin MS 431.

47 *Johns Hopkins University Circulars* 1 (1882): 203. Reprinted in James J. Sylvester, *Mathematical Papers* (Cambridge: University Press, 1909), 3:643.

would it not be well to say so to both in advance." Earlier there had been a suggestion, apparently from Brown, to publish Peirce's reply without Sylvester's note. But Sylvester had responded heatedly to Gilman:

I am astonished at the proposition contained in your note of the 18th that it should be proposed to allow Mr Peirce's virulent and disingenuous statements to be made in the circular without giving me an opportunity of replying thereto. If that course is adopted, self-respect will render it imperative for me to withdraw from all future participation in the circulars.

Peirce's "Communication" (item 67) finally appeared in the April *Circular*, preceded by this "Note" from Sylvester:

I wished (as I still wish) it to be understood that it is Mr. Peirce's statement and not mine that the "forms" in question can be derived from his Logic of Relatives. I certainly know what he has told me and should attach implicit credit to any statement emanating from him, but have not the knowledge which would come from having myself found in his Logic of Relatives the forms referred to; as previously stated I have not read his Logic of Relatives and am not acquainted with its contents.

Many years later, when Peirce recounted these events, he wrote of Sylvester's character:

Sylvester was a man whose imagination and enthusiasm were incessantly running away with him: he was given to harboring the most ridiculous suspicions and to making rasher assertions than became so great a man. His power of distinct recollection was most phenomenally weak, almost incredibly so; while his subconscious memory was not at all wanting in retentiveness. . . . I suppose, as he said, that he "came across" the system of novenions . . . and remembered, or thought he remembered, that I had pointed out these forms. Subsequently, he got a suspicion that I was about to charge him with plagiarizing my *"Description of a Notation* &c," and was anxious to declare that he had never read it, and knew nothing about it. He seems to have fancied that I had some deep-laid plot against him.[48]

Peirce must have felt some relief from the tension of his conflict with Sylvester toward the end of March 1883 when his long-awaited *Studies in Logic,* which had been "in the works" for over two years, finally appeared. Peirce had written to Gilman about it as early as 9 February 1881, and on 8 December 1881 he had said to Christine Ladd that "after a long delay from various causes, I have everything arranged to go on with the publication of our essays except one thing—about $300 is still needed. I shall probably supply this myself, but am not prepared to do so now, so that the matter may rest idle till spring." Although the matter lay idle much longer, when it finally did come out *Studies in Logic* was immediately recognized as an important contribution. The book as a whole covered a vast part of the field of symbolic logic and dealt with the work of the major contributors. Even Frege's *Begriffsschrift* appeared in Ladd's bibliography although it is not mentioned in the paper. In his review for *Mind,* Venn said that the most interesting paper philosophically was the concluding one by Peirce which dealt with the nature and foundations of statistical reasoning and the connection between probability and induction. This was "A Theory of Probable

[48]Robin MS 431.

Inference" (item 64) about which Peirce wrote to Paul Carus: "In my humble opinion you are never likely to say again anything so false as that writings lose their freshness by being worked over. The first page or two of my Theory of Probable Inference was put into more than 90 forms very varied before I was satisfied; yet nobody would suspect any elaborate work on it."[49]

Peirce was pleased with *Studies in Logic.* He sent out many inscribed copies as gifts and for the remainder of his days he often referred to one or another of its papers as an authoritative source. To T. S. Perry he wrote: "If you are going to read any of my papers—which seems inconceivable—I hope you will try note B in the bound book."[50] In 1904 Peirce remarked about Note B to Victoria Lady Welby: "My friend Schröder fell in love with my algebra of dyadic relations. The few pages I gave to it in my Note B in the 'Studies in Logic by Members of the Johns Hopkins University' were proportionate to its importance."[51] It seems doubtful that Peirce was fully of this opinion in 1883 but he embraced it more and more fully as time went by.

Studies in Logic is a landmark not only for logic, but also for education in America. It was a work on the leading edge of research in its field by a team of researchers composed mainly of graduate students. Certainly they were led by a seasoned scholar but he neither demanded nor wanted credit for their work. Even though Peirce edited the book his name did not appear on the title page; it was by MEMBERS of the the Johns Hopkins University. This was in the spirit of Johns Hopkins in its first decade.

Peirce had a great deal on his mind in the winter and spring of 1883. There was his demanding logic course, the trouble with Sylvester, and as always during these years, he had several Coast Survey projects going at once: pendulum operations at the Smithsonian and the Stevens Institute, testing of the new Peirce pendulums, preparations for an eclipse expedition, continuation of spectrum meter experiments and other metrological work, plans (which would fall through) to go to Point Barrow in the Arctic, and the constant pressure to finish overdue reports. He participated in the first three meetings of the Metaphysical Club and remarked on papers by G. S. Morris, A. H. Tolman, and W. T. Sedgwick. In May he wrote to Hilgard to say that he had "written for *Science* a careful review of Dr. Craig's work on projections—a job upon which I have spent a great deal of time"; but apparently it was never published and only one manuscript page (MS 442) has been found. Looming over all this, casting its shadow, was Peirce's coming divorce and remarriage.

Peirce's divorce from Zina became final on 24 April. He married Juliette on 30 April and by 2 May they were sailing to Europe. Peirce had made plans to visit with Hugh McColl in Boulogne, which he probably did near the beginning of his stay. On 16 May he sent Gilman a general plan for his 1883–84 course of lectures based, as he said, on his "forthcoming

[49]Peirce to Carus, 3 March 1893 (L 77).

[50]Peirce to Perry, 24 March 1883 (L 344).

[51]*Semiotics and Significs*, ed. Charles S. Hardwick (Bloomington and London: Indiana University Press, 1977), p. 29.

book." From the plan, Gilman had the following notice printed in the June *Circular:*

Mr. C. S. Peirce. 1. Will give forty lectures to graduate and special students upon General Logic. The course will follow the contents of Mr. Peirce's forthcoming treatise on logic. At least one lecture will be devoted to each chapter, but the preferences of the class will be consulted in deciding upon the topics of nine of the lectures. The distribution of topics in the chapters is as follows:

Generalities (5 chapters)
Deductive Logic:
  Non-mathematical (3 chapters)
  Algebraic (4 chapters)
  Otherwise mathematical (4 chapters)
Inductive Logic:
  Theory (9 chapters)
  Illustrations (6 chapters)

2. Will give special courses or private lessons upon any branch of the subject in which any of the graduates or special students may desire instruction.

As the summer progressed Peirce expanded his plan into a full-fledged syllabus and, as it grew, so did the planned number of lectures. There are two manuscript versions of the syllabus, one with fifty lectures (MS 458) and a more finished one with sixty lectures (item 69). A few features of the syllabus stand out. Peirce has definitely introduced truth values into his system of logic by this time and he is using quantifiers as he will in his 1885 "Algebra of Logic." Most of the topics he had written about at Johns Hopkins are covered in one way or another. Possibly the best general outline we have of his logic of relatives is given in lectures X through XIV. Some lectures treat topics he had not yet written about but soon would. For example, part of lecture XIX is devoted to the nature of geometrical axioms and the last part of lectures XXXIII–XXXVI is devoted to the problem of the duration of play, applied to the theory of natural selection and to philosophy. Peirce's thoughts were turning toward those he would express a few months later in "Design and Chance" (item 79). There is even a provocative reference in lecture XXV to the harmfulness of logic too narrowly studied. Overall, the syllabus provides a detailed account of Peirce's well thought out design for an advanced general course in logic.

There are four lectures or fragments of lectures that Peirce probably composed before classes began in the fall: items 70–73. In them he continues the discussion of the constitution of the universe begun in item 19 and in his class lectures (which Christine Ladd had developed in her *Studies In Logic* paper). Peirce's theory of quantification is also much in evidence. At least the first three lectures were probably written while Peirce was still in Europe, though it is possible that all of them were written out class by class.

When Peirce and Juliette returned from Europe in mid-September 1883, they took a two-year lease on a house in Baltimore and began to furnish it. Peirce had sought and had been given Gilman's assurance that his position with the philosophy department was secure, so he and Juliette were eager to make Baltimore their home. When Peirce began teaching in the fall he may well have supposed that it was just the next of many teaching years ahead of him. It turned out that enrollment in his courses dropped dramatically from the previous year. Only four students took the advanced logic class in the fall—John Dewey, Jastrow, C. W. E. Miller, and Henry Taber—and only Jastrow and Taber were left for the second term. Dewey had dropped out because the course was too mathematical. But he and Jastrow enrolled in Peirce's new course on philosophical terminology. The course met once a week and lasted for only a few weeks. Beginning in early October, Peirce sought special privileges with the university library. On 10 October he requested permission, for special reasons, to take out twelve books at a time. The special reason was that he was engaged in a "piece of work" that "requires me to make use of a great many books." He explained that his research required the regular consultation—in some cases many times a day—of certain books. "Such for instance is the Berlin Aristotle in 5 volumes." Probably the "piece of work" was his set of definitions for the *Century Dictionary,* but soon he was also stymied in his related course on philosophical terminology. Although he tried as best he could to work out a suitable arrangement with the library, he met with no success. Finally on 8 November he wrote to Gilman: "I find my work brought to a complete stand-still for the want of books. I have been obliged to suspend my lectures on Philosophical Terminology until I can obtain the Berlin Aristotle. My application to you to have the University add another Aristotle to the library I understand to be refused." Peirce went on to ask if he could buy back his books listed under Ancient Authors which he had sold three years earlier. Gilman must have taken offense for a week later Peirce wrote to him again: "I deeply regret having said anything which seems to offend you, since I am bound to you by every bond of official respect, personal esteem, gratitude, and if you will permit me to say so even affection." But Peirce continued in a less conciliatory way:

> Then, let me say with candour, my dear Mr. President, that although I believe I have never complained of it to anybody, I have not thought that any heed at all had been given to any of the suggestions which I have made in regard to wants in the Library, although I considered them important. . . . I think, without of course comparing you to the jailer of the Peabody Library, that Cambridge is a trifle ahead of Baltimore in its appreciation of the wants of its students in the way of books. You have always permitted me to express myself with great freedom to you, and I always think a misunderstanding should be seized as an occasion to have a mutual understanding. There*[*fore*]*, I beg you will not find offence in what I am saying. I have lately been offending people everywhere by my speeches.

Peirce then withdrew his request, admitting that it had not been "in good taste or temper." Although it is not clear how the whole matter was finally settled, it does appear that the course on philosophical terminology had come to an end.

Peirce taught a third course in the fall of 1883, described very briefly in the *Circulars:* "He also guided a company of students in studying the psychology of great men."[52] He had invited a group of students to join him in this study, and they worked out an elaborate program that involved reading the chief biographies of the day, extracting data of specified sorts, compiling impressionistic lists of great men and finally, submitting the lot to statistical analysis. Peirce wanted to demonstrate that statistical analysis could be fruitfully applied even in situations where the primary data are impressionistic (based on impressions). This study may have been the first extended application of statistical methods to comparative biography. Although Peirce continued the study with his group of students through the summer and fall of 1884, and even into the winter, it was never completed. Sometime after his move to Milford in 1888 Peirce took up the study again, probably stimulated by the publication of *The Comtist Calendar.* His 1901 paper on "The Century's Great Men of Science" was an offspring of the earlier study, and shortly after Peirce's death one of the members of Peirce's group, Joseph Jastrow, remarked in a memorial article[53] that he had been permitted to publish two rather simple conclusions, one relating to "Longevity," and the other to "Precocity."[54] Although many of the manuscripts related to the study of great men were composed in the period of the present volume, the study as a whole went beyond the period and will therefore be included in the next volume.

Peirce attended all the meetings of the Metaphysical Club for the fall term and gave one paper, a reply to G. S. Morris's "The Philosophical Conception of Life." He heard Jastrow read a paper on "Galton's 'Inquiry into Human Faculty'," Dewey on "The Psychology of Consciousness," and A. T. Bruce on "The Design Argument." Bruce's paper was read on 11 December and the Club's minute book shows that Peirce remarked on it. Just over one month later Peirce would read his "Design and Chance" to the Club.

Looking through the correspondence of this period for clues to Peirce's life and work, one letter stands out as signalling the end of his fortunes at the Johns Hopkins. On 22 December 1883 Simon Newcomb wrote to Gilman: "I felt and probably expressed some uneasiness in the course of our conversation the other evening, lest I might have been the occasion of doing injustice to persons whose only wrong had been lack of prudence. I have therefore taken occasion to inquire diligently of my informant, and am by him assured that everything I had said was fully justified." Newcomb was referring to Peirce as the one he might have injured and his informant was Julius Hilgard. Although it is not known for sure what "wrong" Peirce had committed beyond a "lack of prudence," we do know that Newcomb's revelations led to a resolution of the Johns Hopkins Executive Committee that effectively ended Peirce's connection with the university. The resolution

[52] *Johns Hopkins University Circulars* 3 (1884): 119.

[53] "Charles S. Peirce as a Teacher," 725.

[54] "The Longevity of Great Men," *Science* 8 (1886): 294–96 (also in *Nature* 4 Nov. 1886); "Genius and Precocity," *Christian Union* 37 (1888): 264–66; a related paper with the same title appeared in the *Journal of Education* (July 1888): 326–28.

passed on 26 January 1884, was not to renew the contracts of lecturers in philosophy and logic "after the present engagements expire" and to replace the three lecturers (Peirce, Morris, and Hall) with one professor and an assistant. But only Peirce's appointment expired at the end of the 1883–84 academic year, and he soon realized that the resolution was aimed at him. At first he appealed to the sense of fairness of the university administration. On 8 February he wrote to Gilman and asked that his letter be laid before the Executive Committee:

> On returning to Baltimore last September, I was unable to obtain a suitable house for one year. Therefore, as soon as the President returned I went to him and explained my difficulty and asked whether in his judgment it would be prudent for me to take a house for two years. To this important inquiry he replied that he knew of no disposition to disturb me in my place. The Treasurer suggested my purchasing a house. In view of these encouragements, I did take a house for two years. I have never heard the smallest whisper of dissatisfaction or suggestion of a possible change until I yesterday received your resolutions. My lectures have been much better than hitherto. There has been more coöperation between the different branches of philosophical instruction. There has, in short, been no reason for a change which did not exist before. I, therefore, appeal to your sense of fairness, gentlemen, with great confidence; for to cut short my lectureship at the end of this year, though it be perfectly within the letter of the contract, is not one of the things which it is open to you under the circumstances to do. I have no doubt that President Gilman spoke truly and sincerely in encouraging me to take my house. He now tells me he has for a long time seen this crisis coming; this long time must however have been altogether subsequent to last October.

The final paragraph of Peirce's letter suggests that he thought religion was somehow at issue:

> I also desire to address you briefly upon the present state of philosophy, and to show you that the difficulty of finding a *modus vivendi* between philosophy, science, and religion, is now much less than it has been for a very long period; so that you have only to make the philosophical department really true to the actual condition of thought, and you will bring it into a state of warm sympathy and friendship with science on the one hand and with Christianity on the other.

Peirce was probably right on two counts. The immediate and official cause of the decision to let him go *was* something subsequent to October 1883, and probably something else, like his attitude about religion, had helped bring on the crisis. When Gilman had candidly told Peirce that he had "for a long time seen this crisis coming" he may have revealed a truth he would decline to make official. Certainly Peirce had given Gilman many reasons to be concerned about his long-term continuation at Johns Hopkins beginning with his December 1879 letter about the alarming state of his brain. Peirce's work for the Survey had resulted in a number of conflicts and absences and, perhaps partly because of the pressures of two jobs, his ill health had been a source of inconvenience. His aborted resignation in 1880 and the events following the sale of his books, and the cancellation of his course in philosophical terminology, had been irritating and perhaps even embarrassing for Gilman, who surely also noticed that Peirce's courses often had low enrollments. And then there was the question of religion. Charles W. Nicholls, who was in Peirce's first course in logic and who presented the first paper to the Metaphysical Club, recorded some telling remarks in his "Johns Hopkins University Note Book":

> I read by invitation from the university, before the Johns Hopkins Philosophical Association, a thesis on "Illustrations from Grecian philosophy of the fallacy that differences in nature must correspond to received verbal and grammatical distinctions." Professor Charles S. Peirce, the scientist who presided, was an agnostic, and heartily seconded the sophomoric flaying I administered to old father Aristotle and the Schoolmen.[55]

If Nicholls's perception of Peirce was common among his students, it probably would have come to Gilman's attention and would have disturbed him. He had worked hard to alleviate the fears of conservative Baltimorians who imagined that the university was encouraging agnosticism.

But the official cause of the decision to let Peirce go, and clearly the provocation, was Newcomb's revelation. This is evident in the record of the Johns Hopkins Executive Committee. On 1 December 1884, Committee Chairman William Brown made the following statement:

> The undersigned having read Mr. Peirce's recent letters to Judge Brown, & having refreshed their recollection by reference to the records of the Executive Committee, & the official correspondence, make the following statement so that if there should be any subsequent reference to this affair, their understanding of *[it]* may be on record.
>
> The change of attitude toward Mr. Peirce on the part of President Gilman, which is the cause of complaint, occurred near the beginning of January 1884 in consequence of information first brought to his knowledge in December 1883, several weeks subsequent to his remark that he "knew of no disposition to disturb Mr. Peirce in his relations to the university"; and from that time onward Mr. Gilman's communications to Mr. Peirce were governed by the action of the Executive Committee and were taken in consultation with two members of that body.

Newcomb's revelation never became part of the official record. The most explicit reference appeared in a 15 November 1884 letter from Gilman to the Executive Committee in which he summarized the events surrounding Peirce's dismissal:

> It is true that *[at]* the beginning of the academic year 1883–4, I knew of no disposition to disturb Mr. Peirce in his relations to this university. It was not until several weeks later that one of the Trustees made known to the Executive Committee & to me certain facts which had been brought to his knowledge quite derogatory to the standing of Mr. Peirce as a member of an academic staff. These facts & their bearing upon the philosophical instruction in this university were considered by the Executive Committee, at their meeting, January 26, 1884.

Further light is shed on the matter by Newcomb himself in a letter of 30 December 1883 to his wife:

> I have been somewhat exercised at being the unintended means of making known some of the points of C. Peirce's marital history at Baltimore. When last going to N.

[55]Charles Wilbur de Lyon Nicholls, "Annals of a Remarkable Salon," unpublished brochure, deposited in the Johns Hopkins University Library.

Y. I went from Balt. to Phil. in the same seat with Dr. Thomas, a J. H. U. Trustee, and supposing they all knew more or less of the affair got talking of it, and let several cats out of the bag. What I gave as reports, Dr. Th., I suspect, told Gilman as facts, and troubled the latter greatly, as it seems Mrs P (2) had begun to cultivate Mrs G's acquaintance. The supposition is, that the marriage last summer made no change in the relations of the parties. Mr. Hilgard assures me that it is all true, they having occupied the same apartments in N. Y. some years ago. It is sad to think of the weaknesses which may accompany genius.[56]

An examination of the exchange of letters between Peirce and Gilman and other members of the Executive Committee, which began with Peirce's notification of the 26 January resolution and continued at least into December, reveals that Peirce's initial concern was to keep his position and to defend his honor as an instructor. But as he became aware of the unyielding resolve of Gilman and the Committee, his concern shifted to an interest in reimbursement for damages resulting from his dismissal. If anything beneficial came of that lengthy exchange of letters, it was at most some measure of compensation for his loss in setting up a home in Baltimore. But the loss of an academic career, both to Peirce and to the world, could not be compensated.

During that painful year Peirce must have suspected that his academic life was over. Although he made some attempts to find another teaching position, it was less than four years after his dismissal that he and Juliette moved to Milford, Pennsylvania, to live the rest of their lives in seclusion and relative obscurity. Peirce was never again offered a regular teaching position, and his dismissal from the Johns Hopkins was at least partly the reason. In dropping Peirce from consideration for a position in philosophy at the University of Chicago in 1892, William R. Harper relied on the advice of George H. Palmer of Harvard University, who had written on 4 June 1892:

I am astonished at James's recommendation of Peirce. Of course my impressions may be erroneous, and I have no personal acquaintance with Peirce. I know, too, very well his eminence as a logician. But from so many sources I have heard of his broken and dissolute character that I should advise you to make most careful inquiries before engaging him. I am sure it is suspicions of this sort which have prevented his appointment here, and I suppose the same causes procured his dismissal from the Johns Hopkins.[57]

It is remarkable that James, certainly a man of judgment and discrimination, never gave up on Peirce but continued to recommend him as both teacher and scholar. Regrettably, others were blind to what James saw in Peirce.

Peirce's appointment at Johns Hopkins ran until 1 September 1884, so he labored under the cloud, even disgrace, of his dismissal for about seven months. Yet he persevered with his classes and managed to keep up a steady

[56]Newcomb's wife, Mary Hassler Newcomb, was the granddaughter of Ferdinand Hassler, first superintendent of the Coast Survey. She appears to have taken a special interest in finding out the worst about Peirce. See Josiah L. Auspitz, *Commentary* 52 (1983): 51–64.

[57]Darnell Rucker, *The Chicago Pragmatists* (Minneapolis: University of Minnesota Press, 1969), p. 10.

flow of manuscript pages. In addition to his advanced logic course that continued in the second term with only two students (Jastrow and Taber), he taught a course on probabilities that met twice a week with an enrollment of seven (Davis, Julius J. Faerber, Arthur S. Hathaway, Jastrow, Henry B. Nixon, William E. Story, and Taber). He also gave several papers during the year including one to the Mathematical Seminary and three to the Metaphysical Club. On 16 January he delivered "On the Mode of Representing Negative Quantity in the Logic of Relatives" to the Mathematical Seminary. However, he could not have hoped to enlighten Sylvester about the generality and power of his logic, for Sylvester had departed for England the previous month to take up his chair at Oxford. He did not find out about Peirce's dismissal right away and several years later (on 28 March 1888) he wrote to Gilman: "What was the cause of C. Peirce's leaving? I am truly sorry on his account. I regret the differences which sprang up between him and me for which I was primarily to blame. I fear that he may not have acted with entire prudence in some personal matters."

On 17 January, the night after his talk at the Mathematical Seminary, Peirce gave what may be his most important philosophical paper of the Johns Hopkins period. On that night he presented "Design and Chance" (item 79) to nine members of the Metaphysical Club. The following remarks appear in the Club's minute book: "President Morris in the Chair . . . Principal paper was read by Mr. Peirce. Subject: Chance and Design. Mr. Peirce, Dr. Franklin, Prof. Remsen, Mr. Dewey and Mr. Jastrow as well as the President took part in the discussion." The paper is not such a substantial work in itself, but it represents an important turning point in the evolution of his thought. It is curious that it was written at such a turning point in his life. We shall quickly survey the rest of Peirce's non-scientific papers for the remainder of the year and then return to "Design and Chance."

Peirce delivered his second Metaphysical Club paper of 1884 on 13 May. It was entitled "Logic of Religion." On 7 April he had written to Gilman to seek permission to give six lectures on the logic of religion in the fall "with the purpose of stating some things on the credibility of various religious beliefs." Although it is difficult to make out the text of this letter, Peirce seems to be saying that if the trustees would not sanction his lectures he would give them at his house. No such lectures seem to have been given, though his 13 May paper was probably a preview of what he had in mind. The Club's minute book only reports that "it had special reference to the proofs of the existence of a God," and the June 1884 *Circular* that it was "on the logic of religious life."

One of the manuscripts from this period (MS 505) is evidently an outline for an oral presentation and it *may* be the outline for the Metaphysical Club paper. There are no references, however, to existence proofs. It has "reading times" marked at the left margin which indicate that the first three pages took twenty-one minutes. It begins:

> Religion must be subject to good sense. It is always in danger of being carried to excess. . . . Morality cannot be carried to excess. Logic cannot be carried to excess; and

it is not subject to good sense, but on the contrary gives good sense its law. But religion if not taken in moderation leads to insanity and that *not* as is sometimes said, because it is adulterated, but because of the element of it that is most essential,—the mystical element.

A fourth page, perhaps an outline for a separate talk, begins by asserting that "Scientists have faith in science" and "religionists want faith in religion." Peirce then mentions the prayer test which he says, is "also a test of faith." He goes on to say that if religionists really had faith they would not be afraid of science but would encourage it, "sure that it would ultimately be found on their side." Peirce conluded his outline with the following:

Reality. True nature given by me. Opposed to conception which makes it origin of force. True philosophy adequate to govern the science of the XIX Century, develops itself from my conception alone.

Passage of Cayley's address.[58] Appears at first sight an anachronism. A man like Cayley had better not be rashly accused of anachronism. Really what distinguishes this XIX above all is the force of $\sqrt{-}$.

How this came about.

To the businessman—gold alone is real. To the physicist force alone. To the mathematician relations alone; $\sqrt{-}$ more real than gold.

Peirce presented his final paper to the Metaphysical Club on 18 November, two and a half months after his appointment had ended. He discussed Petrus Peregrinus's *De magnete* which he had transcribed from a manuscript in the Bibliothèque Nationale the previous summer and which, because he thought it held a significant place in the history of scientific method,[59] he hoped to publish with an English translation. At the club's fortieth meeting president Hall recommended that it be reconstituted to reflect the reorganization of the Johns Hopkins philosophy program. According to the minute book there were only three more meetings, the last on 3 March 1885. Even in Peirce's absence, for he did not attend again and had left Baltimore by the new year, his influence continued. On 27 January Jastrow gave a demonstration of logic machines including the Stanhope Demonstrator, Marquand's machine for syllogistic variations, and two machines of his own. At the previous meeting, on 16 December, A. T. Bruce had read a paper on "Final Causes" arguing that "natural selection was a process, not consistent with the notion of a designer but more akin to the action of chance." This suggests the influence of Peirce's "Design and Chance." At the club's last meeting, M. I. Swift also spoke on "Final Causes."

In the spring of 1884 Hall, now Professor of Psychology and Pedagogy, had organized a program of lectures for about eighty graduate students planning to become teachers, with lectures by President Gilman, Gildersleeve, Remsen, Martin, Hall, Adams, Wood, and Peirce. In Hall's original

[58]Peirce may have had the following passage in mind: "I would myself say that the purely imaginary objects are the only realities, the ὄντως ὄντα, in regard to which the corresponding physical objects are as the shadows in the cave." The quotation is from the "Inaugural Address by Arthur Cayley," *Nature* 28 (1883): 492.

[59]*Historical Perspectives on Peirce's Logic of Science*, ed. Carolyn Eisele (Berlin: New York, Amsterdam: Mouton, 1985), 1:4, 15–95.

plan, Peirce was slated to give two lectures, one on "The Observational Element in Mathematics" and another on "The *a priori* Element in Physics." Although no manuscripts with these titles remain, it is likely that item 80 is the first part of a lecture for Hall's special course.

According to a notice in the May issue of *Science* Peirce read two papers at the spring meeting of the National Academy of Sciences, one on the study of comparative biography and the other (with Jastrow) on whether there is a minimum perceptible difference of sensation.[60] But it is doubtful that Peirce attended that meeting, and the paper with Jastrow was first read at the October meeting of the Academy and published in its *Memoirs* (P 303). He read two other papers at the Academy meetings in Newport: "On Gravitation Survey," and "On the Algebra of Logic," the latter probably from what would soon appear in the *American Journal of Mathematics* (P 296).

Despite what must have been a great tragedy for Peirce—the loss of his academic position (and $2500 salary), the disappointment of having to prepare to leave the home he and his new bride had only a few months before begun to furnish, and the growing awareness that he and Juliette were now *personae non gratae,* especially in the home of President Gilman—he remained productive as a scholar (and as a scientist). But such a stunning blow inevitably affected the course of his work. At first Peirce shifted much of his attention to science and his work for the Coast Survey. In October he had taken charge of the Office of Weights and Measures and had sought to convince Congress of the need for an efficient bureau of standards. 1885 was largely devoted to pendulum swinging at Key West, Ann Arbor, Madison, and Cornell. But in July the Coast Survey was rocked by scandal; Hilgard was fired and the value of Peirce's work was impugned. Although Peirce's reputation was soon restored, the allegation that his work was of "meagre value" had greatly wounded him. When the Survey was placed in the hands of the chief clerk of the Internal Revenue Bureau, F. M. Thorn, who was a lawyer and not a man of science, Peirce knew that his days there were numbered. The enthusiasm that had been rekindled after his dismissal from Johns Hopkins began to wane.

Another change in Peirce can be traced to his separation from teaching. He did not immediately give up the idea of teaching—in June 1885 he proposed a course of twelve lectures on advanced logic at Harvard[61] and developed a correspondence course that he offered for a while—but the intense and fruitful interaction he had enjoyed with his logic students at Johns Hopkins was over. Peirce now had time for the more solitary speculations that would lead to his grand architectonic schemes of the late '80s and '90s ("Guess at the Riddle" and the first *Monist* series). The most obvious beginning of this new philosophy was his Metaphysical Club lecture on "Design and Chance."

The lecture draws together a number of ideas that had become prominent in Peirce's writings and lectures. He had long been interested in the

[60]See also *Nature* 30 (1884): 40.

[61]Peirce to James, 20 June 1885 (Wm. James Papers, Harvard University).

Darwinian controversy which had swept America after the first copies of *Origin of the Species* arrived in the fall of 1859, and as early as the following summer he was convinced that Darwin's theory, "which was nourished by positive observation," was destined to play a major rôle in the development of thought for years to come.[62] Philip Wiener has suggested that Peirce saw in evolutionism, when welded to his "rigorous scientific logic," a way to "make room for freedom of the individual will and religious values,"[63] and Max Fisch has suggested that "Peirce had an ulterior interest in the logic of evolution as a weapon in his lifelong war against nominalism."[64] But Peirce was also driven by the desire of the scientific philosopher to find things out and to bring whatever he could within the scope of explanatory hypotheses, and he was committed to the economy of explanation—he was a wielder of Ockham's razor—and always sought theories that represented the universe as parsimoniously as its richness would allow. In evolutionism he saw the prospect for a theory he could generalize and develop into a cosmological principle of the highest order.

Perhaps the key Darwinian idea that so attracted Peirce was that of *the long run:* "Darwin, while unable to say what the operation of variation and natural selection in any individual case will be, demonstrates that in the long run they will adapt animals to their circumstances" (*W*3:244). Peirce had made a special study of induction and probability and was well acquainted with sampling techniques and the tendency of random events, when sufficiently multiplied in controlled experiments, to assume as a group a determinate character. His understanding of statistics led him to his view of induction as a self-corrective process. If we add to these ideas his conception of habit as a tendency to act in ways that have not met with (or that have met with the least) resistance (the irritation of doubt is a kind of resistance), so that a habit is a statistical result of sorts, then we have most of the ingredients for the bold thesis of "Design and Chance." Perhaps it was the suggestive Epicurean vision of the uncaused swerve of atoms that drew together these conceptions in such an original way. What we find in this paper for the first time is Peirce's hypothesis that chance is really operative in the universe, even in the realm of laws.[65]

His main line of argument is that the fundamental postulate of logic, that everything is explicable, cannot be absolutely true—or at least, that there are good reasons for doubting its absoluteness. One of these reasons is that the operation of absolute chance, which is allowed for if the absoluteness of the postulate is rejected, provides the basis for a theory of cosmic evolution that promises both "the possibility of an indefinite approximation to a complete explanation of nature" and general guidelines for further scientific research.

[62]Fisch (1986), p. 23.

[63]Philip P. Wiener, "Peirce's Evolutionary Interpretations of the History of Science," in *Studies in the Philosophy of Charles Sanders Peirce,* ed. Wiener and Young (Cambridge: Harvard University Press, 1952), p. 143.

[64]Fisch (1986), p. 29.

[65]The next four paragraphs are a slight recasting of a summary statement about "Design and Chance" prepared by H. William Davenport.

The hypotheses of absolute chance and universal evolution provide the means, perhaps the only means, of satisfying the non-absolute version of the postulate, which asserts that "everything is explicable . . . in a general way." So even though Peirce is challenging the *absolute* truth of the claim that everything is explicable, his motive is to explain, or to make possible the explanation of, facts which had hitherto remained inexplicable—the laws of nature, similarities among those laws, the general fact that there are laws, and so on. Thus by introducing the hypotheses of absolute chance, habit-taking, and universal evolution, Peirce sought to extend rather than reduce the range of explicability.

The introduction of absolute chance provides for the possibility of an indefinitely close approximation to a complete explanation of nature by allowing for the origin and growth of a tendency to habit-taking. On this view the laws of nature become both "statistical results" and "habits gradually acqruied by systems." A kind of natural selection can take place among various systems, according to whether they develop "good" habits, "bad" habits, or no habits. Selection, in the form of elimination, takes place when a system disintegrates and also when entities move beyond the limits of the perceptible universe.

Peirce has generalized Darwinism, since what Darwin had done was to apply the "statistical method" (or probability theory) to the explanation of species, which had commonly been considered absolute and immutable. Peirce applies the same statistical method to the explanation of *all regularities,* including laws of nature, which still were generally assumed to be absolute and immutable. Add to this the sort of natural selection among habit-systems mentioned above, and the analogy between Darwinism and Peirce's evolutionism is very strong.

Despite its relative brevity and its incompleteness in the extant manuscript, the argument of "Design and Chance" is sufficiently strong and suggestive to stand as a major statement of Peirce's evolutionary explanation of the laws of nature—one worthy of close study and comparison with his later and more detailed presentations of the hypothesis. The paper records his rejection of his earlier necessitarianism in favor of tychism, and sets forth significant new developments in his views on the logic of explanation and the problem of induction. It is an important early attempt to advance his view that nature performs not only deductions, but inductions and retroductions (abductions) as well.

Relieved of the duty to prepare regular lectures, Peirce could now take time to ponder his cosmological speculations. In the coming months his commitment to the Survey would wane, as his methods became less appreciated and his duties became fewer, and he could take even more time for deep reflections. He would soon be ready to make his guess at the riddle of the universe.

NATHAN HOUSER

Read's Theory of Logic[1]

P 148: Nation *28 (3 April 1879): 234–35*

This work is the fruit of a travelling scholarship. But in all his travels the author seems never to have come across any modern logic, except in English. Three views, he observes, have been taken of logic; which, if limited to England, is true. Some writers consider it as a study of the operations of the understanding, thus bringing it into close relations with psychology. Others regard it as an analysis of the conditions which must be conformed to in the transformations of verbal expressions in order to avoid the introduction of falsehood. While others again—our author among them—think the propositions of logic are facts concerning the things reasoned about.

There is certainly this to be said in favor of the last opinion, namely, that the question of the validity of any kind of reasoning is the question how frequently a conclusion of a certain sort will be true when premises of a certain sort are true; and this is a question of fact, of how things are, not of how we think. But, granted that the principles of logic are facts, how do they differ from other facts? For facts, in this view, should separate themselves into two classes, those of which logic itself takes cognizance and those which, if needed, have to be set up in the premises. It is just as if we were to insist that the principles of law were facts; in that case we should have to distinguish between the facts which the court would lay down and those which must be brought out in the testimony. What, then, are the facts which logic permits us to dispense with stating in our premises? Clearly those which may always be taken for granted: namely, those which we cannot consistently doubt, if reasoning is to go on at all; for example, all that is implied in the existence of doubt and of belief, and of the passage from one to the other, of truth and of falsehood,

1. *The Theory of Logic: an Essay.* By Carveth Read. London, 1878.

of reality, etc. Mr. Read, however, recognizes no such distinction between logical principles and other facts. For him logic simply embraces the most general laws of nature. For instance, he recognizes as a logical principle the law of the conservation of energy, which is even yet hardly set beyond all doubt. If he excludes the laws of geometry, as being "quantitative," it is by an ill-founded distinction. If he does not mention the law of gravitation nor the existence of a luminiferous ether as logical principles, it must probably be because he thinks them less general truths than the laws of motion.

The especial purpose of the book is to arrange the principles of logic, considered as matters of fact, in regular order, beginning with the most abstract and general, and proceeding towards the particulars. In short, it is an attempt to give a syllabus of the most general laws of nature. This is a well-conceived idea.

After the introduction, the first chapter treats of *Relation.* We notice immediately the illogic of thus making relation the most abstract of facts. Existence should come first and quality next; no competent logician, however he might modify this statement, will deny its approximate truth. Why does Mr. Read not begin with *Being?* Is it because the writers he follows greatly insist on the point that existence and qualities depend on relations? There is this dependence, no doubt; the abstract and general always depend on the concrete and particular. But having undertaken to arrange the subject in synthetical order, which consists in putting the abstract before the concrete, Mr. Read should not violate the principle of arrangement at the very outset. Turning, however, to the substance of the chapter, we are told that relation cannot be defined. This is not exact; it can and has been defined; but what is true is that it cannot be defined without considering the operations of the mind or the general nature of language. But the author is endeavoring to state the principles of logic without referring to either of these. He is, therefore, unable to explain the notion of relation, because to do so he must explicitly introduce those notions which he wishes to exclude. Not being able to define relation, he typifies it. This he does by the following figure—two spots united by a line:

But here he betrays a not altogether distinct conception of relation. These two spots are similarly related to one another. Now there are certainly relations of this kind. If A is like B, B is like A; if A is unlike B, B is unlike A, etc. But, generally speaking, two related objects are in different relations to one another. The relation of father to son, for example, is different from the relation of son to father. So that if we desire to make a sort of hieroglyph for relation in general, it should be something like this: A $\longrightarrow$ B.

We next meet with an enumeration of the ultimate modes of relation. These are stated to be three—viz.:

1. Likeness and unlikeness.
2. Succession and non-succession.
3. Coexistence and non-coexistence.

Succession is defined as unlikeness in time; and coexistence as likeness in time. If that be so, the second and third modes are not ultimate, but are only species of the first. Substituting the definitions for the terms defined, they are:

2. Unlikeness in time and non-unlikeness in time.
3. Likeness in time and non-likeness in time.

Hardly a model of synthetic orderliness.

But what does the author do with the great body of relations? What pigeon-holes has he for them in his scheme of arrangement? Take, for instance, the relation of striker to struck. A man's striking another constitutes certainly no resemblance between them. But neither is it an unlikeness, for a man may strike himself, and since he is then a striker only so far as he is struck, and *vice versa,* it is impossible to say that striker and struck are unlike. In short, the relation is neither a likeness nor an unlikeness, for the reason that both these latter are relations between objects similarly related to one another, while the relation of striker to struck, like most relations, is between dissimilarly related objects.

The few pages we have thus examined are a fair specimen of the strength of the whole book. Its purpose is a sharply-defined one; its style is clear and free from verbiage; and if it is not a striking success, it is because its author is not thoroughly well grounded in his subject.

Spectroscopic Studies

P 163: Science News *1 (1 May 1879): 196–98*

Two papers were presented by Mr. Peirce, entitled respectively, "On Ghosts in Diffraction-Spectra" and "Comparison of Wave-Lengths with the Metre." It is well known to users of diffraction spectroscopes that ghosts of the lines appear in the images. For instance, on each side of the well-known sodium line, a ghost of it is seen. These attendants only appear in spectra produced by diffraction, and are not found in the spectrum from a prism. They are due to periodic inequality in the ruled lines of the glass. If we suppose that the screw which makes the ruling is somewhat eccentric, we shall find that this eccentricity—so to speak—winds down around the screw. But every diffraction-plate which Mr. Peirce examined was found to have a different eccentricity. In the higher orders of spectra, the first ghost of each line becomes relatively brighter. Mr. Peirce has investigated this subject from a mathematical point of view, and he presented to the Academy a series of calculations based on the conditions which call forth these ghosts, and concluding with formulæ for determining their positions.

In conjunction with Mr. Rutherfurd, Mr. Peirce has been investigating the relation of the wave-lengths of light to the metre. The object is to obtain a basis for measuring the standard metre. The metres that have been issued as standards change in length after a lapse of time. The German metre is said to differ from the French metre by one 25,000th. Mr. Peirce proceeded on the assumption that the wave-lengths of light are of a constant value. We cannot say, however, that on that point we are perfectly certain; there may be a variation in wave-lengths if the ether of space, through which the solar system is travelling, has different degrees of density. But as yet we are not informed of such variation.

The idea of using wave-lengths as a standard of comparison is not new. Arago has suggested it. Ste.-Claire Deville and Mascart made a step in that direction in the measurement of the distances between a flat and a lenticular surface when the Newtonian rings are produced. But the task which was undertaken by Messrs. Peirce and Rutherfurd is more difficult. Indeed, it seems at first sight foolhardy to attempt to attain the measure of a wave-length within 1,000,000th of itself; to be accurate within 1,000th of the distance between the D lines that were formerly considered difficult of separation. The definition now obtained is very much better than was expected; the lines themselves are much finer.

Certain questions have arisen in the course of this research. It was necessary to ascertain whether the spectral lines were fine enough to serve the purpose. There was a doubt as to whether the lines were displaced by "ghosts," and this led to the mathematical inquiry, previously alluded to, which has defined the position of ghosts relatively to the lines. Again, it was found needful that the spectrum to be observed should be at its maximum of brilliance. It had been noticed that two spectra composing a pair—(that is, of the same order)—are usually of different brightness, the right side spectrum differing from the left side one. This was specially true of spectra obtained from ruled glass; those from speculum metal were not so notably diverse in brightness. Examination showed that this characteristic was due to a difference in the sides of the groove ruled in glass. The diamond, in ploughing through the surface, raises a bur on the side of the furrow, and, hence, makes the two sides of the cut of unequal height. At first it was attempted to remove this imperfection by rubbing off the bur; but it was found that the material of the bur went to fill up the groove, and thus rendered the glass-plate unserviceable. But, by first filling the groove with black-lead, then polishing off the bur, and finally removing the black-lead, plates were obtained that gave spectra of the utmost brilliancy, and the right and left spectra of each pair did not differ in brightness from each other.

Mr. Peirce also gave the particulars of other improvements recently made in spectroscopic apparatus. One of these involved the construction of glass circles, and the work was so delicate that a well-known instrument maker had failed in four attempts. A method was described by which the accurate focussing of the heliostat—a matter of great importance—had been satisfactorily attained.

In the experiments for ascertaining the relation of a wave-length to the metre, use was made of a line between D and E; the first prominent line after D. The probability of error with the instrument, as first constructed, was one 200,000th of a wave-length; but better results are anticipated from improved methods. Lines on polished metal serve excellently for the measurements; you can see down to the bottom of each line. The comparator used had heavy ways of cast iron, cemented down with plaster of Paris to a block of marble. Carriages run on the ways, bearing microscopes and scales. The details of the apparatus cannot be properly explained without diagrams. Minute precautions are taken to prevent distortion. The experimenters have succeeded in measuring a number of decimetre scales by centimetres. The probability of a single error is within the fiftieth part of a micron. (A micron is as much smaller than a millimetre as the latter is less than a metre.) Means have been devised which keep the apartment, where the experiments are made, at a fixed temperature, within one-tenth of a degree of Fahrenheit. With a sufficient number of observations, and the use of apparatus having their latest improvements, these experimenters hope to attain the object of their research, and limit the error to one-millionth part of a wave-length.

[Lecture on Logic and Philosophy]

MS 342: May 1879

We have hitherto applied the rule of clearness to ordinary conceptions and have found our advantage in it. In a paper in the *Popular Science Monthly* I have analyzed in a similar way the notion of reality, with the result of reaching a certain ontological theory the full development of which might occupy us for the remainder of this evening. I prefer, however, upon the present occasion, in order to make my remarks as useful as possible to young students of philosophy, to consider a very singular class of conceptions which represent, not facts about the objects to which those conceptions are applied, but the facts and principles of logic itself.

You are all more or less familiar I suppose with the common doctrine of logical extension and comprehension. Take any ordinary *[. . .]* comprehension of the latter, N, embraces the whole of that of the former, M, and more besides. For since the extension of M embraces the whole of that of N (that is, since of whatever thing we can predicate N we can also predicate M) it follows that

Every N is M.

Hence whatever can be predicated of M can also be predicated of N; or in other words the comprehension of N embraces the whole of that of M. And since there are other things of which M can be predicated than those of which N can be predicated we may write

Some M is not N;

whence it follows that some things can be predicated of N which cannot be predicated universally of M, or the comprehension of N embraces something besides that of M.

Hence the general proposition that the greater the extension of breadth of a term the less its comprehension or depth. There are various limitations to the truth of this general principle which you may find carefully stated in the *Proceedings of the American Academy of Arts & Sciences* for Nov. 13, 1867. For the present purpose we may pass these limitations by. The idea of these relations is illustrated by the tree of Porphyry which is here shown

Substance

Corporeal Incorporeal

Animate Inanimate

Sensitive Non-sensitive

Rational Irrational

Socrates Plato etc.

Here is a Jacob's ladder by which we mount from the ground of individuals to the heaven of universal conceptions.

It is a common remark that all determination is through negation; that is to say we only perceive any quality by noticing that some things agree in possessing it and differ from others which possess it not. A quality which occurred once & once only would not be apprehended by the mind; a quality which all things equally possessed would be like the fancied music of the spheres which we never hear because it never ceases. In short, such a universal quality would never attract our attention.

It follows that our Jacob's ladder properly speaking has no definite top nor bottom; above it is lost in the clouds of heaven, below in the smoke of the pit. Those of you who have read the *Kritik der reinen Vernunft* will remember that Kant distinctly lays this down.

Nevertheless, when we examine not the relations of the objects named but the relations of the names themselves as shown in the Jacob's ladder, we are easily led to the conception of a name whose logical comprehension shall be *null* and its extension unlimited, and this name is *Being.* We are also led to the conception of a name whose extension shall be *null* & its comprehension unlimited, & this name is *Nothing.* For we may predicate whatever we like of nothing and there will be no falsity in the proposition. Thus we attain a pair of clear metaphysical ideas of a most abstract nature. Being & Nothing are not mere Reality & Figment. For even a figment is *something,* and therefore comes under the head of being. But the pure metaphysical *being* and *nothing* are conceptions which have little place in our ordinary thoughts. What I wish to call attention to is the very clear apprehension we obtain of these notions when we consider them as representing the fictitious extremes of the logical Jacob's ladder. For instance Hegel as you know tries to make out that Being is Nothing & nothing being. And how? Why he says Being is a purely empty conception. That is true in this sense that it is the conception of a conception devoid of logical comprehension. But that does not make it at all like Nothing which is only devoid of Extension & has unlimited Comprehension. Hegel would therefore not have fallen into his contradiction if he had made his metaphysical conceptions clear by insisting on determining what the phenomena of logic are which these conceptions represent.

Note on the Progress of Experiments for comparing a Wave-length with a Metre

P 136: **American Journal of Science and Arts,**
3rd ser. 18 (July 1879): 51

To C. P. PATTERSON, Superintendent U.S. Coast and Geodetic Survey:—

DEAR SIR

The following is the present state of the Spectrum metre business. The deviation of a spectral line (Van der Willigen's No. 16) has had three complete measures using a certain gitter of $340\frac{1}{2}$ lines to the millimetre. The double deviation (the angle measured) was found to be

1877			
June 23	89°	54′	19″.5
June 29 and July 2			19.25
Sep. 4 and Aug. 27			19.65
Mean———	89°	54′	19″.5

An error of 0″.4 in this would occasion an error of one micron in the metre. These measures were previously communicated to you, but owing to an erroneous value of the coëfficient of expansion of glass having been used (the value for iron having been inadvertently substituted) they did not seem to agree so well as they do. There were two other complete measures but in regard to one of them there is a doubt about the thermometer used, and in regard to the other there is a doubt about the part of the line set on.

This line seems on the whole to be a bad one for the purpose. Another line near it was therefore selected and another much finer gitter. The deviations obtained were on the different days

1879			
May 8	90°	03′	51″.7
May 9			51.75
May 10			52.0
May 15			50.35
May 21			51.75
May 22			51.2
Mean———	90°	03′	51″.45

Notwithstanding the bad result of May 15, which is unaccountable, these measures are evidently good enough.

One of these gitters has been compared with all the centimetres of a decimetre scale of centimetres. The other is still to be compared with all the even two centimetres of the same scale.

Mr. Chapman is now comparing this decimetre scale with all the decimetres of a metre scale of decimetres.

As soon as that is done *a metre* will have been compared with a wave-length. But shortly after, this will be improved by comparing the other gitter and also a third upon which I propose to measure a deviation.

It will remain, *first,* to find the coëfficient of expansion of the glass metre. The apparatus is all ready for this and it will not take a fortnight. Second, the glass metre will have to be compared with a brass metre. This will be an operation of some difficulty but I think we shall complete it before long.

Yours respectfully, C. S. PEIRCE, *Assistant.*

Cambridge 1879 May 23

On a method of swinging Pendulums for the determination of Gravity, proposed by M. Faye

P 137: **American Journal of Science and Arts,**
3rd ser. 18 (August 1879): 112–19

At the Stuttgart, 1877, meeting of the International Geodetic Association, M. Faye suggested a method of avoiding the flexure of a pendulum-support, which promises important advantages. The proposal was that two similar pendulums should be oscillated on the same support with equal amplitudes and opposite phases. If the pendulums could be made precisely alike, the amplitudes precisely equal, and the phases precisely opposite, it is obvious that the support would be continually solicited by two equal and opposite forces and would undergo no horizontal flexure, except from the distortion of the parts between the two edges. But since none of these three elements can be made equal, it is necessary to inquire what would be the effect of such slight imperfections in their equalization as would have to be expected, in practice.

I had the advantage many years ago of learning the main characteristics of the mutual influence of pendulums from Professor Benjamin Peirce. As my father's studies of the subject were never, I believe, written out, I am unable to say definitely what I derive from that source. But the truth is the little knowledge I have of mathematics was learned from him, and from him I got a clear idea of the nature of this particular problem; so that acknowledgements of detail, even if I were able to make them, would be quite inadequate.

In M. Faye's proposed experiment, four finite forces would be in operation; namely, the weights of the two pendulums, the elastic force tending to restore the two knife-edge supports to their position of equilibrium when they are both displaced together, and the elastic force tending to restore them when their relative positions are displaced. The system has, also, four degrees of freedom corresponding to motions against each of the four finite forces. Accordingly, there will be four differential equations of motion. By neglecting the terms of the second order, these equations are made linear, and by the general theory of such equations they indicate that each of the four motions of the system (viz., those of the pendulums and of the two knife-edges) is compounded of four simple harmonic motions. Two of these will have periods nearly equal to those of the pendulums, the other two will be mere tremors having periods nearly those of the natural elastic oscillations of the supports. These tremors will be so small that they may be neglected. In fact, if we simply suppose that the knife-edges are constantly in equilibrium under the various forces which solicit them (which is simply to neglect their living forces under their very small velocities) the tremors disappear, to the great simplification of the formulae.

Putting, then, φ_1 and φ_2 for the momentary angles of displacement of the two pendulums, s_1 and s_2 for the momentary horizontal displacements of the two knife-edges, l_1 and l_2 for the lengths of the two equivalent simple pendulums (on an absolutely rigid support), g for the acceleration of gravity, and t for the time, we have

$$l_1\frac{d^2\varphi_1}{dt^2}+\frac{d^2s_1}{dt^2}=-g\varphi_1$$

$$l_2\frac{d^2\varphi_2}{dt^2}+\frac{d^2s_2}{dt^2}=-g\varphi_2.$$

These equations are exactly like what we have in the case of a single pendulum on a flexible support; and I have shown their correctness in my paper on that subject.

There would be no difficulty in making the two pendulums so nearly alike that they might be regarded as entirely so in their actions on the stand, the whole amount of which is small. We may also consider the parts of the stand on which the two knives rest as equally elastic. We may, therefore, take $\frac{1}{2}(s_1+s_2)$ as proportional to $\frac{1}{2}(\varphi_1+\varphi_2)$, and $\frac{1}{2}(s_1-s_2)$ as proportional to $\frac{1}{2}(\varphi_1-\varphi_2)$. Denoting, then, by x and y two constants whose values will be easily determinable by experiments, we have

$$s_1 + s_2 = (x + y)\,(\varphi_1 + \varphi_2)$$
$$s_1 - s_2 = (x - y)\,(\varphi_1 - \varphi_2);$$

or

$$s_1 = x\varphi_1 + y\varphi_2$$
$$s_2 = x\varphi_2 + y\varphi_1.$$

Substituting these values of s_1 and s_2 in the differential equations, and also writing $l + \delta l$ for l_1 and $l - \delta l$ for l_2, they become

$$(l + x + \delta l)\frac{d^2\varphi_1}{dt^2} + y\frac{d^2\varphi_2}{dt^2} = -g\varphi_1$$
$$(l + x - \delta l)\frac{d^2\varphi_2}{dt^2} + y\frac{d^2\varphi_1}{dt^2} = -g\varphi_2.$$

The solution of these equations is (A, B, t_1, and t_2 being the arbitrary constants)

$$\varphi_1 = \mathrm{A}\cos\left\{\sqrt{\frac{g}{l + x - \sqrt{(\delta l)^2 + y^2}}}\cdot(t - t_1)\right\}$$
$$+ \mathrm{B}\cos\left\{\sqrt{\frac{g}{l + x + \sqrt{(\delta l)^2 + y^2}}}\cdot(t - t_2)\right\}$$

$$\varphi_2 = -\mathrm{A}\left(\frac{\delta l}{y} + \sqrt{1 + \left(\frac{\delta l}{y}\right)^2}\right)\cos\left\{\sqrt{\frac{g}{l + x - \sqrt{(\delta l)^2 + y^2}}}\cdot(t - t_1)\right\}$$
$$-\mathrm{B}\left(\frac{\delta l}{y} - \sqrt{1 + \left(\frac{\delta l}{y}\right)^2}\right)\cos\left\{\sqrt{\frac{g}{l + x + \sqrt{(\delta l)^2 + y^2}}}\cdot(t - t_2)\right\}.$$

The condition that the pendulums are started by drawing them away from their positions of equilibrium and then letting them escape nearly at the same instant, makes t_1 and t_2 nearly equal. We may reckon the time from the mean instant of starting. Then, at that instant we have very nearly

$$\varphi_1 = \mathrm{A} + \mathrm{B}$$

$$\varphi_2 = -\mathrm{A}\left(\frac{\delta l}{y} + \sqrt{1 + \left(\frac{\delta l}{y}\right)^2}\right) - \mathrm{B}\left(\frac{\delta l}{y} - \sqrt{1 + \left(\frac{\delta l}{y}\right)^2}\right);$$

or, if we write z for $\dfrac{\delta l}{y}$,

$$\varphi_2 = -\mathrm{A}(z + \sqrt{1 + z^2}) - \mathrm{B}(z - \sqrt{1 + z^2}).$$

And since the amplitudes are nearly equal and the phases nearly opposite,

$$\varphi_1 = -\varphi_2,$$

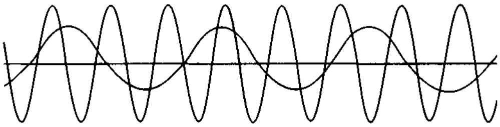
or

$$A + B = (\mathit{nearly})\, A(z + \sqrt{1 + z^2}) + B(z - \sqrt{1 + z^2}).$$

This gives

$$\frac{B}{A} = (\mathit{nearly}) \frac{\sqrt{1 + z^2} - 1 + z}{\sqrt{1 + z^2} + 1 - z}.$$

There would be no insuperable difficulty in making the pendulums so near alike that δl should be less than y, even if the latter quantity were smaller than it would be likely to be. But it will be seen presently that care must be taken in the construction not to make y too small. We shall have, then, $\delta l < y$ or $z < 1$; whence $B < A$. Thus, the amplitudes of the first terms in the expressions for both φ_1 and φ_2 are greater than those of the second terms, while the period of the first terms is shorter than that of the second terms. From this it can be shown to follow that the whole oscillations of the two pendulums have the same period which is that of the harmonic motions represented by the first terms of their values. Thus, in the figure, the abscissas representing the time, we have a wave of short period and large amplitude placed in comparison with a wave of long period and small amplitude.



The phase of the short wave advances on the long one and goes over and over it. In each complete cycle of the curve representing the short wave (beginning and ending at $y = 0$), it must cut the other curve twice unless the latter has meantime crossed the axis of abscissas, once and not twice. When this happens there will be three intersections or only one according to the direction of the crossing. Hence when the short curve has advanced over any even number of crossings by the long one of the axis of abscissas, the mean number of intersections per cycle of the short curve will be exactly two. Now let the short curve represent the first term in the expressions for φ_1 or φ_2 and let the long curve represent the second term *with its sign changed;* then, the intersections will represent passages of the pendulum over the vertical and it will be seen that there are two for each complete period of the quicker harmonic component of the motion.

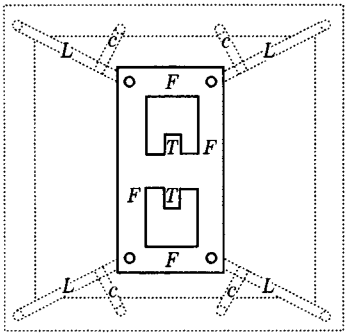
The mean period, then, of the oscillation of either pendulum will be

$$T = \pi\sqrt{\frac{l + x - \sqrt{(\delta l)^2 + y^2}}{g}}.$$

Now let us suppose that δl is so small that $\frac{1}{2}\frac{(\delta l)^2}{ly}$ may be neglected, being less than one-millionth. This would happen, for instance, if l were one metre, y a half a millimetre (so that the stand would be somewhat less stiff than the Repsold tripod), and δl were one twenty-fifth of a millimetre so that the difference between the natural times of oscillation of the two pendulums was not over four seconds a day, a perfectly attainable adjustment. Then the period would reduce to

$$T = \pi\sqrt{\frac{l + x - y}{g}}.$$

The terms $x - y$ here indicate that the apparatus would still be subject to a correction for flexure; but it would be only for the relative flexure due to the distortion of the support between the two knife-edges. This could of course be made very small. It would still have to be measured; but it would be measured once for all, since it would be the same at all stations. At present, the measurement of the flexure at each station, involving as it does the erection of a separate pier, threatens to be one of the most troublesome and expensive parts of the whole work of determining gravity. This would be entirely obviated by M. Faye's plan, except that the small differential flexibility would have to be determined once for all. The proper way to make the stand so as to bind the two knives to their relative position as firmly as possible while allowing a moderately large flexibility to the whole stand, so that the two pendulums could freely influence one another, would easily be found out.



The accompanying figure, for instance, represents one such arrangement, as viewed from above. T, T, are tongues upon which the pendulums would rest. These would be cast in one piece with the heavy frame F, F, F, F. This frame would rest on four legs L, L, L, L, which would spread at the bottom in the direction of the motion of the pendulum. At the bottom they would be bolted into another heavy frame. The support-braces C, C, C, C, would prevent twisting.

The average period of oscillation of either pendulum, after correction for flexure, would be that belonging to a simple pendulum having the length l, the mean of the lengths of the two simple pendulums whose natural periods of oscillation would be the same as those of the given pendulums. But although this would be the average time of oscillation of either pendulum, yet neither pendulum would have all its oscillations of the same duration. It is therefore necessary to inquire what error might arise owing to the observations not extending over any exact number of cycles of motion, so that the mean of the observed periods would not be the same as the mean of the periods of a cycle.

The quickest oscillation of either pendulum would occur when the phases of the component harmonic motions were coincident, the slowest when these phases were opposed. The period of the slow harmonic component motion would be

$$\pi \sqrt{\frac{l+x+y}{g}}$$

or the mean of the periods of the two given pendulums oscillating on the given stand with coincident phases, so as to be affected by the flexibility of the whole stand but not by its liability to distortion. Suppose, then, that in the course of the experiment an instant comes at which the pendulums are vertical at once. Let us reckon the time from this instant, and put

$$\mathrm{I} = \frac{\mathrm{A}}{\mathrm{B}} \cdot \frac{\sqrt{1+z^2}-1+z}{\sqrt{1+z^2}-1-z},$$

so that I is nearly unity. Then using the abbreviations

$$\sin_1 = \sin\left\{\sqrt{\frac{g}{l+x-\sqrt{(\delta l)^2+y^2}}}\cdot t\right\}$$

$$\sin_2 = \sin\left\{\sqrt{\frac{g}{l+x+\sqrt{(\delta l)^2+y^2}}}\cdot t\right\}$$

we have

$$C\varphi_1 = (\sqrt{1+z^2}+1-z)\sin_1 \pm \mathrm{I}(\sqrt{1+z^2}-1+z)\sin_2$$

$$C\varphi_2 = (-\sqrt{1+z^2}-1-z)\sin_1 \pm \mathrm{I}\,(-\sqrt{1+z^2}+1+z)\sin_2,$$

where the double sign distinguishes between coincidence and opposition of the phases of the harmonic constituents at the zero of t.

Then since the value of z is between 0 and unity, the values of these four coëfficients lie

$\sqrt{1+z^2}+1-z$ between	2 and	1.414
$\sqrt{1+z^2}-1+z$	0	1.414
$-\sqrt{1+z^2}-1-z$	-2	-3.414
$-\sqrt{1+z^2}+1+z$	0	0.586.

It follows that for one pendulum the phases of the harmonic constituents are coincident at the moment when they are for the other in exact opposition. Hence, one pendulum is making its slowest oscillation at the moment when the other is making its quickest and *vice versa.* Then, from the symmetrical character of harmonic motion it follows that if observations were taken of both pendulums during any interval of time, then the mean of the average periods of the two during that interval, would give the mean period of either through a complete cycle of motion. A better method of observing, however, would be to set up a lens between the two pendulums so as to bring the plane of oscillation of the one into focus on the plane of oscillation of the other. Then, by means of a reading telescope set up at a little distance, the oscillation at which both crossed on the vertical could be noted with some accuracy. It would then only be necessary to determine the mean period of oscillation of either from one such event to another. As the difference between the longest and shortest periods of oscillation would only amount to a few ten-thousandths of a second, it would not be necessary to be very exact in the time of beginning or ending the experiment. The number of oscillations between one coincidence at the vertical and another would afford a very accurate determination of y. For suppose n to be that number. Then,

$$n\sqrt{\frac{l+x-y}{g}} = (n-1)\sqrt{\frac{l+x+y}{g}},$$

whence,

$$l+x = \left(n - \frac{n-1}{2n-1}\right)y.$$

But as n is large (several thousand) we may take $\frac{n-1}{2n-1} = \frac{1}{2}$, and x as equal to y. This gives

$$y = \frac{l}{n - \frac{3}{2}}.$$

Then, $x - y$ having been determined, we ascertain the value of x also.

The greatest departure of the oscillations of the two pendulums from complete opposition of phase would occur when the phases of the harmonic components differed by a quadrant. In this case, the pendulums would cross at an angle equal to $\mathrm{CI}\frac{\delta l}{y}$ from the vertical. The difference in the time of their passage over the vertical could only amount to a minute fraction of a second.

If the pendulums should not be nearly enough adjusted to the same natural period, or if the stand should be too stiff, so that δl were greater than y, the slower harmonic component would have a greater amplitude than the quicker one. In this case, the pendulums would pass over all differences of phase, and whether the mean period of oscillation were that of the faster or of the slower component might depend upon the initial phases or, if δl were still larger relatively to y, it might be the same as if the pendulums were oscillating with coincident phases. Care would have to be taken to avoid such a state of things.

On the whole, it appears that the suggestion of M. Faye, though it was thrown out on the spur of the moment, and was not received with very warm approval on every hand, is as sound as it is brilliant, and offers some peculiar advantages over the existing method of swinging pendulums.

Feb. 17, 1879.

On the Algebraic Principles of Formal Logic

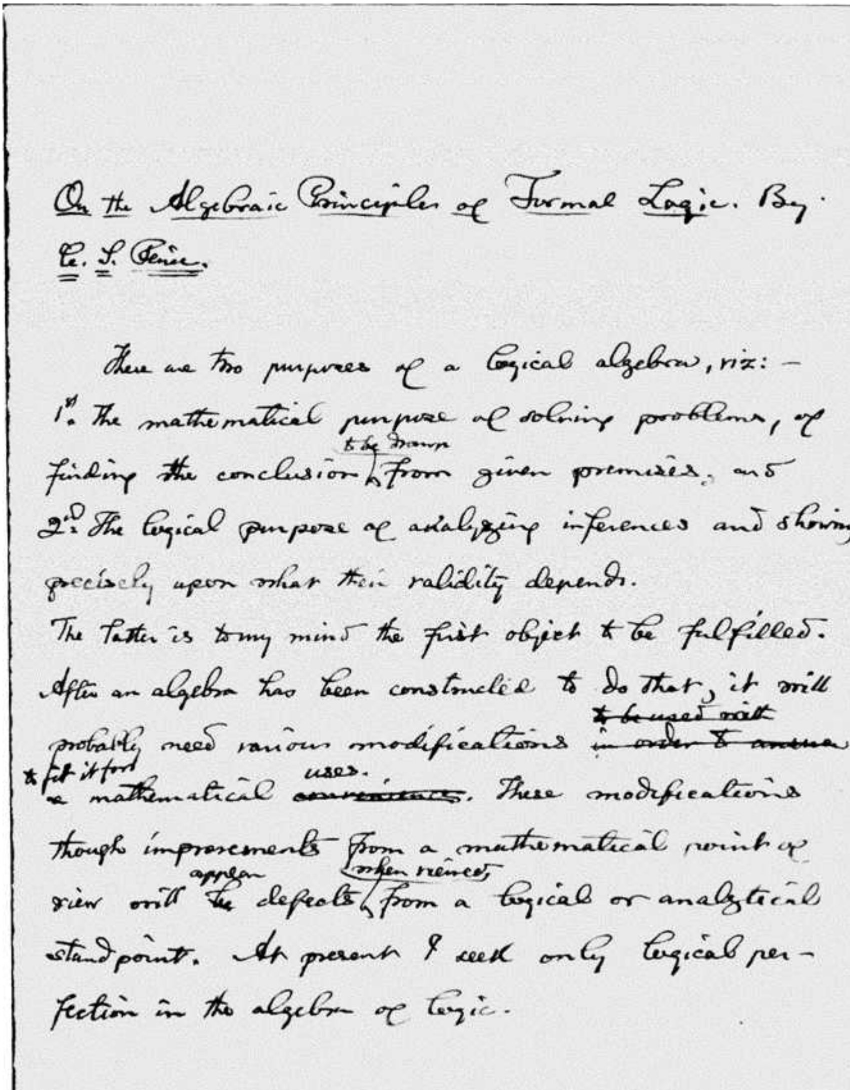
MS 348: Fall 1879

There are two purposes of a logical algebra, viz.:—1st The mathematical purpose of solving problems, of finding the conclusion to be drawn from given premises, and 2nd The logical purpose of analyzing inferences and showing precisely upon what their validity depends.

The latter is to my mind the first object to be fulfilled. After an algebra has been constructed to do that, it will probably need various modifications to fit it for mathematical uses. These modifications though improvements from a mathematical point of view will appear defects when viewed from a logical or analytical standpoint. At present I seek only logical perfection in the algebra of logic.

The effort to trace analogies between ordinary or other algebra and formal logic has been of the greatest service; but there has been on the part of Boole and also of myself a straining after analogies of this kind with a neglect of the differences between the two algebras, which must be corrected, not by denying any of the resemblances which have been found, but by recognizing relations of contrast between the two subjects.

Some writers have supposed that there was a connection between the algebraical view of formal logic and the doctrine of the quantification of the predicate. But this is far from being true. I have irrefragably proved, and no one has ventured to dispute my argument, that a proposition with a quantified predicate is a compound of two propositions whose predicates are unquantified. The proof is this. The proposition 'Any A is any B' expresses the *identity* of A and B. The proposition 'Any A is B' expresses the *inclusion* of A under B. Now all *identity* is *inclusion* but the reverse is not true. Hence, *inclusion* is a concept having a greater logical extension than *iden-*


On the Algebraic Principles of Formal Logic. By C. S. Peirce.

There are two purposes of a logical algebra, viz:—
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2nd. The logical purpose of analyzing inferences and showing precisely upon what their validity depends.
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Holograph page from MS 348
(Harvard Peirce Papers MS 575)

tity. Hence, the logical comprehension of *identity* is greater than that of *inclusion,* or in other words *inclusion* is a simpler idea than *identity.*

I, accordingly, adopt the copula of inclusion; because the first object of logical algebra is to represent the ultimate analysis of arguments. This copula, I represent by the sign $\prec$. This is a different way of writing $\leqq$. The latter sign is logically objectionable because it involves a definition by enumeration, and seems to represent inclusion as less simple than identity.

This reasoning is absolutely irrefragable; and he who does not accept it simply shows that he does not comprehend the essential purpose of logic, as the analysis of argument, not the art of drawing inferences.

Whichever copula be adopted, it is impossible to represent particular propositions by ordinary class symbols and symbols of their combinations without the introduction of relative terms. The reason is that a particular proposition is not merely the subsumption of one class under another, but asserts the *existence* of something.

On this account, it might be preferable, instead of the copula of inclusion to adopt two, viz.:—

1st, $\curlyvee\!\!\!\prec$, the copula of exclusion; writing No A is B, A $\curlyvee\!\!\!\prec$ B; and

2nd $\succ\!\!\prec$, the copula of intersection; writing Some A is B, A $\succ\!\!\prec$ B.

This system deserves following out. For the present, I content myself with pointing it out.

The copula of inclusion is defined as follows:—

1st, $A \prec A$, whatever A may be.

2nd If $A \prec B$, and $B \prec C$, then $A \prec C$.

This definition is sufficient for the purposes of formal logic, although it does not distinguish between the relation of inclusion and its converse. Were it desirable thus to distinguish, it would be sufficient to add that the real truth or falsity of $A \prec B$, supposes the existence of A.

I define the logical sum as follows:—

1st, $A \prec A +\!\!, B$

2nd $B \prec A +\!\!, B$

3rd If $A \prec C$ and $B \prec C$, then $A +\!\!, B \prec C$.

From this we easily prove that logical addition is associative. For by the first condition

$$y \prec y +\!\!, z. \tag{1}$$

By the second condition

$$(y \mathbin{+\!\!,} z) \prec x \mathbin{+\!\!,} (y \mathbin{+\!\!,} z). \tag{2}$$

Hence, by the second condition in the definition of the copula,

$$y \prec x \mathbin{+\!\!,} (y \mathbin{+\!\!,} z). \tag{3}$$

But by the first condition of the definition of the sum

$$x \prec x \mathbin{+\!\!,} (y \mathbin{+\!\!,} z) \tag{4}$$

Hence, by the third condition

$$(x \mathbin{+\!\!,} y) \prec x \mathbin{+\!\!,} (y \mathbin{+\!\!,} z) \tag{5}$$

In a similar way it can be shown that

$$z \prec x \mathbin{+\!\!,} (y \mathbin{+\!\!,} z)$$

Hence, by the third condition,

$$(x \mathbin{+\!\!,} y) \mathbin{+\!\!,} z \prec x \mathbin{+\!\!,} (y \mathbin{+\!\!,} z).$$

In the same way, it could be shown that

$$x \mathbin{+\!\!,} (y \mathbin{+\!\!,} z) \prec (x \mathbin{+\!\!,} y) \mathbin{+\!\!,} z$$

or

$$x \mathbin{+\!\!,} (y \mathbin{+\!\!,} z) = (x \mathbin{+\!\!,} y) \mathbin{+\!\!,} z.$$

The proof that logical addition is commutative is still more simple.

By the first condition $y \prec y \mathbin{+\!\!,} x$
By the second, $x \prec y \mathbin{+\!\!,} x$
Hence, by the third, $x \mathbin{+\!\!,} y \prec y \mathbin{+\!\!,} x$.

From the first condition of inclusion

$$x \prec x$$

Hence by the third condition of addition

$$x \mathbin{+\!\!,} x \prec x$$

By the 1^{st} or 2^{nd} condition of addition

$$x \prec x \mathbin{+\!\!,} x.$$

I define the logical product as follows:—
1^{st}, A,B $\prec$ A;
2^{nd}, A,B $\prec$ B;
3^{rd}, If C $\prec$ A and C $\prec$ B, then C $\prec$ A,B.

The exact correspondence between logical addition and multiplication appears from these definitions; and was, as Schröder acknowledges, recognized by me in my first paper on the subject.

We have, therefore,

$$(x,y),z = x,(y,z)$$
$$x,y = y,x$$
$$x,x = x.$$
$$\text{If } x \prec y, \text{ then } x,z \prec y,z.$$

The proof of the propositions

$$(x \mathbin{+\!\!,} y),z = x,z \mathbin{+\!\!,} y,z$$
$$x,y \mathbin{+\!\!,} z = (x \mathbin{+\!\!,} z)(y \mathbin{+\!\!,} z)$$

given by me in my paper of March 1867, seems too obvious for repetition.

The same is true of Schröder's formulae,

$$x \mathbin{+\!\!,} x,y = x,$$
$$x,(x \mathbin{+\!\!,} y) = x.$$

Also, the following extension of a highly interesting theorem of Schröder,

$$\text{If } mx \prec my \text{ and } m \mathbin{+\!\!,} x \prec m \mathbin{+\!\!,} y, \text{ then } x \prec y.$$

The definition of *what is* is

$$x \prec 1 \quad \text{whatever } x \text{ may be.}$$

The definition of *nothing* is

$$0 \prec x \quad \text{whatever } x \text{ may be.}$$

Hence it follows directly that

$$x,1 = x \qquad x \mathbin{+\!\!,} 0 = x$$
$$x \mathbin{+\!\!,} 1 = 1 \qquad x,0 = 0$$

Also

if $x \mathbin{+\!\!,} y = 0$, then $x = 0$ and $y = 0$.
if $x,y = 1$, then $x = 1$ and $y = 1$.

Schröder takes

$$x,1 = x$$

as an axiom; and

$$x \dotplus 0 = x$$

as a theorem; deducing it from the former by means of

$$x, \bar{x} = 0 \quad \text{and} \quad x \dotplus \bar{x} = 1$$

where $\bar{x}$ is non-x. But it follows, at once, from $x \dotplus \bar{x} = 1$ that $x \prec 1$, whence $x, 1 = x$ and the so called axiom receives proof.

The negative must be defined by the formulae of contradiction and excluded middle.

$$x, n\text{-}x \prec 0 \qquad 1 \prec x \dotplus n\text{-}x.$$

Suppose $x \prec y$. Multiply by $n\text{-}y$

$$x, n\text{-}y \prec 0$$

Add $n\text{-}x$

$$x, n\text{-}y \dotplus n\text{-}x \prec n\text{-}x$$

But $\quad n\text{-}y \prec x, n\text{-}y \dotplus n\text{-}x, n\text{-}y \prec x, n\text{-}y \dotplus n\text{-}x$

$\therefore \quad n\text{-}y \prec n\text{-}x.$

[The Algebraic Principles (A)*]*

Definition of the Copula of Inclusion.

(1) $x \prec x$

(2) If $x \prec y$ and $y \prec z$, then $x \prec z$.

This definition does not distinguish between the relation expressed by the copula and its converse. But no further determination is relevant to formal logic.

Corollary 1. Corresponding to every general proposition of logic, is another in which $\succ$ replaces $\prec$.

Definition of Copula of Identity.

If $x \prec y$ and $y \prec x$ then $x = y$ and conversely.

Definition of the Logical Sum.

(3) $x \dotplus y \prec y \dotplus x$.

(4) $x \prec x \dotplus y$.

(5) If $x \prec z$ and $y \prec z$, then $x \dotplus y \prec z$.

Definition of the Logical Internal Product.

(6) $x,y \prec y,x.$

(7) $x,y \prec x$

(8) If $z \prec x$ and $z \prec y$, then $z \prec x,y.$

Corollary 2 From (4), (7), (2) it follows that $x,y \prec x \mathbin{+\!\!,} y$ $(8\frac{1}{2})$

Corollary 3 From (1) and (5) it follows that $x \mathbin{+\!\!,} x \prec x$. So, $x \prec x,x.$

From (4) $x \prec x \mathbin{+\!\!,} x$ and from (7) $x,x \prec x.$

Theorem 1. (Boole) $y \prec x \mathbin{+\!\!,} y$ and $x,y \prec y.$

Proof. From (4) $y \prec y \mathbin{+\!\!,} x$ (9)

By (9) (3), (2) $y \prec x \mathbin{+\!\!,} y.$ (10)

The corollary above shows that this being true, we have also $x,y \prec y.$ (11)

Theorem 2. (Boole) *Logical addition and internal multiplication are associative operations.*

By (4)	$x \prec x \mathbin{+\!\!,} (y \mathbin{+\!\!,} z)$	(12)
By (4)	$y \prec y \mathbin{+\!\!,} z$	(13)
By (10)	$y \mathbin{+\!\!,} z \prec x \mathbin{+\!\!,} (y \mathbin{+\!\!,} z)$	(14)
By (13), (14), (2)	$y \prec x \mathbin{+\!\!,} (y \mathbin{+\!\!,} z)$	(15)
By (12), (15), (5)	$x \mathbin{+\!\!,} y \prec x \mathbin{+\!\!,} (y \mathbin{+\!\!,} z)$	(16)
By (10)	$z \prec y \mathbin{+\!\!,} z$	(17)
By (17) (14), (2)	$z \prec x \mathbin{+\!\!,} (y \mathbin{+\!\!,} z)$	(18)
By (16), (18), (5)	$(x \mathbin{+\!\!,} y) \mathbin{+\!\!,} z \prec x \mathbin{+\!\!,} (y \mathbin{+\!\!,} z)$	(19)

In the same way it is easily proved that

$$x \mathbin{+\!\!,} (y \mathbin{+\!\!,} z) \prec (x \mathbin{+\!\!,} y) \mathbin{+\!\!,} z.$$

The same reasoning applies to multiplication by Corollary 1.

Theorem 3. Logical Addition and Internal Multiplication are Mutually distributive; that is,

$$x,(y \mathbin{+\!\!,} z) = x,y \mathbin{+\!\!,} x,z. \tag{20}$$

$$x \mathbin{+\!\!,} y,z = (x \mathbin{+\!\!,} y),(x \mathbin{+\!\!,} z). \tag{21}$$

Proof. By 4	$y \prec y \mathbin{+\!\!,} z$	(22)
By (22), (11), (2)	$x,y \prec y \mathbin{+\!\!,} z$	(23)
By (23), (7), (8)	$x,y \prec x,(y \mathbin{+\!\!,} z)$	(24)

In the same way, we could show that

$$x,z \prec x,(y \mathbin{+\!\!,} z) \tag{25}$$

From (24), (25), (5)

$$x, y \mathbin{+\!\!,} x, z \prec x, (y \mathbin{+\!\!,} z). \tag{26}$$

Whence by Corollary 1,

$$x \mathbin{+\!\!,} y, z \prec (x \mathbin{+\!\!,} y), (x \mathbin{+\!\!,} z) \tag{27}$$

Theorem 4. (Boole.) If $x \prec y$, then $x \mathbin{+\!\!,} z \prec y \mathbin{+\!\!,} z$ and $x, z \prec y, z$.
Proof. By (4)

$$y \prec y \mathbin{+\!\!,} z \tag{29}$$

But

$$x \prec y \tag{30}$$

Hence, from (29), (30), (2)

$$x \prec y \mathbin{+\!\!,} z \tag{31}$$

By (10)

$$z \prec y \mathbin{+\!\!,} z \tag{32}$$

By (31), (32), (5)

$$x \mathbin{+\!\!,} z \prec y \mathbin{+\!\!,} z \tag{33}$$

Then, by corollary 1,

$$\text{If } y \prec x \qquad y, z \prec x, z \tag{34}$$

[. . .]

Definition of the negative.

$$x, n\text{-}x \prec 0 \tag{45}$$
$$1 \prec x \mathbin{+\!\!,} n\text{-}x \tag{46}$$

Theorem 6. If $x \prec y$, then $n\text{-}y \prec n\text{-}x$.
Proof. Assuming

$$x \prec y \tag{47}$$

By (34)

$$x, n\text{-}y \prec y, n\text{-}y \tag{48}$$

By (48), (45), (2)

$$x, n\text{-}y \prec 0 \tag{49}$$

By (33), (49), (41), (2)

$$x, n\text{-}y \mathbin{+\!\!,} n\text{-}x \prec n\text{-}x.$$

But by (46), (34), (42), (2)

$$n\text{-}y \prec x, n\text{-}y \mathbin{+\!\!,} n\text{-}x$$

[The Algebraic Principles (B)*]*

First Fundamental Definition. Copula of Inclusion.
First Clause.

(I.) If $x \prec y$ and $y \prec z$ then $x \prec z$.

Secondary Definition.

If $x \prec y$ and $y \prec x$ then $x = y$
If $x = y$ then $x \prec y$ and $y \prec x$.

Corollary 1. If $x \prec x$ then $x = x$.
Theorem 1. Corresponding to every general proposition of logic deducible from I without taking into account any other character of the copula, there is a proposition obtainable from the first by everywhere interchanging $\prec$ with $\succ$.

Proof. This follows from the fact that this substitution in (I), leaves the sense unaltered.

Second and third fundamental definitions. 1st: *Logical Addition.*

(II.) If $x \prec y$ then $x \prec y \mathbin{+\!\!,} z$ and $x \prec z \mathbin{+\!\!,} y$.
(III.) If $x \prec z$ and $y \prec z$, then $x \mathbin{+\!\!,} y \prec z$.

In order to render theorem 1 applicable, we require an operation which in such application is to be interchanged with logical addition. Its definition will be derived from that of addition by the same rule. Thus, we have,

2nd: *Definition of Logical Internal Multiplication.*

(II′.) If $y \prec x$ then $y, z \prec x$ and $z, y \prec x$.
(III′.) If $z \prec x$ and $z \prec y$ then $z \prec x, y$.

Fourth and fifth fundamental definitions. 1st: *Being.*

(IV.) $x \prec 1$.

To render theorem 1 applicable, we require a term which in such application shall be interchanged with 1. Its definition will be obtained from that of 1 by the same interchange. We have, accordingly,

2nd: *Definition of nothing.*

(IV′.) $0 \prec x$.

Corollary 2. $1 \prec 1 \quad 0 \prec 0$
Whence by II

$$1 \prec x \dotplus 1 \quad x,0 \prec 0$$

Corollary 3. Put $x \dotplus 1$ for x in (IV). This gives

$$x \dotplus 1 \prec 1$$

Then by theorem 1

$$0 \prec x,0.$$

First Fundamental definition. Copula of inclusion. Second Clause, being at the same time the definition of negation.

V. $y \prec x,y \dotplus n\text{-}x,y$ (Principle of excluded middle.)
V′. $(x \dotplus y),(n\text{-}x \dotplus y) \prec y$ (Principle of contradiction.)

Extension of Theorem 1. This theorem applies equally to propositions involving the second clause of the definition of the copula.
Theorem 2. *Logical addition and internal multiplication are commutative operations.*

Proof. In the definitions of $x \dotplus y$ and x,y there is no distinction between x and y.

Theorem 3. If $x \prec y$ and $u \prec v$
then $x \dotplus u \prec y \dotplus v$
$x,u \prec y,v$.

Proof. If $x \prec y$ then by (II) $x \prec y \dotplus v$
If $u \prec v$ then by (II) $u \prec y \dotplus v$.
Hence by III, if both $x \prec y$ and $u \prec v$ then

$$x \dotplus u \prec y \dotplus v$$

whence by theorem 1, if $y \prec x$ and $v \prec u$ then

$$y,v \prec x,u.$$

[The Algebraic Principles (C)]

First part of the definition of the Copula of Inclusion.

(I) If $x \prec y$ and $y \prec z$, then $x \prec z$.
Corollary 1. This does not distinguish between the relation expressed by the copula and the converse relation. It follows that corresponding to every general proposition of logic deducible from (I) without taking into account any other character of the copula, there is another proposition in which $\succ$ everywhere takes the place of $\prec$ in the first. These two propositions may in some cases be the same.

Definition of the Logical Sum.

(II.) $x \dotplus y \prec y \dotplus x$.
(III.) $x \prec x \dotplus y$.
(IV.) If $x \prec z$ and $y \prec z$, then $x \dotplus y \prec z$.

To apply corollary 1, we must have, corresponding to logical addition, another operation which may be called internal multiplication. In applying corollary 1, these operations are to be interchanged.

Definition of the Logical Internal Product.

(II'.) $y,x \prec x,y$.
(III'.) $x,y \prec x$.
(IV'.) If $z \prec x$ and $z \prec y$, then $z \prec x,y$.
Corollary 2. From III, III', I, it follows that

$$x,y \prec x \dotplus y \tag{1}$$

Corollary 3. From II and III, it follows that

$$x \prec y \dotplus x \tag{2}$$

So,

$$y,x \prec x \tag{2'}$$

Definition of being.

(V) $x \prec 1$
To apply corollary 1, corresponding to 1, we need another term to be interchanged with it, in such application. This is as follows.

Definition of Nothing.

(V′) $0 \prec x$.

Corollary 4. From V, it follows that

$$x \dagger 1 \prec 1$$

And by corollary 1,

$$0 \prec x, 0$$

By II and III

$$1 \prec x \dagger 1$$

and by corollary 1,

$$x, 0 \prec 0.$$

Corollary $4\frac{1}{2}$. From II and III it follows that if

$$x \dagger y \prec 0$$

then $\qquad x \prec 0$ and $y \prec 0$

whence by corollary 1,

if $\qquad 1 \prec x, y$

then $\qquad 1 \prec x$ and $1 \prec y$

Second part of the definition of the copula of inclusion.

VI. $\qquad x \prec x.$

Corollary 5. From III without VI or even I, it follows that

$$x \prec x \dagger x \qquad x, x \prec x.$$

By VI, and IV,

$$x \dagger x \prec x \qquad x \prec x, x.$$

Corollary 6. (Schröder) From III′, VI, and IV,

$$x \dagger x, y \prec x.$$

Then, by corollary 1,

$$x \prec x, (x \dagger y).$$

Corollary 7. Without VI or even I, we have from III,

$$x \prec x \dagger 0 \qquad x, 1 \prec x.$$

By VI, IV, and V′,

$$x \mathbin{+\!\!,} 0 \prec x \qquad x \prec x, 1.$$

Third part of the definition of the copula of inclusion, forming at the same time a definition of negation.

VII. $$y \prec x, y \mathbin{+\!\!,} n\text{-}x, y$$

This is the principle of excluded middle.

VII′. $$(x \mathbin{+\!\!,} y), (n\text{-}x \mathbin{+\!\!,} y) \prec y.$$

This is the principle of contradiction.

Corollary 8. In applying corollary 1, the negative remains unchanged.

Corollary 9. Put 1 for y in VII and we have

$$1 \prec x \mathbin{+\!\!,} n\text{-}x$$

and by corollary 1

$$x, n\text{-}x \prec 0$$

Hence also $$n\text{-}1 \prec 0 \text{ and } 1 \prec n\text{-}0$$

Theorem I. (Boole.) Logical addition and internal multiplication are associative operations.

From principles I, II, III, IV, we have

$$\begin{aligned}
& y \prec y \mathbin{+\!\!,} z \\
& y \mathbin{+\!\!,} z \prec x \mathbin{+\!\!,} (y \mathbin{+\!\!,} z) \\
\therefore\ & y \prec x \mathbin{+\!\!,} (y \mathbin{+\!\!,} z) \\
& x \prec x \mathbin{+\!\!,} (y \mathbin{+\!\!,} z) \\
\therefore\ & x \mathbin{+\!\!,} y \prec x \mathbin{+\!\!,} (y \mathbin{+\!\!,} z) \\
& z \prec x \mathbin{+\!\!,} (y \mathbin{+\!\!,} z) \\
\therefore\ & (x \mathbin{+\!\!,} y) \mathbin{+\!\!,} z \prec x \mathbin{+\!\!,} (y \mathbin{+\!\!,} z)
\end{aligned}$$

Similarly $$x \mathbin{+\!\!,} (y \mathbin{+\!\!,} z) \prec (x \mathbin{+\!\!,} y) \mathbin{+\!\!,} z.$$

The same applies to multiplication.

First part of Theorem 2. $x, y \mathbin{+\!\!,} x, z \prec x, (y \mathbin{+\!\!,} z)$ (Boole)

$x \mathbin{+\!\!,} y, z \prec (x \mathbin{+\!\!,} y), (x \mathbin{+\!\!,} z)$ (Peirce.)

Proof. By II′, III′, III, II, I, IV,

$$\begin{aligned}
& x, y \prec y \prec y \mathbin{+\!\!,} z \prec x, (y \mathbin{+\!\!,} z) \\
& x, z \prec z \prec y \mathbin{+\!\!,} z \prec x, (y \mathbin{+\!\!,} z) \\
\therefore\ & x, y \mathbin{+\!\!,} x, z \prec x, (y \mathbin{+\!\!,} z)
\end{aligned}$$

By corollary 1, $x \mathbin{+\!\!,} y, z \prec (x \mathbin{+\!\!,} y), (x \mathbin{+\!\!,} z)$.
The converse of this theorem cannot be proved without the aid of the third part of the definition of the copula.
Second part of theorem 2. $x, (y \mathbin{+\!\!,} z) \prec x, y \mathbin{+\!\!,} x, z$. (Boole.)
$(x \mathbin{+\!\!,} y), (x \mathbin{+\!\!,} z) \prec x \mathbin{+\!\!,} y, z$. (Peirce.)

Proof. By VII,

$$x, (y \mathbin{+\!\!,} z) \prec y, x, (y \mathbin{+\!\!,} z) \mathbin{+\!\!,} n\text{-}y, x, (y \mathbin{+\!\!,} z)$$
$$n\text{-}y, x, (y \mathbin{+\!\!,} z) \prec n\text{-}y, z, x, (y \mathbin{+\!\!,} z) \mathbin{+\!\!,} n\text{-}y, n\text{-}z, x, (y \mathbin{+\!\!,} z).$$

Whence by theorem 3, and I

$$x, (y \mathbin{+\!\!,} z) \prec y, x, (y \mathbin{+\!\!,} z) \mathbin{+\!\!,} n\text{-}y, z, x, (y \mathbin{+\!\!,} z) \mathbin{+\!\!,} n\text{-}y, n\text{-}z, x, (y \mathbin{+\!\!,} z).$$

By III and II,

$$y, x, (y \mathbin{+\!\!,} z) \prec x, y \prec x, y \mathbin{+\!\!,} x, z$$
$$n\text{-}y, z, x, (y \mathbin{+\!\!,} z) \prec x, z \prec x, y \mathbin{+\!\!,} x, z$$

Hence by IV

$$y, x, (y \mathbin{+\!\!,} z) + n\text{-}y, z, x, (y \mathbin{+\!\!,} z) \prec x, y \mathbin{+\!\!,} x, z.$$

Hence, by I,

$$x, (y \mathbin{+\!\!,} z) \prec x, y \mathbin{+\!\!,} x, z \mathbin{+\!\!,} n\text{-}y, n\text{-}z, x, (y \mathbin{+\!\!,} z)$$

But by III

$$n\text{-}y \prec n\text{-}y \mathbin{+\!\!,} z$$

Hence, by theorem 3 and VII′

$$n\text{-}y, n\text{-}z, x, (y \mathbin{+\!\!,} z) \prec n\text{-}z, x, (n\text{-}y \mathbin{+\!\!,} z), (y \mathbin{+\!\!,} z) \prec n\text{-}z, z \prec 0$$

Hence,

$$x, (y \mathbin{+\!\!,} z) \prec x, y \mathbin{+\!\!,} x, z.$$

By corollary 1,

$$(x \mathbin{+\!\!,} y), (x \mathbin{+\!\!,} z) \prec x \mathbin{+\!\!,} y, z.$$

Theorem 3. (Boole.) If $x \prec y$ then $x \mathbin{+\!\!,} z \prec y \mathbin{+\!\!,} z$; also, $x, z \prec y, z$.
Proof. By III and I,

$$x \prec y \prec y \mathbin{+\!\!,} z.$$

By II and III,

$$z \prec z \mathbin{+\!\!,} y \prec y \mathbin{+\!\!,} z.$$

Hence, by IV,

$$x \mathbin{+\!\!,} z \prec y \mathbin{+\!\!,} z.$$

Whence, by corollary 1,

$$\text{If } y \prec x \quad y,z \prec x,z.$$

Theorem IV. If $x \prec y$ then $n\text{-}y \prec n\text{-}x$.

From $x \prec y$, by theorem 3, corollary 9, and I

$$x, n\text{-}y \prec y, n\text{-}y \prec 0$$

Hence, by VII, theorem 3, corollary 7, III′, and I,

$$n\text{-}y \prec x, n\text{-}y \mathbin{+\!\!,} n\text{-}x, n\text{-}y \prec 0 \mathbin{+\!\!,} n\text{-}x, n\text{-}y \prec n\text{-}x, n\text{-}y \prec n\text{-}x.$$

Corollary 10. Put $x \mathbin{+\!\!,} z$ for y in theorem IV and we have

$$n\text{-}(x \mathbin{+\!\!,} z) \prec n\text{-}x$$

So $$n\text{-}(x \mathbin{+\!\!,} z) \prec n\text{-}z$$

Whence by IV′ $$n\text{-}(x \mathbin{+\!\!,} z) \prec n\text{-}x, n\text{-}z$$

And by corollary 1

$$n\text{-}x \mathbin{+\!\!,} n\text{-}z \prec n\text{-}(x,z)$$

Otherwise, put y,z for x in theorem IV and we have

$$n\text{-}y \prec n\text{-}(y,z)$$

So $$n\text{-}z \prec n\text{-}(y,z)$$

Whence by IV

$$n\text{-}y \mathbin{+\!\!,} n\text{-}z \prec n\text{-}(y,z)$$

and by corollary 1

$$n\text{-}(y \mathbin{+\!\!,} z) \prec n\text{-}y, n\text{-}z$$

Theorem V. If $x \prec n\text{-}y$ then $y \prec n\text{-}x$.

Proof. From $x \prec n\text{-}y$, we have by theorem 3, and corollary 9

$$x, y \prec n\text{-}y, y \prec 0$$

Hence by VII, corollary 7, and III′

$$y \prec x, y \mathbin{+\!\!,} n\text{-}x, y \prec 0 \mathbin{+\!\!,} n\text{-}x, y \prec n\text{-}x, y \prec n\text{-}x$$

Whence by corollary 1,

$$\text{If } n\text{-}y \prec x \text{ then } n\text{-}x \prec y.$$

Scholium. Theorem IV is the principle of the syllogism in *Camestres*

All negroes are men,
No monkey is a man;
∴ No monkey is a negro.

Theorem V is the principle of *Cesare*

No monkey is a man,
All negroes are men;
∴ No negro is a monkey.

Accordingly, *Camestres* holds when instead of "other than," or the negative, we put any other relative term, as "lover of." Thus,

All negroes are men,

$$\text{God is } \left\{\begin{matrix}\text{other than}\\ \text{a lover of}\end{matrix}\right\} \text{every man;}$$

$$\therefore \text{ God is } \left\{\begin{matrix}\text{other than}\\ \text{a lover of}\end{matrix}\right\} \text{every negro.}$$

But *Cesare* only holds provided the relative is an equiparant. Thus,

$$\text{All monkeys } \left\{\begin{matrix}\text{are other than}\\ \text{resemble}\end{matrix}\right\} \text{every man,}$$

All negroes are men;

$$\therefore \text{ All negroes } \left\{\begin{matrix}\text{are other than}\\ \text{resemble}\end{matrix}\right\} \text{every monkey.}$$

Theorem VI. (Schröder) If

$$x,z \prec y,z \text{ and } x \dagger z \prec y \dagger z$$

then

$$x \prec y$$

Proof. By III and I

$$x \prec x \dagger z \prec y \dagger z$$

By theorem 3, corollary 5, theorem 2, III and I

$$x \prec x, x \prec x, (y \dagger z) \prec x, y \dagger x, z \prec x, y \dagger y, z \prec y, (x \dagger z) \prec y.$$

Theorem VII. (Schröder's proof.) $n\text{-}n\text{-}x = x.$

Since, $x, n\text{-}x \prec 0$ and $1 \prec x \dagger n\text{-}x$

We have, by substituting $n\text{-}x$ for x,

$$n\text{-}x, n\text{-}n\text{-}x \prec 0 \text{ and } 1 \prec n\text{-}x \dotplus n\text{-}n\text{-}x$$

Whence

$$x, n\text{-}x \prec n\text{-}n\text{-}x, n\text{-}x \text{ and } x \dotplus n\text{-}x \prec n\text{-}n\text{-}x \dotplus n\text{-}x$$

also

$$n\text{-}n\text{-}x, n\text{-}x \prec x, n\text{-}x \text{ and } n\text{-}n\text{-}x \dotplus n\text{-}x \prec x \dotplus n\text{-}x$$

Whence by theorem VI

$$x \prec n\text{-}n\text{-}x \qquad n\text{-}n\text{-}x \prec x.$$

Corollary 11. By corollary 10, and theorem VII

$$n\text{-}x, n\text{-}y \prec n\text{-}(x \dotplus y)$$
$$n\text{-}(x, y) \prec n\text{-}x \dotplus n\text{-}y.$$

Logic. Chapter I. Of Thinking as Cerebration

MS 350: Fall–Winter 1879

Thinking is done with the brain, and the brain is a complexus of nerves; so that thinking is necessarily subject to the general laws of nervous action.

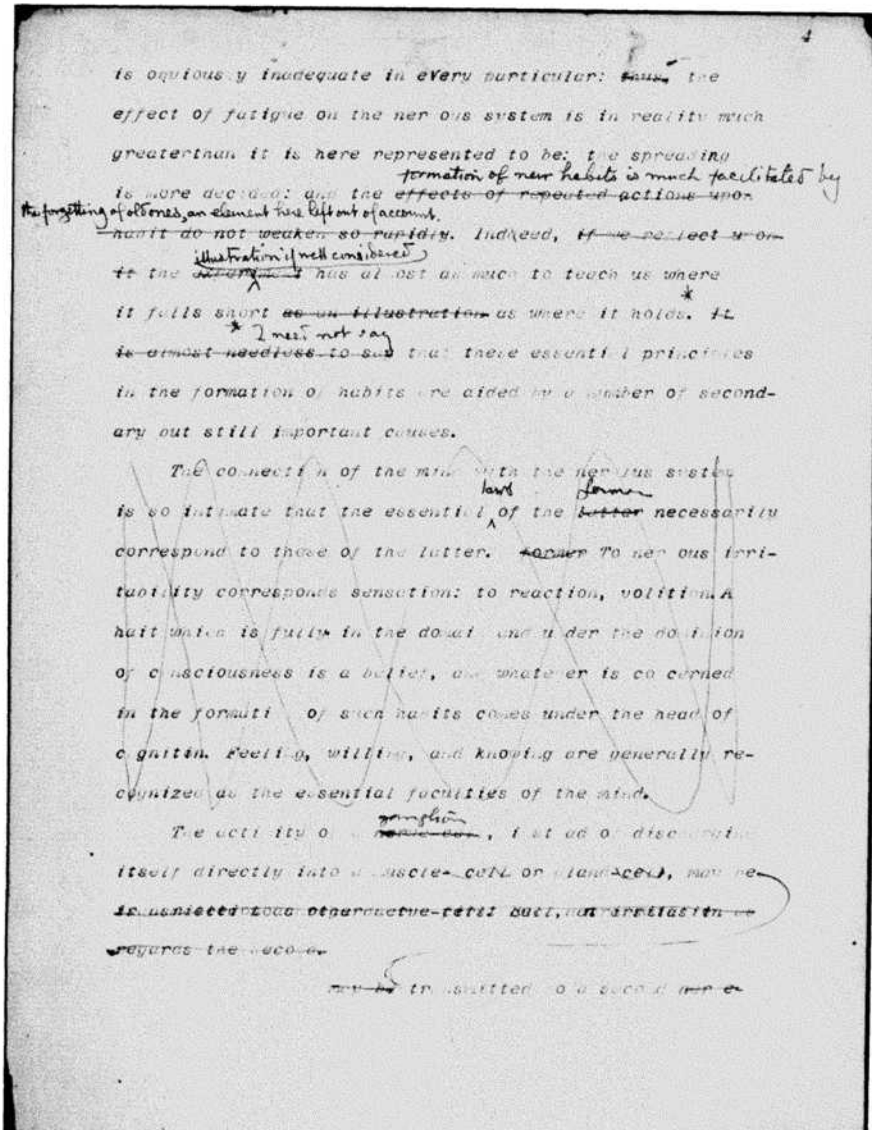
The nervous system consists of nerve-cells connected together by nerve-fibres in an intricate way, and connected by other nerve-fibres with muscle-cells and gland-cells. Thus, there are three kinds of anatomical elements in the nervous system: the cells, the interconnecting fibres, and the efferent fibres. The only things the nerves directly do are to cause the muscles to contract and the glands to secrete; but the usefulness of them depends upon the extraordinary laws to which their action is subject. What is usually seen is that a muscular contraction or glandular secretion takes place on the application of some external force to some nerve-cell often far from the muscle or gland affected: at the same time the energy of the action is only partially dependent upon and commonly exceeds that of the stimulus, which may even occasionally be entirely wanting. The contents of the cells seem to be in a state of chemical or other unstable equilibrium, so that upon very slight provocation, change takes place, accompanied by a transformation of certain energy resident in the cell to a form in which it can travel along the nerve-fibres; but a single discharge of this sort seems to draw off but a very small proportion of the energy of the cell, and the operation may be repeated many times at rapid intervals, before exhaustion ensues. Whatever may be the truth of the chemical hypothesis, it is observed that the nerves are highly irritable, being set into violent action by slight stimuli, and that after long action they show signs of fatigue. Besides fatigue, long continued stimulation causes another phenomenon, namely, the spread from cell to cell of the nervous activity, with an increase of total energy. Under the combined influence of this spreading and the fatigue of cells which have already acted, the reaction, unless it be of a sort to immediately remove the source of irritation, will vary its character again and again, until some action is produced that accidentally removes the stimulus, when the activity immediately subsides. According to the universal law of habit, all vital processes whatever tend to become easier on repetition. (J. J. Murphy. *Habit and Intelligence.* 1869. Chap. XV. Vol. I. p. 169.) The nervous system is peculiarly susceptible to habit, and along whatever path a nervous discharge has once taken place, along that path a new discharge is the more likely to take place. It is an essential characteristic of this law that it is not absolute. The reactions from a given stimulus will not generally be exactly the same as on the last occasion when a similar irritation was experienced, but the particular reactions that did then occur will on that account be more likely to occur again than they otherwise would be.

By the operation of these laws, the actions of an animal come to be directed towards an end: that end being the removal of irritations. For on the one hand, during the continuance of an irritation, owing to fatigue and spreading, an unsuccessful reaction is not likely to recur; while on the other hand, on different occasions, the successful reactions are likely to recur. The following experiment will illustrate, though very imperfectly, the mode of formation of habits. Out of a pack of 52 cards, the suits of which have been separated, we take one card from each suit to begin with: these are to be shuffled up together and held face down. This pack of cards, consisting at first of only four, is to represent the habit of the nervous system, and we are to give it a determination toward the suit of hearts. The reaction from an imaginary stimulus shall be represented by turning up a card at random from this pack; and the irritation is supposed to continue, with successive reactions, until a heart is turned up, when it ceases. During the continuance of any one irritation, the fatigue shall be represented by not returning to the pack the cards that have been turned. In order to represent the immediate effect of the action upon the state of habit, after the heart has been turned we will return to the pack not merely the cards that have been drawn, but also with them as many more of the same suits as they, so that there shall be an increased tendency to do what has been done. After a dozen irritations, the thirteen hearts will have been taken, and it will be found that the total number in the pack seldom amounts to thirty, so that the ratio of hearts has been greatly increased. This makes it clear that nothing but the elements mentioned, to wit, the spreading of irritation and the fatigue, are requisite in order to direct acquired habits to the removal of irritations. Yet the illustration is obviously inadequate in every particular: the effect of fatigue on the nervous system is in reality much greater than it is here represented to be, the spreading is more decided, and the formation of new habits is much facilitated by the forgetting of old ones, an element here left out of account. Indeed, the illustration, if well considered, has almost as much to teach us where it falls short as where it holds.

The activity of a ganglion, instead of discharging itself directly into a muscle or gland, may be transmitted to a second, perhaps more central, ganglion. Such a transmission is at once a reaction as regards the first ganglion and an irritation as regards the second. Of all habits, none is more important than that universal one by virtue of which every irritation that is unfamiliar is so transmitted to a higher centre. In this way, certain kinds of stimuli come to react at first upon the brain.

We meet no sure indications of a consciousness unconnected with a nervous organism; and the more complicated the organism, the higher is the consciousness. Whether the soul exists as an independent substance or not, certain it is that intelligence, as we know it, resides in the nervous system; so that the laws of the former necessarily correspond to those of the latter. To trace the correspondence throughout, with scientific accuracy, would not at present be possible; but such rough sketch as we can make of it, though not free from error, will not fail to shed a strong light upon the theory of logic.

Wherever there is a feeling, there a nerve-cell is in action. Undoubtedly, nerve-cells may be in action, without sensation; but we need not concern ourselves with distinctions of that sort; we seek only to assign the genera, and not the species, of physiological phenomena to which psychological phenomena correspond. Feeling corresponds to nerve-cell activity; sensibility in psychology, to nervous irritability in physiology. External sensations come from the terminal cells of peripheral nerves; internal sensation from the termini of visceral nerves; secondary sensations, the sense of beauty, the perception of a tune, the sense of effort, moral conscience, and the like, reside in central nerve-cells. Muscular reaction corresponds to volition outwardly directed. We know that there are reactions that


is obviously inadequate in every particular: ~~thus~~ the effect of fatigue on the nervous system is in reality much greater than it is here represented to be; the spreading is more decided; and the ~~effects of repeated actions upon habit do not weaken so rapidly~~ formation of new habits is much facilitated by the forgetting of old ones, an element here left out of account. Indeed, ~~if we reflect upon it~~ the ~~[illegible]~~ illustration if well considered has almost as much to teach us where it falls short ~~as an illustration~~ as where it holds. ~~It is almost needless to say~~ I need not say that these essential principles in the formation of habits are aided by a number of secondary but still important causes.

The connection of the mind with the nervous system is so intimate that the essentials of the ~~latter~~ former necessarily correspond to those of the latter. ~~Former~~ To nervous irritability corresponds sensation; to reaction, volition. A habit which is fully in the domain and under the dominion of consciousness is a belief, and whatever is concerned in the formation of such habits comes under the head of cognition. Feeling, willing, and knowing are generally recognized as the essential faculties of the mind.

The activity of [illegible] ganglion, [illegible] itself directly into a muscle-cell or gland-cell, may [illegible] [illegible] transmitted to a second [illegible]

Page from typescript in MS 349, draft of MS 350; revisions in Peirce's hand (Harvard Peirce Papers MS 748) are not voluntary; but as just observed, such distinctions are of no particular interest for logic, and will not be further alluded to. It concerns us more to notice that there are volitions directed inwardly, and accompanied by no muscular reaction; but there can be no doubt that these are nevertheless connected with reactions, with reactions upon the brain itself.

The psychological correlative of cerebration, or brain activity, is the active operation of the mind, or thinking. To the physiological fact that cerebration consists in the activity of nerve-cells, corresponds the psychological fact that thinking is as to its matter, a mere flow of sensations. To the physiological fact that cerebration is a species of reaction of one centre upon another, corresponds the psychological fact that thinking is an act of inward volition of the kind called attention. The irritation of the brain, in another aspect, appears as the uneasy sense of doubt; and the end of all thinking is the removal of doubt, just as the end of all nervous action is the removal of the source of irritation.

The action of the brain has, at last, its reaction upon the muscles, without which it would be absolutely useless. But so plastic, so to say, are the avenues of nervous discharge in the higher centres, that a single performance of a brain-action suffices for the full formation of a habit. There are, however, certain cases of brain-action, namely those in which the thinking relates to something imagined, where there is no immediate muscular reaction. The operation of the imagination, which is most important in all but the lowest kind of thinking, is somewhat difficult to explain physiologically. It used to be said that the recollection of a visible object differed from the sensation in being *fainter.* But surely there is no sensation of colour or other primary quality in any product of the imagination. In dreams and in some waking consciousness, the peripheral nerves seem to be spontaneously excited; but in any sane intellect, such phenomena play a most trivial part, very different indeed from that of ordinary imagination. It is, indeed, true that the energy of the nerve-action is usually relatively feeble in imagining. But this is hardly so characteristic of the operation as are two other properties of it, viz., 1st, that the activity of a central nerve-cell which in perceiving is excited by a stimulus from the periphery, is in imagining, excited by a stimulus from another central cell; and 2nd, that the process of imagination is accompanied throughout by an inhibitory volition, which not only diminishes the energy of action of the nerve-cells, but also altogether prevents reaction upon the voluntary muscles.

In all speculative and prudential thinking, the doubt or irritation of the brain comes from the imagination. The brain and mind proceed to act very nearly as if there had been a real perception similar to what has been imagined; only they act more deliberately and less violently. Although no muscular reaction is seen, yet a kind of inward reaction seems to take place, accompanied by the imagination that we would act in a certain way in the imagined circumstances. Meantime, the brain having certainly acted, a habit is formed which will give rise to outward reaction on another occasion, when there is no inhibition. Such a habit, accompanied by an imagination which brings it into the sphere of consciousness, is a belief, and the inward act that we seem to perform when we first acquire it is called a judgment.

It is to be observed that besides the more immediate cerebral reactions, various outward actions are performed in the course of that struggle which is set in motion by doubt,—these actions being directed toward the fixation of a belief-habit.

THE SETTLEMENT OF OPINION.

Seeing that the sole object to which all our efforts tend in that struggle called inquiry or research is the fixation of a habit of belief, why should we not attain the desired end, by taking any answer to a question that we may fancy, and constantly reiterating it to ourselves, dwelling on all that may conduce to that belief, and learning to turn with contempt and hatred from anything that might disturb it? This simple and direct method is really pursued by many men. I remember being once entreated not to read a certain newspaper lest it might change my opinion about free-trade. "Lest I might be entrapped by its fallacies and mis-statements," was the form of expression. "You are not," my friend said, "a special student of political economy. You might, therefore, easily be deceived by fallacious arguments on the subject. You might, then, if you read this paper, be led to believe in protection. But you admit that free-trade is the correct doctrine; and you do not wish to believe what is not true." I have often known this system to be deliberately adopted. Still oftener, the instinctive dislike of an undecided state of mind, exaggerated into a vague dread of doubt, makes men cling spasmodically to the views they already take. The man feels that, if he only holds his belief without wavering, it will be entirely satisfactory. Nor can it be denied that a steady and immovable faith yields great peace of mind. It may, indeed, give rise to inconveniences, as if a man should resolutely continue to believe that fire would not burn him, or that he would be eternally damned if he received his ingesta otherwise than through a stomach-pump. But then the man who adopts this method will not allow that its inconveniences are greater than its advantages. He will say, "I hold steadfastly to the truth, and the truth is always wholesome." And in many cases it may very well be that the pleasure he derives from his calm faith overbalances any inconveniences resulting from its deceptive character. Thus, if it be true that death is annihilation, then the man who believes he will certainly go straight to heaven when he dies, provided he have fulfilled certain simple observances in this life, has a cheap pleasure which will not be followed by the least disappointment. A similar consideration seems to have weight with many persons in religious topics, for we frequently hear it said, "Oh, I could not believe so and so because I should be wretched if I did." When an ostrich buries its head in the sand as danger approaches, it very likely takes the happiest course. It hides the danger, and then calmly says there is no danger; and, if it feels perfectly sure there is none, why should it raise its head to see? A man may go through life, systematically keeping out of view all that might cause a change in his opinions, and if he only succeeds, —basing his method as he does, on two fundamental psychological laws—I do not see what can be said against his doing so. It would be an egotistical impertinence to object that his procedure is *[. . .]*

Logic. Chapter I. Thinking as Cerebration

MS 354: Winter–Spring 1880

Can mind be defined in terms of mechanics? If the mind will do something that a machine cannot do, since mechanics can describe in general terms what machines are capable of doing, it should be able to describe what effects mental action produces that machines cannot produce. A man is sitting in church listening to the sermon, in complete quiescence: the thunders of the pulpit affect his action not the least: but somebody comes behind him and whispers some faint sounds, "Your house is on fire." The man is instantly roused to energetic muscular work. There is, in the first place, no relation between the mechanical energy of the whisper and that of the muscular contractions; but this is often the case in pure mechanical action. Thus, a slender wooden cylinder is sometimes set up on end to detect earthquake-shocks; but, if it falls, it falls with the same force, whether the overturning shock was less or more violent. So a piece of phosphorus burns with the same energy whether the friction that set it on fire were less or greater, so long as it was sufficient. What really distinguishes intelligent action is that it is directed towards ends (as all vital action is) and varies as the ends vary, with a facility that does not belong to other vital processes. There is, perhaps, nothing which absolutely distinguishes the action of the nerves from that of other tissues; and intelligent action seems to be nothing but nervous action of a high grade. Still, it must be remembered that these matters have not yet been completely elucidated by science.

Thinking is, however, a species of cerebration, and cerebration is a species of nervous action: and here as almost everywhere there will be an advantage in beginning our study by a comparison of the object in hand with the more typical cases of the genus to which it belongs.

The physiologists tell us that the rudimentary type of a nervous system would be a single ganglionic cell with an afferent and an efferent nerve. I am not a physiologist and am bound to accept the dictum of those who are, although it would seem to me that complexity is almost an essential feature of a nervous organism. At any rate, although in this simplest type we recognize germs of sensation and of volition, we find not the faintest beginning of anything analogous to intellect. In the higher animals, however, multitudes of ganglionic cells are bound together in the most intricate manner. An irritation which is at first followed by immediate peripheral reaction will if the stimulation is continued spread from ganglion to ganglion, and thus produce new reactions. Soon, too, the parts first excited begin to show fatigue; and thus for a double reason the bodily activity is of a changing kind. Accordingly, when a nerve is stimulated, if the immediate reflex action does not happen to remove the source of irritation, it will change its character again and again, and ultimately will very likely remove the irritation, when the action will cease.

Now comes in another element. All vital processes become easier on repetition. Along whatever path a nervous discharge has taken place, along that path a new discharge is the more likely to take place. Habit plays somewhat the same part in the history of the individual that natural selection does in that of the species; namely, it causes actions to be directed towards ends. When an irritation of the nerves is repeated, all the various actions which have taken place on previous similar occasions are the more likely to take place now, and those are the most likely to take place which have most frequently taken place on those previous occasions. Now, the various actions which did not remove the irritation may have previously sometimes been performed and sometimes not; but the action which removes the irritation must have always been performed, because the action had every time continued until it was performed. Thus, habits come to be directed towards an end that of removing irritations. The transmission of habits by inheritance of course greatly aids this process.

Rood's Chromatics[1]

P 149: Nation *29 (16 October 1879): 260*

The utility and significance of visual perceptions distract attention from the mere sensuous delight of color and light; yet few elementary pleasures are so insatiable. The spectrum, however often it may be seen, never ceases to afford the same sense of joy. The prices paid for luminous and colored stones, though exaggerated by fashion, could only be maintained on the solid foundation of a universal pleasure in color and light, together with a sense of similitude between this feeling and those which the contemplation of beauty, youth, and vigor produces. This pleasure makes one of the fascinations of the scientific study of color. Besides this, the curious three-fold character of color which assimilates it to tri-dimensional space, invites the mathematician to the exercise of his powers. And then there is the psychological phenomenon of a multitude of sensations as unaltered by the operation of the intellect, and as near to the first impression of sense, as any perception which it is in our power to extricate from the complexus of consciousness—these sensations given, too, in endless variety, and yet their whole diversity resulting only from a triple variation of quantity of such a sort that all of them are brought into intelligible relationship with each other, although it is perfectly certain that quantity and relation cannot be objects of sensation, but are conceptions of the understanding. So that the question presses, What is there, then, in color which is not relative, what difference which is indescribable, and in what way does the pure sense-element enter into its composition?

In view of these different kinds of interest which the scientific

1. *Modern Chromatics, with Applications to Art and Industry.* By Ogden N. Rood, Professor of Physics in Columbia College. With 130 original illustrations. New York: D. Appleton & Co. 1879.

study of color possesses, it is not surprising that the pursuit is one which has engaged some of the finest minds which modern physics can boast. The science was founded partly by Newton and partly by Young. It has been pursued in our day by Helmholtz and by Maxwell; and now Professor Rood produces a work so laden with untiring and skilful observation, and so clear and easy to read, that it is plainly destined to remain the classical account of the color-sense for many years to come. Chromatics is to be distinguished from several other sciences which touch the same ground. It is not chemistry, nor the art of treating pigments, nor optics (which deals with light as an undulation, or, at least, as an external reality); nor is it a branch of physiology, which might study the various ways of exciting the sensation of color, as by direct sensation, contrast, fatigue, hallucination, etc.; nor is it the account of the development of the color-sense. The problems of chromatics are two: First, to define the relations of the appearances of light to one another; and second, to define their relations to the light which produces them. It is, therefore, a classificatory, not a cause-seeking science. The first series of relations according to which it classifies colors are those of the appearances in themselves. Here we have grey ranging in value from the darkest shade to the white of a cloud. The shades may be conceived as arranged along an axis about which we have circles of color—yellow, red, blue, and green, with their infinite intermediate gradations. Each of these varies in value, and also in its color-intensity, from neutrality at the centre to the most glaring hues at the circumference.

The second series of relations which the science of chromatics considers are those which subsist between the appearance of a mixture of lights and the appearances of its constituents. By a mixture of lights is not meant a mixture of pigments, but the effect of projecting two colors—say, for instance, by two magic-lanterns—upon the same spot. It has been found that for this kind of mixture (although not for the mixture of pigments) the appearance of the mixture is completely determined by the appearances of the constituents, whatever may be the physical constitution of the light of the latter. The effect of mixing two lights is, roughly speaking, similar to that of adding together the sensations produced by the two lights separately. Let, for example, two precisely similar lights be projected on the same spot, and the result will be brighter than either, and in hue and color-intensity nearly like them. If white and blue be thrown together, the result will be a brighter and more whitish blue. Red and blue thrown together will give purple, blue and green will give blue-green, yellow and red will give orange, etc. Unfortunately for the perspicuity of the subject, this approximate equivalence between mixing light and adding together sensations is not precise, nor even very close. On the contrary, the mixture is always less bright and nearer to a certain yellow than the sum of the sensations of the constituents. This yellow, the precise color of which is defined, is one in comparison with which the purest yellow that can be isolated appears whitish. It has been called the *color of brightness.* The most striking example of this effect is afforded by a mixture of red and green, which gives a strong yellow effect, although the sum of the two sensations is nearly white.

The study of mixtures has thus given rise to a system of classifying colors which coincides just nearly enough with that derived from the appearances themselves to be generally confused with it, while it differs from it enough to make such a confusion utterly destructive of clear conceptions of the relationships of color. One of the highest merits of the work of Professor Rood is the avoidance of this confusion; and if, for instance, no distinction is made between complementary colors in the sense of those which, when mixed, give white, and in the sense of those whose sensations sum up to white, it is doubtless because here, as elsewhere in the book, logic and scientific precision have more or less suffered from a determination not to repel indolent minds.

On the Ghosts in Rutherfurd's Diffraction-Spectra

P 134: American Journal of Mathematics
2 (1879): 330–47

Let there be a periodical irregularity in the ruling of a diffraction-plate, so that the side of the r^{th} slit nearest a fixed line of reference parallel to the ruling shall be distant from that line by

$$\left(r-\frac{1}{2}\alpha\right)\mathrm{w}+\mathrm{e}\sin\left(r\theta-\frac{1}{2}\theta\right)$$

while the side of the same opening furthest from the line of reference is distant from it by

$$\left(r+\frac{1}{2}\alpha\right)\mathrm{w}+\mathrm{e}\sin\left(r\theta+\frac{1}{2}\theta\right).$$

This is supposing the opaque lines to have a constant breadth, $(1-\alpha)\,\mathrm{w}$.

Suppose the collimator and telescope of the spectrometer to be focused for parallel rays, and neglect the angular aperture of the slit. Let the angle of incidence be i, and the angle of emergence j. Write

$$\nu=\sin i-\sin j.$$

Then the ray which strikes the gitter at a distance x from the line of reference is longer than that which passes through the line of reference by νx. Consequently, the resultant oscillation from the r^{th} slit will be

$$\int_{(r-\frac{1}{2}\alpha)\mathrm{w}+\mathrm{e}\sin(r\theta-\frac{1}{2}\theta)}^{(r+\frac{1}{2}\alpha)\mathrm{w}+\mathrm{e}\sin(r\theta+\frac{1}{2}\theta)} \mathrm{d}x\cdot\sin 2\frac{Vt-\nu x}{\lambda}\pi$$

where t is the time, V the velocity of light, and λ the wave-length. (In this paper π will be written for the ratio of the circumference to the diameter, e for the natural base, and ι for the imaginary unit.) If then we sum this for all integral values of r, we obtain an expression for the resultant oscillation from the whole gitter.

Performing the integration relatively to x, indicating the summation relative to r, and using the abbreviations

$$\omega = 2\frac{\text{w}\nu}{\lambda}\pi, \qquad \epsilon\omega = 2\frac{\text{e}\nu}{\lambda}\pi, \qquad \tau = 2\frac{Vt}{\lambda}\pi,$$

we obtain the following expression for the resultant oscillation from the whole grating:

$$\begin{aligned}\frac{\text{w}}{\omega}\Sigma_r\Big\{&\cos\left[\epsilon\omega\sin\left(r\theta+\frac{1}{2}\theta\right)\right]\cdot\cos\left(\tau-\frac{1}{2}\alpha\omega-r\omega\right)\\&+\sin\left[\epsilon\omega\sin\left(r\theta+\frac{1}{2}\theta\right)\right]\cdot\sin\left(\tau-\frac{1}{2}\alpha\omega-r\omega\right)\\&-\cos\left[\epsilon\omega\sin\left(r\theta-\frac{1}{2}\theta\right)\right]\cdot\cos\left(\tau+\frac{1}{2}\alpha\omega-r\omega\right)\\&-\sin\left[\epsilon\omega\sin\left(r\theta-\frac{1}{2}\theta\right)\right]\cdot\sin\left(\tau+\frac{1}{2}\alpha\omega-r\omega\right)\Big\}.\end{aligned}$$

We now need a formula for developing sines and cosines of sines. For this purpose take $y = e^{\iota x}$. Then we have

$$\cos(\alpha\sin x) + \sin(\alpha\sin x)\cdot\iota = e^{\iota\cdot\alpha\sin x} = e^{\frac{1}{2}\alpha(y-\frac{1}{y})}.$$

By the usual development of an exponential function, this is

$$e^{\frac{1}{2}\alpha(y-\frac{1}{y})} = \sum_0^\infty{}_p\frac{\alpha^p}{p!2^p}\left(y-\frac{1}{y}\right)^p,$$

and by the binomial theorem, this is,

$$e^{\frac{1}{2}\alpha(y-\frac{1}{y})} = \sum_0^\infty{}_p\frac{\alpha^p}{p!2^p}\sum_0^p{}_q(-1)^q\frac{p!}{q!(p-q)!}y^{p-2q}.$$

The pq^{th} term is

$$(-1)^q\frac{\alpha^p y^{p-2q}}{2^p q!(p-q)!}.$$

Put $m = p - 2q$ and this becomes

$$(-1)^q \frac{\alpha^m y^m}{2^m} \cdot \frac{\alpha^{2q}}{4^q q!(m+q)!}.$$

In regard to the limits of the summation, q may have any value from zero to positive infinity, and, for every value of q, p may have any value from q to positive infinity; hence, m may have any value from $-q$ to positive infinity, and we have

$$\cos(\alpha \sin x) + \sin(\alpha \sin x) \cdot \iota$$
$$= \sum_0^{\infty}{}_q (-1)^q \frac{\alpha^{2q}}{4^q \cdot q!} \sum_{-q}^{\infty}{}_m \frac{\alpha^m}{2^m \cdot (m+q)!} (\cos mx + \sin mx \cdot \iota).$$

If m has a positive value, q may have any positive value; but if m has a negative value, q can only have any positive value greater than $-m$. Hence, we may take the terms for which m is not zero in pairs, embracing in each pair a term for which m has a positive value, M, and q has a value, Q, and also a term for which $m = -M$ and $q = M + Q$. The sum of two terms composing the pair is, then,

$$(-1)^Q \frac{\alpha^M(\cos Mx + \sin Mx \cdot \iota)}{2^M} \cdot \frac{\alpha^{2Q}}{4^Q Q!(M+Q)!}$$

$$+ (-1)^{M+Q} \frac{\alpha^{-M}(\cos Mx - \sin Mx \cdot \iota)}{2^{-M}} \cdot \frac{\alpha^{2M+2Q}}{4^{M+Q}(M+Q)!Q!}.$$

If M is even, the value of this is

$$(-1)^Q \frac{\alpha^M}{2^{M-1}} \cdot \frac{\alpha^{2Q}}{4^Q Q!(M+Q)!} \cos Mx;$$

and if M is odd, its value is

$$(-1)^Q \frac{\alpha^M}{2^{M-1}} \cdot \frac{\alpha^{2Q}}{4^Q Q!(M+Q)!} \sin Mx \cdot \iota.$$

We have then

$$\cos(\alpha \sin x) + \sin(\alpha \sin x) \cdot \iota$$
$$= \sum_0^{\infty}{}_q (-1)^q \frac{\alpha^{2q}}{4^q (q!)^2} + \sum_1^{\infty}{}_m \frac{A_m \alpha^m}{m! 2^{m-1}} (\cos x + \sin x \cdot \iota)^m;$$

where

$$A_m = \sum_0^{\infty}{}_q (-1)^q \frac{m!}{4^q q!(m+q)!} \alpha^{2q}.$$

Performing the numerical calculations, we have

$$\cos(\alpha \sin x) =$$

$$\left(1 - \frac{1}{4}\alpha^2 + \frac{1}{64}\alpha^4 - \frac{1}{2304}\alpha^6 + \frac{1}{147456}\alpha^8 - \frac{1}{14745600}\alpha^{10} + \text{etc.}\right)$$

$$+ \frac{1}{4}\alpha^2\left(1 - \frac{1}{12}\alpha^2 + \frac{1}{384}\alpha^4 - \frac{1}{23040}\alpha^6 + \frac{1}{2211840}\alpha^8 - \text{etc.}\right)\cos 2x$$

$$+ \frac{1}{192}\alpha^4\left(1 - \frac{1}{20}\alpha^2 + \frac{1}{960}\alpha^4 - \frac{1}{80640}\alpha^6 + \text{etc.}\right)\cos 4x$$

$$+ \frac{1}{23040}\alpha^6\left(1 - \frac{1}{28}\alpha^2 + \frac{1}{1792}\alpha^4 - \text{etc.}\right)\cos 6x$$

$$+ \frac{1}{5160960}\alpha^8\left(1 - \frac{1}{36}\alpha^2 + \text{etc.}\right)\cos 8x$$

$$+ \frac{1}{1857945600}\alpha^{10}(1 - \text{etc.})\cos 10x$$

$$+ \text{etc.}$$

$$\sin(\alpha \sin x) =$$

$$\alpha\left(1 - \frac{1}{8}\alpha^2 + \frac{1}{192}\alpha^4 - \frac{1}{9216}\alpha^6 + \frac{1}{737280}\alpha^8 - \frac{1}{88473600}\alpha^{10} + \text{etc.}\right)\sin x$$

$$+ \frac{1}{24}\alpha^3\left(1 - \frac{1}{16}\alpha^2 + \frac{1}{640}\alpha^4 - \frac{1}{46080}\alpha^6 + \frac{1}{5160960}\alpha^8 - \text{etc.}\right)\sin 3x$$

$$+ \frac{1}{1920}\alpha^5\left(1 - \frac{1}{24}\alpha^2 + \frac{1}{1344}\alpha^4 - \frac{1}{129024}\alpha^6 + \text{etc.}\right)\sin 5x$$

$$+ \frac{1}{322560}\alpha^7\left(1 - \frac{1}{32}\alpha^2 + \frac{1}{2304}\alpha^4 - \text{etc.}\right)\sin 7x$$

$$+ \frac{1}{92897280}\alpha^9\left(1 - \frac{1}{40}\alpha^2 + \text{etc.}\right)\sin 9x$$

$$+ \frac{1}{40874803200}\alpha^{11}(1 - \text{etc.})\sin 11x$$

$$+ \text{etc.}$$

Making use of these series, the expression for the resultant oscillation from the gitter becomes

$$-\mathrm{w}\sum_0^\infty{}_{(\text{even } m)}A_m\frac{\epsilon^m\omega^{m-1}}{m!2^{m-2}}\sum\nolimits_r\left(\cos mr\theta\cdot\sin(r\omega-\tau)\cdot\cos\frac{1}{2}m\theta\cdot\sin\frac{1}{2}\alpha\omega\right.$$
$$\left.+\sin mr\theta\cdot\cos(r\omega-\tau)\cdot\sin\frac{1}{2}m\theta\cdot\cos\frac{1}{2}\alpha\omega\right)$$

$$-\mathrm{w}\sum_1^\infty{}_{(\text{odd } m)}A_m\frac{\epsilon^m\omega^{m-1}}{m!2^{m-2}}\sum\nolimits_r\left(\cos mr\theta\cdot\sin(r\omega-\tau)\cdot\sin\frac{1}{2}m\theta\cdot\cos\frac{1}{2}\alpha\omega\right.$$
$$\left.+\sin mr\theta\cdot\cos(r\omega-\tau)\cdot\cos\frac{1}{2}m\theta\cdot\sin\frac{1}{2}\alpha\omega\right).$$

The summation relatively to r may be effected by means of the formula,

$$\sum\nolimits_x\sin(hx+a)\cdot\sin(kx+b)$$
$$=\frac{-\sin(hx+a-\frac{1}{2}h)\cdot\cos(kx+b-\frac{1}{2}k)\cdot\cos\frac{1}{2}h\cdot\sin\frac{1}{2}k}{\cos h-\cos k}$$
$$+\frac{\cos(hx+a-\frac{1}{2}h)\cdot\sin(kx+b-\frac{1}{2}k)\cdot\sin\frac{1}{2}h\cdot\cos\frac{1}{2}k}{\cos h-\cos k}.$$

For a modern gitter, it would be quite as satisfactory to consider r as infinite, and to use, in place of the above, an infinitesimal formula, which will be found in Hirsch's Integral Tables. Applying, however, the formula of finite integration, we have, as an integrated expression for the resultant oscillation from the whole gitter,

$$\frac{\mathrm{w}}{\omega}\cdot\frac{A_0}{1-\cos\omega}\cos\left(r\omega-\tau-\frac{1}{2}\omega\right)\left[\cos\frac{1}{2}(\omega-\alpha\omega)-\cos\frac{1}{2}(\omega+\alpha\omega)\right]$$

$$+\mathrm{w}\sum_2^\infty{}_{(\text{even } m)}A_m\frac{\frac{\epsilon^m\omega^{m-1}}{m!2^{m-1}}}{\cos m\theta-\cos\omega}\left\{-\sin m\left(r\theta-\frac{1}{2}\theta\right)\right.$$
$$\cdot\sin\left(r\omega-\tau-\frac{1}{2}\omega\right)\cdot\sin m\theta\cdot\sin\frac{1}{2}(\omega-\alpha\omega)+\cos m\left(r\theta-\frac{1}{2}\theta\right)$$
$$\left.\cdot\cos\left(r\omega-\tau-\frac{1}{2}\omega\right)\left[\cos m\theta\cdot\cos\frac{1}{2}(\omega-\alpha\omega)-\cos\frac{1}{2}(\omega+\alpha\omega)\right]\right\}$$

$$+\mathrm{w}\sum_1^\infty{}_{(\text{odd } m)}A_m\frac{\frac{\epsilon^m\omega^{m-1}}{m!2^{m-1}}}{\cos m\theta-\cos\omega}\left\{\cos m\left(r\theta-\frac{1}{2}\theta\right)\cdot\cos\left(r\omega-\tau-\frac{1}{2}\omega\right)\right.$$

$$\cdot \sin m\theta \cdot \sin\frac{1}{2}(\omega - \alpha\omega) - \sin m\left(r\theta - \frac{1}{2}\theta\right)$$
$$\cdot \sin\left(r\omega - \tau - \frac{1}{2}\omega\right)\left[\cos m\theta \cdot \cos\frac{1}{2}(\omega - \alpha\omega) - \cos\frac{1}{2}(\omega + \alpha\omega)\right]\Bigg\}.$$

This expression may be simplified by writing

$$x = \frac{1}{2}(\omega + m\theta),$$
$$y = \frac{1}{2}(\omega - m\theta);$$

so that

$$\sin\left[\left(r - \frac{1}{2}\right)m\theta\right] \cdot \sin\left[\left(r - \frac{1}{2}\right)\omega - \tau\right] = \frac{1}{2}\cos[(2r - 1)y - \tau]$$
$$- \frac{1}{2}\cos[(2r - 1)x - \tau]\cos\left[\left(r - \frac{1}{2}\right)m\theta\right] \cdot \cos\left[\left(r - \frac{1}{2}\right)\omega - \tau\right]$$
$$= \frac{1}{2}\cos[(2r - 1)y - \tau] + \frac{1}{2}\cos[(2r - 1)x - \tau].$$

We have also to observe that

$$\mp \sin m\theta \cdot \sin\frac{1}{2}(\omega - \alpha\omega) + \cos m\theta \cdot \cos\frac{1}{2}(\omega - \alpha\omega) - \cos\frac{1}{2}(\omega + \alpha\omega)$$
$$= \cos\left[\frac{1}{2}(\omega - \alpha\omega) \pm m\theta\right] - \cos\frac{1}{2}(\omega + \alpha\omega) = +2\sin\frac{1}{2}(\omega \pm m\theta)$$
$$\cdot \sin\frac{1}{2}(\alpha\omega \mp m\theta).$$

Thus, the quantity in parenthesis, under the sum for even values of m, reduces to

$$\cos[(2r - 1)y - \tau] \cdot \sin\frac{1}{2}(\omega + m\theta) \cdot \sin\frac{1}{2}(\alpha\omega - m\theta)$$
$$+ \cos[(2r - 1)x - \tau] \cdot \sin\frac{1}{2}(\omega - m\theta) \cdot \sin\frac{1}{2}(\alpha\omega + m\theta),$$

and the corresponding quantity for odd values of m, to

$$- \cos[(2r - 1)y - \tau] \cdot \sin\frac{1}{2}(\omega + m\theta) \cdot \sin\frac{1}{2}(\alpha\omega - m\theta)$$
$$+ \cos[(2r - 1)x - \tau] \cdot \sin\frac{1}{2}(\omega - m\theta) \cdot \sin\frac{1}{2}(\alpha\omega + m\theta).$$

The integral is to be taken between limiting values of r, say r_1 and r_2. Let the whole number of openings in the gitter be R, so that

$$R = r_2 - r_1.$$

Then, a second equation to determine r_1 and r_2 may be assumed arbitrarily without affecting the result. Let this equation be

$$r_2 + r_1 = 1.$$

Then

$$(2r_2 - 1) = -(2r_1 - 1) = R.$$

Now r occurs only in the factors

$$\cos[(2r - 1)y - \tau] = \cos(2r - 1)y \cdot \cos\tau + \sin(2r - 1)y \cdot \sin\tau$$

and

$$\cos[(2r - 1)x - \tau] = \cos(2r - 1)x \cdot \cos\tau + \sin(2r - 1)x \cdot \sin\tau.$$

Taken between these limits, these factors will be respectively,

$$2\sin Ry \cdot \sin\tau,$$
$$2\sin Rx \cdot \sin\tau.$$

Applying these reductions, and also remembering that

$$\cos m\theta - \cos\omega = 2\sin x \sin y,$$

the expression for the resultant oscillation from the whole gitter reduces to

$$\sin\tau \cdot \mathrm{w} \sum_{-\infty}^{+\infty} {}_mA_m \frac{\epsilon^m \omega^{m-1}}{m!2^{m-1}} \cdot \frac{\sin\frac{1}{2}R(\omega + m\theta)}{\sin\frac{1}{2}(\omega + m\theta)} \sin\frac{1}{2}(\alpha\omega + m\theta),$$

where, in summing for negative values of m, positive values are to be taken in the coëfficients, and where terms arising from odd negative values of m in the parenthesis are to have the opposite sign, and where the term in $m = 0$ is to have only half the above value.

We have now to study the principal maxima of the amplitude of this oscillation, for varying ω. Taking each term of the series separately, we observe that one factor of it, namely,

$$\frac{\sin\frac{1}{2}R(\omega + m\theta)}{\sin\frac{1}{2}(\omega + m\theta)},$$

reaches a maximum when

$$\omega + m\theta = 2N\pi,$$

and this maximum value is R. Now R is a number amounting to several thousand, while α is less than unity. Hence, the maximum of the whole term will be very nearly at the same place, and one of the maxima of the sum of all the terms will also be nearly in that place.

To ascertain the precise position of the maximum of any one term, put

$$\omega = 2N\pi - m\theta + \delta\omega.$$

Then, neglecting the cube of $\delta\omega$, in comparison with unity, we have

$$\sin\frac{1}{2}R(\omega + m\theta) = \pm\sin\frac{1}{2}R\,\delta\omega = \pm\frac{1}{2}R\,\delta\omega \mp \frac{1}{48}R^3(\delta\omega)^3$$

$$\sin\frac{1}{2}(\omega + m\theta) = \pm\sin\frac{1}{2}\,\delta\omega = \pm\frac{1}{2}\,\delta\omega \mp \frac{1}{48}(\delta\omega)^3$$

$$\frac{\sin\frac{1}{2}R(\omega + m\theta)}{\sin\frac{1}{2}(\omega + m\theta)} = \pm\frac{\sin\frac{1}{2}R\,\delta\omega}{\sin\frac{1}{2}\delta\omega} = \pm R \mp \frac{1}{24}(R^3 - R)(\delta\omega)^2.$$

As for $\sin\frac{1}{2}(\alpha\omega + (-1)^m m\theta)$, it may have any value whatever from -1 to $+1$, according to the magnitude of α. But it is when it vanishes that the maximum is at the greatest value of $\delta\omega$. Let us then suppose

$$\sin\frac{1}{2}(\alpha\omega + (-1)^m m\theta) = \pm\frac{1}{2}\alpha\delta\omega \mp \frac{1}{48}\alpha^3(\delta\omega)^3.$$

Finally, there is the factor ω^{m-1}. Dividing this by $(2N\pi - m\theta)^{m-1}$, we have

$$\left(\frac{\omega}{2N\pi - m\theta}\right)^{m-1} = 1 + (m-1)(2N\pi - m\theta)^{-1}\delta\omega + \frac{(m-1)(m-2)}{2}(2N\pi - m\theta)^{-2}(\delta\omega)^2;$$

finally, multiplying together the quantities thus obtained, we find as that factor of the m^{th} term which contains $(\delta\omega)$

$$\delta\omega + (m-1)(2N\pi - m\theta)^{-1}\delta\omega^2 + \left\{\frac{(m-1)(m-2)}{2}(2N\pi - m\theta)^{-2} - \frac{1}{24}\alpha^2 - \frac{1}{24}(R^2 - 1)\right\}(\delta\omega)^3.$$

Differentiating, we find as the equation for determining the value of $\delta\omega$ at the maximum of the m^{th} term

$$1 + 2(m-1)(2N\pi - m\theta)^{-1}\delta\omega$$
$$+ 3\left\{\frac{(m-1)(m-2)}{2}(2N\pi - m\theta)^{-2} - \frac{1}{24}\alpha^2 - \frac{1}{24}(R^2 - 1)\right\}(\delta\omega)^2 = 0.$$

If we neglect $\frac{1}{R^2}$, the solution of this equation is

$$\delta\omega = \frac{8(m-1)}{R^2(2N\pi - m\theta)}.$$

It will be seen that $\delta\omega$ is zero when $m = 1$, and that for the principal spectrum, for which $m = 0$, if $R = 1000$, $\frac{\delta\omega}{\omega}$ is altogether inappreciable, but if $R = 100$, $\frac{\delta\omega}{\omega} =$ about $\frac{1}{50000}$ for the first order, which displaces the spectrum by about $\frac{1}{50}$ part of the distance between the two D lines.

We have now to consider how far the maxima of the sum of the series representing the oscillation may differ from those of the single terms. A term will have the most influence in displacing a maximum when it is itself nearly zero, or more accurately when its differential coëfficient relatively to ω is at a maximum. As ω increases by 2π so as to pass from one principal maximum of oscillation to another, $R\omega$ passes R times through 2π, so that the term passes through as many maxima and minima. Then the differential coëfficient relative to ω of the sum of all the terms will be the greatest for a value of ω such that

$$\omega + m_0\theta = 2N\pi,$$

(m_0 being a given value of m), when, in addition to the above equation, we have

$$R\theta = 4N\pi.$$

In this case, the differential coëfficient of the m^{th} term of the expression for the oscillation will be

$$\frac{R}{\omega} m!\left(\frac{\epsilon\omega}{2}\right)^2 \frac{1}{\sin\frac{1}{2}(\omega + m\theta)}.$$

It will be sufficiently accurate to put

$$\sin\frac{1}{2}(\omega + m\theta) = \frac{1}{2}(m - m_0)\theta.$$

Then it is plain that, were the term for $m = 0$ of the same value as the others, the total differential coëfficient would be

$$\frac{R}{\omega}m_0 e^{\left(\frac{\epsilon\omega}{2}\right)}.$$

Owing, however, to the term for $m = 0$ having only half the value given by the formula, the value is

$$\frac{R}{\omega}m_0\left(e^{\left(\frac{\epsilon\omega}{2}\right)} - \frac{1}{2}\right).$$

In consequence of the differential coëfficient having this value, the maximum will not occur exactly at the value of α for which

$$\omega + m_0\theta = 2N\pi,$$

but will be shifted along to the point where the differential coëfficient of the $m_0{}^{\text{th}}$ term is equal to the negative of the differential coëfficient just found. If $\delta\omega$ is the amount of the shifting, the $m_0{}^{\text{th}}$ term of the oscillation (R being very large) is

$$\frac{\sin\cdot\frac{R}{2}\delta\omega}{\delta\omega}.$$

The differential coëfficient of this is

$$\frac{1}{4}\cdot\frac{\sin\cdot R\cdot\delta\omega - R\delta\omega}{(\delta\omega)^2},$$

and the equation to determine $\delta\omega$ is

$$\frac{1}{4}\cdot\frac{\sin\cdot R\delta\omega - R\delta\omega}{(\delta\omega)^2} = \frac{R}{\omega}m_0\left(1 - e^{\frac{\epsilon\omega}{2}}\right).$$

In the worst case, this becomes

$$\delta\omega = \frac{24}{R^2}m_0\left(e^{\frac{\epsilon\omega}{2}} - 1\right).$$

It thus appears that the position of the principal spectrum will not be disturbed by the circumstance here considered, and that the distance between the successive ghosts will be very slightly altered.

It is to be remarked that, when two spectral lines fall very near together, they will be attracted to one another in consequence of the mixture of light by a sensible amount. This will especially affect the position of a faint line near a very intense one.

The Phenomena.

Mr. Rutherfurd's diffraction-plates are ruled with a machine which is described by Professor A. M. Mayer in the article "Spectrum," in the second edition of *Appleton's Cyclopædia.* In consequence of the periodic error of the screw, a periodic inequality is produced in the ruling. This is shewn by putting a gitter into the spectrometer, illuminating it with homogeneous light, and observing it without the eye-piece, when it appears striped. If the eye-piece is replaced and a real solar spectrum is thrown on the slit-plate, of such purity that the light admitted into the slit varies only by a few ten-thousandths of a micron on wave-length, the maxima of light which have been investigated above appear as repetitions of the principal spectrum, in which even the fine lines due to the solar atmosphere are distinctly visible.

The positions of some of these "ghosts," or repetitions of the principal spectrum, have been carefully measured in order to test the theory.

Measures of the Positions of the Ghosts.

To determine whether the screw of the filar micrometer had the same pitch throughout its length, the distance between D_1 and D_2 was measured on different places on the screw. Gitter: speculum metal 681 lines to the millimetre. Second order, principal spectrum. Readings given are means of five pointings each. Date: 1879, July 3.

Place on the Screw	First End.		Second End.		Second End.		First End.	
Line of Spectrum	D_1	D_2	D_1	D_2	D_2	D_1	D_2	D_1
Micrometer reading	$7^r.109$	$7^r.947$	$12^r.108$	$12^r.943$	$12^r.937$	$12^r.102$	$7^r.925$	$7^r.089$
Distance of Lines	$0^r.838$		$0^r.835$		$0^r.835$		$0^r.836$	

The following were made with a speculum-metal gitter of $340\frac{1}{2}$ teeth to the millimetre. Each reading given is the mean of five pointings. Date: 1879, July 3. To pass from one spectrum to another the gitter alone was turned.

Order of Spectrum	Order IV.						
Number of Ghost	Ghost, – 1.		Ghost, 0.		Ghost, + 1.		Means.
Line of Spectrum	D_2	D_1	D_2	D_1	D_2	D_1	
Micrometer reading	8'.241	9'.330	9'.723	10'.800	11'.187	12'.272	
Distance ($D_1 - D_2$)	1'.089		1'.077		1'.085		1'.084
Distance of successive Ghosts D_2		1'.482		1'.464			1.473
Distance of successive Ghosts D_1		1.470		1.472			1.471
Distance of successive Ghosts Mean		1.476		1.468			1.472

Order of Spectrum	Order V.						
Number of Ghost	Ghost, – 1.		Ghost, 0.		Ghost, + 1.		Means.
Line of Spectrum	D_2	D_1	D_2	D_1	D_2	D_1	
Micrometer reading	7'.847	9'.337	9'.466	10'.962	11'.090	12'.575	
Distance ($D_1 - D_2$)	1'.490		1'.496		1'.485		1'.490
Distance of successive Ghosts D_2		1'.619		1'.624			1.621
Distance of successive Ghosts D_1		1.625		1.613			1.619
Distance of successive Ghosts Mean		1.622		1.618			1.620

Order of Spectrum	Order VI.						
Number of Ghost	Ghost, – 1.		Ghost, 0.		Ghost, + 1.		Means.
Line of Spectrum	D_2	D_1	D_2	D_1	D_2	D_1	
Micrometer reading	7'.378	9'.421	9'.265	11'.304	11'.152	13'.173	
Distance ($D_1 - D_2$)	2'.043		2'.039		2'.021		2'.034
Distance of successive Ghosts D_2		1'.887		1'.887			1.887
Distance of successive Ghosts D_1		1.883		1.869			1.876
Distance of successive Ghosts Mean		1.885		1.878			1.881

Order of Spectrum	Order VII.						
Number of Ghost	Ghost, – 1.		Ghost, 0.		Ghost, + 1.		Means.
Line of Spectrum	D_2	D_1	D_2	D_1	D_2	D_1	
Micrometer reading	6'.637	9'.595	8'.955	11'.876	11'.262	14'.191	
Distance ($D_1 - D_2$)	2'.958		2'.921		2'.929		2'.936
Distance of sucessive Ghosts D_2		2'.318		2'.307			2.312
Distance of sucessive Ghosts D_1		2.281		2.315			2.298
Distance of sucessive Ghosts Mean		2.299		2.311			2.305

Order of Spectrum	Order VIII.						
Number of Ghost	Ghost, – 1.		Ghost, 0.		Ghost, + 1.		Means.
Line of Spectrum	D_2	D_1	D_2	D_1	D_2	D_1	
Micrometer reading	4'.737	9'.467	8'.002	12'.680	11'.256	15'.885	
Distance ($D_1 - D_2$)	4'.730		4'.678		4'.629		4'.679
Distance of successive Ghosts D_2		3'.265		3'.254			3.261
Distance of successive Ghosts D_1		3.213		3.205			3.209
Distance of successive Ghosts Mean		3.239		3.229			3.234

Order of Spectrum	Order IX.						
Number of Ghost	Ghost, – 1.		Ghost, 0.		Ghost, + 1.		Means.
Line of Spectrum	D_2	D_1	D_2	D_1	D_2	D_1	
Micrometer reading	6'.865[1]	9'.403	4'.281	16'.977	12'.075	24'.435	
Distance ($D_1 - D_2$)	12'.538		12'.696		12'.360		12'.532
Distance of sucessive Ghosts D_2		7'.416		7'.794			7.605
Distance of sucessive Ghosts D_1		7.574		7.458			7.516
Distance of sucessive Ghosts Mean		7.495		7.626			7.560

1. Read 5'.865. Either this is an erroneous reading, or a wrong line was measured.

The following measures were made with a metal gitter of 681 lines to the millimetre. Dates: 1879, June 20 and July 2.

Order of Spectrum		Order I.										
Number of Ghost		Ghost, −2.		Ghost, −1.		Ghost, 0.		Ghost, +1.		Ghost, +2.		Means.
Line of Spectrum		D_2	D_1	D_2	D_1	D_2	D_1	D_2	D_1	D_2	D_1	
Micrometer reading		$7^r.286$	$7^r.799$	$8^r.632$	$9^r.112$	$9^r.925$	$10^r.383$	$11^r.196$	$11^r.664$	$12^r.496$	$12^r.928$	
$D_1 - D_2$		$0^r.513$		$0^r.480$		$0^r.458$		$0^r.468$		$0^r.432$		$0^r.470$
Distance of	D_2		$1^r.346$		$1^r.293$		$1^r.271$		$1^r.300$			1.302
successive	D_1		1.313		1.271		1.281		1.264			1.282
Ghosts	Mean		1.330		1.282		1.276		1.282			1.292

Order of Spectrum		Order II.										
Number of Ghost		Ghost, −2.		Ghost, −1.		Ghost, 0.		Ghost, +1.		Ghost, +2.		Means.
Line of Spectrum		D_2	D_1	D_2	D_1	D_2	D_1	D_2	D_1	D_2	D_1	
Micrometer reading		$5^r.312$	$6^r.482$	$6^r.907$	$8^r.059$	$8^r.477$	$9^r.627$	$10^r.067$	$11^r.191$	$11^r.632$	$12^r.752$	
$D_1 - D_2$		$1^r.170$		$1^r.152$		$1^r.150$		$1^r.124$		$1^r.120$		$1^r.143$
Distance of	D_2		$1^r.595$		$1^r.570$		$1^r.590$		$1^r.565$			1.580
successive	D_1		1.577		1.568		1.564		1.561			1.568
Ghosts	Mean		1.586		1.569		1.577		1.563			1.574

Order of Spectrum		Order III.										
Number of Ghost		Ghost, −2.		Ghost, −1.		Ghost, 0.		Ghost, +1.		Ghost, +2.		Means.
Line of Spectrum		D_2	D_1	D_2	D_1	D_2	D_1	D_2	D_1	D_2	D_1	
Micrometer reading		$4^r.593$	$6^r.896$	$6^r.713$	$9^r.053$	$8^r.876$	$11^r.205$	$10^r.989$	$13^r.280$	$13^r.057$	$15^r.308$	
$D_1 - D_2$		$2^r.303$		$2^r.340$		$2^r.329$		$2^r.291$		$2^r.251$		$2^r.303$
Distance of	D_2		$2^r.120$		$2^r.163$		$2^r.113$		$2^r.068$			2.116
successive	D_1		2.157		2.152		2.075		2.028			2.103
Ghosts	Mean		2.138		2.158		2.094		2.048			2.110

The following measures were made on spectra produced by a narrow silvered-glass plate of 681 lines to the millimetre. This gitter was selected as making unusually bright ghosts. The refraction by the glass must sensibly displace the ghosts. The two sodium lines, and the nickel line between them, were observed. Date: 1879, June 19. [Table on p. 63.]

The following measures were made upon C, with the metal gitter of 681 lines per mm. The distance of the fine line $\lambda = 6567.91$ (Å.) from C was measured in the principal spectrum to determine the dispersion. Date: 1879, July 1.

	Order I.				
Ghost, −1.		Ghost, 0.		Ghost, +1.	Fine line.	C.
$8^r.241$		$9^r.792$		$11^r.289$	$9^r.255$	$9^r.801$
	$1^r.551$		$1^r.497$		$0^r.546$	

	Order II.				
Ghost, −1.		Ghost, 0.		Ghost, +1.	Fine line.	C.
$8^r.054$		$9^r.941$		$11^r.774$	$8^r.629$	$9^r.960$
	$1^r.887$		$1^r.833$		$1^r.331$	

	Order III.				
Ghost, −1.		Ghost, 0.		Ghost, +1.	Fine line.	C.
$7^r.115$		$10^r.010$		$12^r.734$	$7^r.054$	
	$2^r.895$		$2^r.724$		$2^r.956$	

Order of Sp.								ORDER I.								
No. of Ghost	Ghost, −2.			Ghost, −1.			Ghost, 0.			Ghost, +1.			Ghost, +2.			Means.
Line of Sp.	D_2		D_1	D_2		D_1	D_2		D_1	D_2		D_1				
Mic. reading	9.′076		9.′590	10.′405		10.′886	11.′695		12.′164	12.′976		13.′439				
$D_1 - D_2$		0.′513			0.′480			0.′469			0.′463					0.′481
Dist. suc. D_2			1.′329			1.′290			1.′281							1.300
Ghosts D_1			1.296			1.279			1.275							1.283
Mean			1.312			1.284			1.278							1.291

Order of Sp.								ORDER II.								
No. of Ghost	Ghost, −2.			Ghost, −1.			Ghost, 0.			Ghost, +1.			Ghost, +2.			Means.
Line of Sp.	D_2		D_1	D_2	Ni	D_1	D_2	Ni	D_1	D_2	Ni	D_1	D_2	Ni	D_1	
Mic. reading	5.′451		6.′531	6.′916	7.′465	7.′981	8.′370	8.′926	9.′439	9.′838	10.′379	10.′897	11.′289	11.′837	12.′345	
Ni − D_2		1.′080		0.′549			0.′556			0.′541			0.′548			0.′548
D_1 − Ni					0.′516			0.′513			0.′518			0.′508		0.514
Dist. D_2			1.′465			1.′454			1.′468			1.′451				1.460
success. Ni						1.461			1.453			1.458				1.457
Ghosts D_1			1.450			1.458			1.458			1.448				1.454
Mean			1.458			1.458			1.460			1.452				1.457

Order of Sp.								ORDER III.								
No. of Ghost	Ghost, −2.			Ghost, −1.			Ghost, 0.			Ghost, +1.			Ghost, +2.			Means.
Line of Sp.	D_2	Ni	D_1	D_2	Ni	D_1	D_2	Ni	D_1	D_2	Ni	D_1	D_2	Ni	D_1	
Mic. reading	6.′822	7.′874	8.′843	8.′671	9.′715	10.′693	10.′515	11.′559	12.′535	12.′365	13.′399	14.′372	14.′192	15.′228	16.′198	
Ni − D_2	1.′052			1.′044			1.′044			1.′034			1.′036			1.′044
D_1 − Ni		0.′969			0.′978			0.′976			0.′973			0.′970		0.973
Dist. D_2			1.′849			1.′844			1.′850			1.′827				1.842
success. Ni			1.841			1.844			1.840			1.829				1.838
Ghosts D_1			1.850			1.842			1.837			1.826				1.839
Mean			1.847			1.843			1.842			1.827				1.840

Order of Sp.								ORDER IV.								
No. of Ghost	Ghost, −2.			Ghost, −1.			Ghost, 0.			Ghost, +1.			Ghost, +2.			Means.
Line of Sp.	D_2		D_1	D_2		D_1	D_2		D_1	D_2		D_1	D_2		D_1	
Mic. reading	3.′614		8.′222	6.′777		11.′361	9.′939		14.′473	13.′064		17.′581	16.′148		20.′616	
$D_1 - D_2$		4.′608			4.′584			4.′534			4.′517			4.′468		4.′542
Dist.																
success. D_2			3.′163			3.′162			3.′125			3.′084				3.134
Ghosts D_1			3.139			3.112			3.108			3.035				3.096
Mean			3.151			3.137			3.116			3.060				3.115

The following measure was made upon F, with the same gitter. The mean of lines 4870.47 and 4871.29 was pointed on to determine the dispersion. Date: 1879, July 1.

Order II.

Double.		F.	
Ghost, 0.		Ghost, 0.	Ghost, + 1.
$8^{r}.617$		$10^{r}.484$	$11^{r}.683$
	$1^{r}.867$		$1^{r}.190$

The above measures satisfy the theory moderately well. Thus, according to theory, the product of the ratio of the distance of successive ghosts to the distance between the D line by the order of the spectrum should be constant, and should be twice as great for the gitter of $340\frac{1}{2}$ lines to the millimetre as for that of 681 lines to the millimetre. Now this product is as follows:

Metal Gitter of $340\frac{1}{2}$ *lines to the mm.*

Order	IV.	$5.43 = 2 \times 2.72$
"	V.	$5.44 = 2 \times 2.72$
"	VI.	$5.55 = 2 \times 2.77$
"	VII.	$5.50 = 2 \times 2.75$
"	VIII.	$5.53 = 2 \times 2.76$
"	IX.	$5.46 = 2 \times 2.73$

Metal Gitter of 681 *lines to the mm.*

Order	I.	2.75
"	II.	2.75
"	III.	2.75

Silvered-glass Gitter of 681 *lines to the mm.*

Order	I.	2.68
"	II.	2.74
"	III.	2.74
"	IV.	2.74

It is evident that the value which best satisfies the observations lies between 2.74 and 2.75. This ratio multiplied by the ratio of the difference of wave-length of the D lines to their mean wave-length, should give the number of lines of the finer gitters to a period of the inequality. This, from the construction of the ruling-machine, is known to be nearly, but not exactly, 360. Mr. Chapman, who works with the machine, has made certain observations, from which it would appear that the period differs about 1 per cent. from 360. The product of the ratios just mentioned (taking 2.746 for the first) is 357. This is therefore a happy confirmation of the theory.

Next, using the value 2.746, I calculate by least squares the best values of the distance of the D lines and the distance of consecutive ghosts in each order. In this way, we shall be able to judge whether the discrepancies of the observations from theory are, or are not, greater than their probable errors. The results are as follows:

Metal Gitter of $340\frac{1}{2}$ *lines to the mm.*

	Distance $D_1 - D_2$.			Distance of successive Ghosts.		
Order.	Obs.	Calc.	O. – C.	Obs.	Calc.	O. – C.
IV.	$1^{r}.084$	$1^{r}.076$	$+0^{r}.008$	$1^{r}.472$	$1^{r}.477$	$-0^{r}.005$
V.	1.490	1.481	+0.009	1.620	1.626	−0.006
VI.	2.034	2.045	−0.011	1.881	1.872	+0.009
VII.	2.936	2.936	0.000	2.305	2.305	0.000
VIII.	4.679	4.691	−0.012	3.234	3.221	+0.013
IX.	12.532	12.485	+0.047	7.560	7.618	−0.058

Metal Gitter of 681 *lines to the mm.*

	Distance $D_1 - D_2$.			Distance of successive Ghosts.		
Order.	Obs.	Calc.	O. – C.	Obs.	Calc.	O. – C.
I.	$0^{r}.470$	$0^{r}.470$	$0^{r}.000$	$1^{r}.292$	$1^{r}.292$	$0^{r}.000$
II.	1.143	1.147	−0.004	1.574	1.573	+0.001
III.	2.303	2.304	−0.001	2.110	2.109	+0.001

Silvered-glass Gitter of 681 *lines per mm.*

	Distance $D_1 - D_2$.			Distance of successive Ghosts.		
Order.	Obs.	Calc.	O. – C.	Obs.	Calc.	O. – C.
I.	$0^{r}.481$	$0^{r}.470$	$+0^{r}.011$	$1^{r}.291$	$1^{r}.292$	$-0^{r}.001$
II.	1.062	1.063	−0.001	1.457	1.457	0.000
III.	2.017	2.021	−0.004	1.840	1.838	+0.002
IV.	4.542	4.544	−0.002	3.115	3.113	+0.002

The discrepancies between observation and calculation are, in the case of the observations with the coarse-ruled plate in the 4^{th} to the 7^{th} orders, inclusive, pretty well accounted for by the attractions of neighboring lines. This is shown by the subjoined table. In other cases, there are large discrepancies amounting to 7″, or even more, which cannot be so accounted for, and which vastly exceed the errors of observation. Thus, it will almost invariably be found that the ghosts of D_1 are closer together than those of D_2, and that the distances decrease as m increases algebraically. The measures of the ghosts of C and F indicate a much longer period in the inequality. Some attempts have been made to measure the brilliancy of the ghosts. These only roughly agree with the theory.

DETAILED COMPARISON OF CALCULATION AND OBSERVATION.

Metal Gitter of $340\frac{1}{2}$ *lines per mm.*

Order IV.

	Obs.	Calc.	O. – C.	
$G-1, D_2$	$8\overset{\mathrm{r}}{.}241$	$8\overset{\mathrm{r}}{.}244$	–.003	
$G-1, D_1$	9.330	9.320	+.010	Carried toward $G\,0, D_2$.
$G\,0, D_2$	9.723	9.721	+.002	
$G\,0, D_1$	10.800	10.797	+.003	
$G+1, D_2$	11.187	11.198	–.011	Carried toward $G\,0, D_1$.
$G+1, D_1$	12.272	12.274	–.002	

Order V.

	Obs.	Calc.	O. – C.	
$G-1, D_2$	$7\overset{\mathrm{r}}{.}847$	$7\overset{\mathrm{r}}{.}846$	+.001	
$G-1, D_1$	9.337	9.327	+.010	Carried toward $G\,0, D_2$.
$G\,0, D_2$	9.466	9.472	–.006	Carried toward $G-1, D_1$.
$G\,0, D_1$	10.962	10.953	+.009	Carried toward $G+1, D_2$.
$G+1, D_2$	11.090	11.098	–.008	Carried toward $G\,0, D_1$.
$G+1, D_1$	12.575	12.579	–.004	

Order VI.

	Obs.	Calc.	O. – C.	
$G-1, D_2$	$7\overset{\mathrm{r}}{.}387$	$7\overset{\mathrm{r}}{.}388$	–.001	
$G\,0, D_2$	9.265	9.260	+.005	Carried a little toward $G-1, D_1$.
$G-1, D_1$	9.421	9.433	–.012	Carried toward $G\,0, D_2$.
$G+1, D_2$	11.152	11.132	+.020	Carried toward $G\,0, D_1$.
$G\,0, D_1$	11.304	11.305	–.001	Carried a little toward $G+1, D_2$.
$G+1, D_1$	13.173	13.177	–.004	

Order VII.

	Obs.	Calc.	O. – C.	
$G-1, D_2$	$6\overset{\mathrm{r}}{.}637$	$6\overset{\mathrm{r}}{.}646$	–.009	Single pointings discordant. Rejecting worst obs. = $6\overset{\mathrm{r}}{.}643$.
$G\,0, D_2$	8.955	8.951	+.005	Carried toward $G-1, D_2$.
$G-1, D_1$	9.595	9.582	+.013	Should be carried toward $G\,0, D_2$.
$G+1, D_2$	11.262	11.256	+.006	Carried toward $G\,0, D_1$.
$G\,0, D_1$	11.876	11.887	–.011	Carried toward $G+1, D_2$.
$G+1, D_1$	14.191	14.192	–.001	

Order VIII.

	Obs.	Calc.	O. – C.	
$G-1, D_2$	$4\overset{\mathrm{r}}{.}737$	$4\overset{\mathrm{r}}{.}771$	–.034	No distinct attractions.
$G\,0, D_2$	8.002	7.992	+.010	
$G-1, D_1$	9.467	9.462	+.005	
$G+1, D_2$	11.256	11.213	+.043	
$G\,0, D_1$	12.680	12.683	–.003	
$G+1, D_1$	15.885	15.904	–.019	

Order IX.

$G-1, D_2$	$6^{\mathrm{r}}865$	$6^{\mathrm{r}}812$	+.053
$G0, D_2$	4.281	4.430	−.149
$G-1, D_1$	9.403	9.297	+.106
$G+1, D_2$	12.075	12.048	+.027
$G0, D_1$	16.977	16.915	+.062
$G+1, D_1$	24.435	24.533	−.098

Metal Gitter 681 *lines per mm.*

Order I.

				O. − C.	
				−.012	
$G-2, D_2$	$7^{\mathrm{r}}286$	$7^{\mathrm{r}}323$	−.037	−.049	Noted at the time of obs. extremely uncertain.
$G-2, D_1$	7.799	7.793	+.006	−.006	
$G-1, D_2$	8.632	8.615	+.017	+.005	
$G-1, D_1$	9.112	9.085	+.027	+.015	
$G0, D_2$	9.925	9.907	+.018	+.006	
$G0, D_1$	10.383	10.377	+.006	−.006	General attraction toward the middle.
$G+1, D_2$	11.196	11.199	−.003	−.015	
$G+1, D_1$	11.664	11.669	−.005	−.017	
$G+2, D_2$	12.496	12.491	+.005	−.007	
$G+2, D_1$	12.928	12.961	−.033	−.045	

Order II.

				O. − C.
				−.004
$G-2, D_2$	$5^{\mathrm{r}}312$	$5^{\mathrm{r}}331$	−.019	−.023
$G-2, D_1$	6.482	6.478	+.004	−.000
$G-1, D_2$	6.907	6.904	+.003	−.001
$G-1, D_1$	8.059	8.051	+.008	+.004
$G0, D_2$	8.477	8.477	.000	−.004
$G0, D_1$	9.627	9.624	+.003	−.001
$G+1, D_2$	10.067	10.050	+.017	+.013
$G+1, D_1$	11.191	11.197	−.006	−.010
$G+2, D_2$	11.632	11.623	+.009	+.005
$G+2, D_1$	12.752	12.770	−.018	−.022

Order III.

$G-2, D_2$	$4^{\mathrm{r}}593$	$4^{\mathrm{r}}627$	−.034
$G-1, D_2$	6.713	6.736	−.023
$G-2, D_1$	6.896	6.931	−.035
$G0, D_2$	8.876	8.845	+.031
$G-1, D_1$	9.053	9.040	+.013
$G+1, D_2$	10.989	10.954	+.035
$G0, D_1$	11.205	11.149	+.056
$G+2, D_2$	13.057	13.063	−.006
$G+1, D_1$	13.280	13.258	+.022
$G+2, D_1$	15.308	15.367	−.059

A Quincuncial Projection of the Sphere

P 135: **American Journal of Mathematics**
2 (1879): 394–96

For meteorological, magnetological and other purposes, it is convenient to have a projection of the sphere which shall show the connection of all parts of the surface. This is done by the one shown in the plate. It is an orthomorphic or conform projection formed by transforming the stereographic projection, with a pole at infinity, by means of an elliptic function. For that purpose, l being the latitude, and θ the longitude, we put

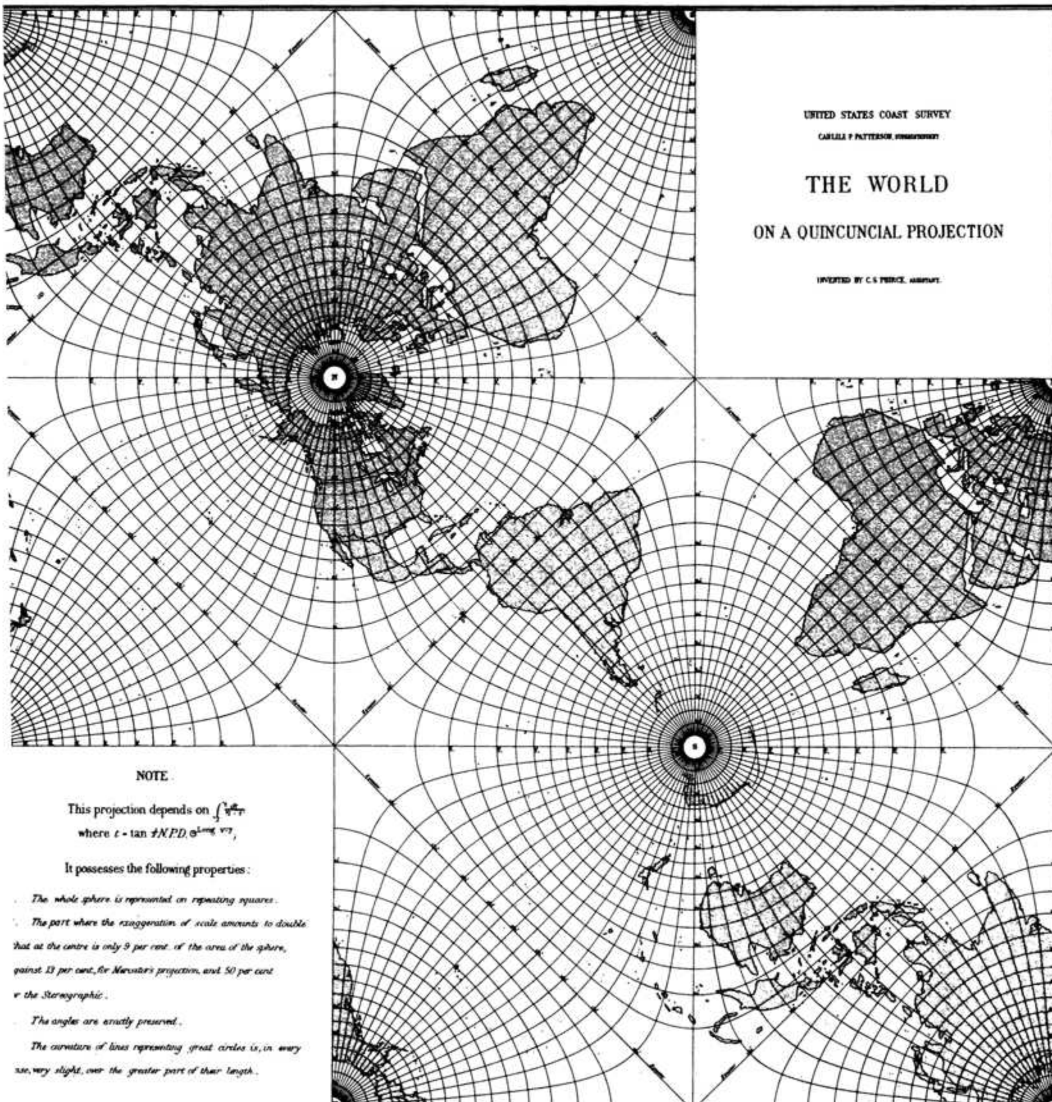
$$\cos^2\varphi = \frac{\sqrt{1-\cos^2 l \cos^2\theta} - \sin l}{1+\sqrt{1-\cos^2 l\cos^2\theta}},$$

and then $\frac{1}{2}F\varphi$ is the value of one of the rectangular coördinates of the point on the new projection. This is the same as taking

$$\cos am\,(x + y\sqrt{-1})\,(\text{angle of mod.} = 45°) = \tan\frac{p}{2}(\cos\theta + \sin\theta\sqrt{-1}),$$

where x and y are the coördinates on the new projection, p is the north polar distance. A table of these coördinates is subjoined.

Upon an orthomorphic projection the parallels represent equipotential or level lines for the logarithmic potential, while the meridians are the lines of force. Consequently we may draw these lines by the method used by Maxwell in his *Electricity and Magnetism* for drawing the corresponding lines for the Newtonian potential. That is to say, let two such projections be drawn upon the same sheet, so that upon both are shown the same meridians at equal angular distances, and the same parallels at such distances that the ratio of successive values of $\tan\frac{p}{2}$ is constant. Then, number the meridians and also the parallels. Then draw curves through the intersections of meridians with meridians, the sums of numbers of the intersecting


UNITED STATES COAST SURVEY
THE WORLD
ON A QUINCUNCIAL PROJECTION
NOTE.
This projection depends on
It possesses the following properties:
The whole sphere is represented on repeating squares.
The part where the exaggeration of scale amounts to double
hat at the centre is only 9 per cent. of the area of the sphere,
gainst 13 per cent., for Mercator's projection, and 50 per cent
r the Stereographic.
The angles are exactly preserved.
The curvature of lines representing great circles is, in every
ase, very slight, over the greater part of their length.

meridians being constant on any one curve. Also, do the same thing for the parallels. Then these curves will represent the meridians and parallels of a new projection having north poles and south poles wherever the component projections had such poles.

Functions may, of course, be classified according to the pattern of the projection produced by such a transformation of the stereographic projection with a pole at the tangent points. Thus we shall have—

1. Functions with a finite number of zeros and infinites (algebraic functions).

2. Striped functions (trigonometric functions). In these the stripes may be equal, or may vary progressively, or periodically. The stripes may be simple, or themselves compounded of stripes. Thus, sin (a sin z) will be composed of stripes each consisting of a bundle of parallel stripes (infinite in number) folded over onto itself.

3. Chequered functions (elliptic functions).

4. Functions whose patterns are central or spiral.

I.
Table of Rectangular Coördinates for Construction of the "Quincuncial Projection."

x (for longitudes in upper line). y (for longitudes in lower line).

LAT.	0° 90	5° 85	10° 80	15° 75	20° 70	25° 65	30° 60	35° 55	40° 50	45° 45	50° 40	55° 35	60° 30	65° 25	70° 20	75° 15	80° 10	85° 5	LAT.
85°	.033	.033	.033	.032	.031	.030	.029	.027	.025	.024	.021	.019	.017	.014	.011	.009	.006	.003	85°
80	.067	.066	.066	.064	.063	.061	.058	.055	.051	.047	.043	.038	.033	.028	.023	.017	.012	.006	80
75	.100	.100	.099	.097	.094	.091	.087	.082	.077	.071	.065	.058	.050	.042	.034	.026	.017	.009	75
70	.135	.134	.133	.130	.127	.122	.117	.110	.103	.095	.087	.077	.067	.057	.046	.035	.023	.012	70
65	.169	.169	.167	.163	.159	.154	.147	.139	.130	.120	.109	.097	.085	.072	.058	.044	.029	.015	65
60	.205	.204	.201	.198	.192	.185	.177	.168	.157	.145	.131	.117	.102	.086	.070	.053	.036	.018	60
55	.241	.240	.237	.232	.226	.218	.208	.197	.184	.170	.154	.138	.120	.102	.082	.062	.042	.021	55
50	.278	.277	.274	.269	.261	.251	.240	.227	.212	.196	.178	.159	.139	.117	.095	.072	.048	.024	50
45	.317	.316	.312	.306	.297	.286	.273	.258	.241	.223	.202	.181	.158	.134	.109	.083	.055	.028	45
40	.357	.356	.351	.344	.334	.321	.307	.290	.270	.250	.228	.204	.179	.151	.123	.094	.063	.032	40
35	.400	.398	.393	.384	.373	.358	.341	.322	.301	.279	.254	.228	.200	.170	.139	.106	.071	.036	35
30	.446	.443	.437	.427	.413	.396	.377	.356	.332	.308	.281	.253	.222	.190	.155	.119	.081	.041	30
25	.495	.492	.484	.471	.455	.435	.414	.391	.365	.338	.309	.279	.246	.211	.174	.134	.091	.046	25
20	.548	.545	.534	.518	.498	.476	.452	.426	.398	.369	.339	.307	.272	.235	.195	.151	.104	.053	20
15	.609	.604	.589	.568	.544	.517	.490	.461	.432	.401	.369	.336	.300	.262	.219	.173	.121	.062	15
10	.681	.672	.649	.620	.590	.559	.528	.497	.466	.434	.401	.367	.330	.291	.248	.200	.143	.076	10
5	.775	.752	.713	.673	.635	.600	.566	.532	.500	.467	.433	.399	.363	.324	.282	.234	.177	.102	5
0	1.000	.841	.774	.723	.679	.639	.602	.567	.533	.500	.467	.433	.398	.361	.321	.277	.226	.159	0

II.
Preceding Table Enlarged for the Spaces Surrounding Infinite Points.

x (for longitudes in upper line). y (for longitudes in lower line).

LAT.	0° 90	1° 89	2° 88	3° 87	4° 86	5° 85	6° 84	8° 82	10° 80	12½° 77½	15° 75		75° 15	77½° 12½	80° 10	82° 8	84° 6	85° 5	86° 4	87° 3	88° 2	89° 1	LAT.
15°	.609	.609	.608	.607	.606	.604	.602	.596	.589	.579	.568		.173	.147	.121	.098	.074	.062	.050	.038	.025	.013	15°
12½	.643	.643	.642	.641	.639	.636	.634	.627	.618	.606	.594		.185	.159	.131	.107	.082	.069	.055	.042	.028	.014	12½
10	.681	.681	.680	.678	.675	.672	.668	.659	.649	.635	.620		.200	.173	.143	.118	.091	.076	.062	.047	.031	.016	10
8	.715	.714	.713	.710	.706	.702	.697	.686	.674	.658	.641		.213	.185	.155	.129	.100	.085	.069	.052	.035	.018	8
6	.753	.752	.750	.746	.741	.735	.728	.714	.700	.681	.662		.227	.199	.169	.142	.112	.095	.078	.060	.040	.020	6
5	.775	.774	.770	.765	.759	.752	.745	.729	.713	.692	.673		.234	.207	.177	.150	.119	.102	.084	.065	.044	.022	5
4	.798	.797	.793	.786	.779	.770	.761	.743	.725	.704	.683		.242	.215	.185	.158	.128	.110	.092	.071	.049	.025	4
3	.825	.823	.817	.808	.798	.788	.778	.757	.738	.715	.693		.250	.224	.194	.168	.137	.120	.101	.079	.055	.029	3
2	.857	.853	.843	.831	.819	.806	.794	.772	.750	.726	.703		.259	.233	.204	.178	.148	.131	.112	.090	.065	.035	2
1	.899	.889	.872	.854	.839	.824	.810	.785	.763	.737	.713		.268	.243	.215	.190	.161	.144	.126	.105	.079	.046	1
0	1.000	.929	.899	.877	.857	.841	.825	.798	.774	.747	.723		.277	.253	.226	.202	.175	.159	.143	.123	.101	.071	0

Note on the Theory of the Economy of Research

P 160: Coast Survey Report 1876, *197–201*

When a research is of a quantitative nature, the progress of it is marked by the diminution of the probable error. The results of non-quantitative researches also have an inexactitude or indeterminacy which is analogous to the probable error of quantitative determinations. To this inexactitude, although it be not numerically expressed, the term "probable error" may be conveniently extended.

The doctrine of Economy, in general, treats of the relations between utility and cost. That branch of it which relates to research considers the relations between the utility and the cost of diminishing the probable error of our knowledge. Its main problem is how with a given expenditure of money, time, and energy, to obtain the most valuable addition to our knowledge.

Let r denote the probable error of any result; and write $s = \frac{1}{r}$. Let $\mathrm{U}r \cdot \mathrm{d}r$ denote the infinitesimal utility of any infinitesimal diminution, $\mathrm{d}r$, of r. Let $\mathrm{V}s \cdot \mathrm{d}s$ denote the infinitesimal cost of any infinitesimal increase, $\mathrm{d}s$, of s. The letters U and V are here used as functional symbols. Let subscript letters be attached to r, s, U, and V, to distinguish the different problems into which investigations are made. Then the total cost of any series of researches will be

$$\Sigma_i \int \mathrm{V}_i s_i \cdot \mathrm{d}s_i;$$

and their total utility will be

$$\Sigma_i \int \mathrm{U}_i r_i \cdot \mathrm{d}r_i.$$

The problem will be to make the second expression a maximum by varying the inferior limits of its integrations, on the condition that the first expression remains of constant value.

The functions U and V will be different for different researches. Let us consider their general and usual properties.

And first as to the relation between the exactitude of knowledge and its utility. The utility of knowledge consists in its capability of being combined with other knowledge so as to enable us to calculate how we should act. If the knowledge is uncertain we are obliged to do more than is really necessary in order to cover this uncertainty. And thus the utility of any increase of knowledge is measured by the amount of wasted effort it saves us, multiplied by the specific cost of that species of effort. Now we know from the theory of errors that the uncertainty in the calculated amount of effort necessary to be put forth, may be represented by an expression of the form

$$c\sqrt{a+r^2},$$

where a and c are constants. And, therefore, the differential coëfficient of this multiplied by the specific cost of the effort in question, say $\frac{h}{c}$, gives

$$\mathrm{U}r = h\frac{r}{\sqrt{a+r^2}}.$$

When a is very small compared with r this becomes nearly constant, and in the reverse case it is nearly proportional to r. An analogous proposition must hold for non-quantitative research.

Let us next consider the relation between the exactitude of a result and the cost of attaining it. When we increase our exactitude by multiplying observations, the different observations being independent of one another as to their cost, we know from the theory of errors that $\int \mathrm{V}s \cdot ds$ is proportional to s^2 and that consequently $\mathrm{V}s$ is proportional to s. If the costs of the different observations are not independent (which usually happens) the cost will not increase so fast relatively to the accuracy; but if the errors of the observations are not independent (which also usually happens) the cost will increase faster relatively to the accuracy; and these two perturbing influences may be supposed, in the long run, to balance one another. We may, therefore, take $\mathrm{V}s = ks$, where k represents the specific cost of the investigation.

We thus see that when an investigation is commenced, after the initial expenses are once paid, at little cost we improve our knowledge, and improvement then is especially valuable; but, as the investigation goes on, additions to our knowledge cost more and more and at the same time are of less and less worth. Thus, when Chemistry sprang into being, Dr. Wollaston with a few test tubes and phials on a tea-tray, was able to make new discoveries of the greatest moment.

In our day, a thousand chemists, with the most elaborate appliances, are not able to reach results which are comparable, in interest, with those early ones. All the sciences exhibit the same phenomenon; and so does the course of life. At first, we learn very easily, and the interest of experience is very great; but it becomes harder and harder and less and less worth while, until we are glad to have done with life.

Let us now apply the expressions obtained for Ur and Vs to the economic problem of research. The question is, having certain means at our disposal, to which of two studies they should be applied. The general answer is that we should study that problem for which the economic urgency, or the ratio of the utility to the cost

$$\frac{\mathrm{U}r\cdot dr}{\mathrm{V}s\cdot ds} = r^2\frac{\mathrm{U}r}{\mathrm{V}s} = \frac{h}{k}\cdot\frac{r^4}{\sqrt{a+r^2}}$$

is a maximum. When the investigation has been carried to a certain point this fraction will be reduced to the same value which it has for another research, and the two must then be carried on together, until finally we shall be carrying on at once researches into a great number of questions, with such relative energies as to keep the urgency-fraction of equal values for all of them. When new and promising problems arise, they should receive our attention to the exclusion of the old ones, until their urgency becomes no greater than that of others. It will be remarked that our ignorance of a question, is a consideration which has between three and four times the economic importance of either the specific value of the solution or the specific cost of the investigation, in deciding upon its urgency.

In order to solve an economical problem, we may use as variables

$$x = \int \mathrm{V}s\cdot ds,$$

or the total cost of an inquiry, and

$$y = \frac{\mathrm{U}r\cdot dr}{\mathrm{V}s\cdot ds},$$

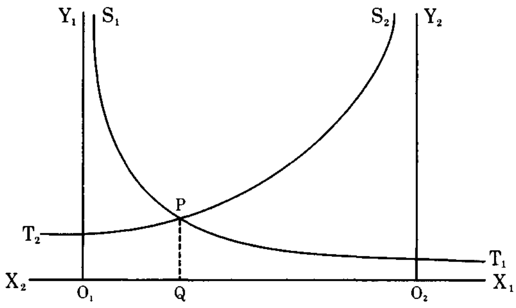
or the economic urgency. Then, C being the total amount we have to spend in certain researches, our equations will be

$$\mathrm{C} = x_1 + x_2 + x_3 + \text{etc.}$$

$$y_1 = y_2 = y_3 = \text{etc.}$$

Then, expressing each y in terms of x, we shall have as many equations as unknown quantities.

When we have to choose between two researches only, the solution may be represented graphically, as follows:



From any point O_1 taken as an origin, draw the axis of abscissas O_1X_1, along which x_1, the total cost of the first investigation, is to be measured. Draw also the axis of ordinates O_1Y_1, along which y_1, the economic urgency of the first investigation, is to be measured. Draw the curve S_1T_1 to represent the relations of x_1 and y_1. Take, on the axis O_1X_1, a point O_2 such that O_1O_2 shall measure the total cost of the two investigations. Let x_2, the total cost of the second investigation, be measured on the same axis as x_1, but in the opposite direction. From O_2 draw the axis of ordinates O_2Y_2 parallel to O_1Y_1, and measure y_2, the economic urgency of the second investigation, along this axis. Draw the curve S_2T_2 to represent the relations of x_2 and y_2. Then, the two curves S_1T_1 and S_2T_2 will generally cut one another at one point, and only one, between the axes O_1Y_1 and O_2Y_2. From this point, say P, draw the ordinate P Q, and the abscissas O_1Q and O_2Q will measure the amounts which ought to be expended on the two inquiries.

According to the usual values of U and V, we shall have

$$y = \frac{1}{4} \cdot \frac{hk}{x\sqrt{ax^2 + \frac{1}{2}kx}}.$$

In this case, when there are two inquiries, the equation to determine x_1 will be a biquadratic. Two of its roots will be imaginary, one will give a negative value of either x_1 or x_2, and the fourth, which is the significant one, will give positive values of both.

Let us now consider the economic relations of different researches to one another, 1st, as alternative methods of reaching the same result, and 2nd, as contributing different premises to the same argument.

Suppose we have two different methods of determining the same quantity. Each of these methods is supposed to have an accidental probable error and a constant probable error so that the probable errors as derived from n observations in the two ways are

$$r_1 = \sqrt{R_1^2 + \frac{\rho_1^2}{n}} \quad \text{and} \quad r_2 = \sqrt{R_2^2 + \frac{\rho_2^2}{n}}.$$

The probable error of their weighted mean is

$$\frac{1}{\sqrt{\frac{1}{r_1^2} + \frac{1}{r_2^2}}},$$

if their constant probable errors are known. The sole utility of any observation of either is to reduce the error of the weighted mean; hence,

$$Ur_1 = D_{r_1}(r_1^{-2} + r_2^{-2})^{-\frac{1}{2}} = (r_1^{-2} + r_2^{-2})^{-\frac{3}{2}} r_1^{-3}.$$

And as the cost is proportional to the number of observations

$$Vs_1 = k_1 \frac{1}{D_{n_1} s_1} = \frac{k_1}{D_{n_1}(R_1^2 + \rho_1^2 n_1^{-1})^{-\frac{1}{2}}} = \frac{2k_1 r_1^3 n_1^2}{\rho_1^2}.$$

Hence the urgency is (omitting a factor common to the values for the two methods)

$$r_1^2 \frac{U r_1}{V s_1} = \frac{1}{k_1 \rho_1^2 \left(1 + n_1 \frac{R_1^2}{\rho_1^2}\right)^2}.$$

And as the urgency of the two methods ought to be the same at the conclusion of the work we should have

$$\sqrt{k_1} \cdot \rho_1 \left(1 + n_1 \frac{R_1^2}{\rho_1^2}\right) = \sqrt{k_2} \cdot \rho_2 \left(1 + n_2 \frac{R_2^2}{\rho_2^2}\right),$$

which equation serves to determine the relative values of n_1 and n_2. We again perceive that the cost is the smallest consideration. The method which has the smallest accidental probable error is the one which is to be oftenest used in case only a small number of observations are made, but if a large number are taken the method with the larger accidental probable error is to be oftenest used, unless it has so much greater a probable constant error as to countervail this consideration. If one of the two methods has only $\frac{1}{p}$th the accidental probable error of the other, but costs p^2 times as much, the rule should be to make the total cost of the two methods inversely proportional to the squares of their constant errors.

Let us now consider the case in which two quantities x_1 and x_2 are observed, the knowledge of which serves only to determine a certain function of them, y. In this case, the probable error of y is

$$\sqrt{\mathrm{D}_{x_1} y \cdot r_1^2 + \mathrm{D}_{x_2} y \cdot r_2^2},$$

and we shall have

$$\mathrm{U}r_1 = 2r_1 \frac{\mathrm{d}y}{\mathrm{d}x_1}.$$

$\mathrm{V}s_1$ will have the same value as before; but neglecting now the constant error, we may write

$$\mathrm{V}s_1 = 2k_1 \rho_1 n_1^{\frac{1}{2}}.$$

Then the urgency (with omission of the common factor) is

$$\frac{\rho_1^2}{k_1 n_1^2} \cdot \frac{\mathrm{d}y}{\mathrm{d}x_1},$$

and as the two urgencies must be equal, we have

$$\frac{n_1}{n_2} = \frac{\rho_1}{\rho_2}\sqrt{\frac{k_2}{k_1}}\sqrt{\frac{\frac{\mathrm{d}y}{\mathrm{d}x_1}}{\frac{\mathrm{d}y}{\mathrm{d}x_2}}}.$$

The following is an example of the practical application of the theory of economy in research: Given a certain amount of time, which is to be expended in swinging a reversible pendulum, how much should be devoted to experiments with the heavy end up, and how much to those with the heavy end down?

Let T_d be the period of oscillation with heavy end down, T_u the same with heavy end up. Let h_d and h_u be the distances of the centre of mass from the points of support of the pendulum in the two positions. Then the object of the experiments is to ascertain a quantity proportional to

$$h_d \mathrm{T}_d - h_u \mathrm{T}_u.$$

Accordingly, if dT_d and dT_u are the probable errors of T_d and T_u, that of the quantity sought will be

$$\sqrt{h_d^2 (\mathrm{d}\ \mathrm{T}_d^2) + h_u^2 (\mathrm{d}\ \mathrm{T}_u^2)}.$$

We will suppose that it has been ascertained, by experiment, that the whole duration of the swinging being C, and the excess of the duration of the swinging with heavy end down over that with heavy end up being x, the probable errors of the results are

$$dT_d = \sqrt{a + \left(b + \frac{c}{h_d^2}\right)\frac{1}{C+x}}$$

$$dT_u = \sqrt{a + \left(b + \frac{c}{h_u^2}\right)\frac{1}{C-x}}$$

where a, b, and c are constants. Then, the square of the probable error of the quantity sought will be

$$a(h_d^2 + h_u^2) + (b\,h_d^2 + c)\frac{1}{C+x} + (bh_u^2 + c)\frac{1}{C-x}.$$

The differential coëfficient of this relatively to x is

$$-(bh_d^2 + c)\frac{1}{(C+x)^2} + (bh_u^2 + c)\frac{1}{(C-x)^2}.$$

Putting this equal to zero and solving, we find for the only significant root,

$$\frac{x}{C} = \frac{b(h_d^2 + h_u^2) + 2c}{b(h_d^2 - h_u^2)} - \sqrt{\left(\frac{b(h_d^2 + h_u^2) + 2c}{b(h_d^2 - h_u^2)}\right)^2 - 1}.$$

When b vanishes, x reduces to zero, and the pendulum should be swung equally long in the two positions. When c vanishes, as it would if the pendulum experiment were made absolutely free from certain disturbing influences, we have

$$\frac{x}{C} = \frac{h_d - h_u}{h_d + h_u},$$

so that the duration of an experiment ought to be proportional to the distance of the centre of mass from the point of support. This would be effected by beginning and ending the experiments in the two positions with the same amplitudes of oscillation.

It is to be remarked that the theory here given rests on the supposition that the object of the investigation is the ascertainment of truth. When an investigation is made for the purpose of attaining personal distinction, the economics of the problem are entirely different. But that seems to be well enough understood by those engaged in that sort of investigation.

Measurements of Gravity at Initial Stations in America and Europe

P 161: **Coast Survey Report 1876, *202–337, 410–16***

UNITED STATES COAST AND GEODETIC SURVEY,
ALLEGHENY, PA., *December* 13, 1878.

C. P. PATTERSON,
Superintendent United States Coast and Geodetic Survey,
Washington, D. C.

DEAR SIR: I present herewith the first part of my report on the measurement of gravity at initial stations of Europe and America. I here describe the methods employed and communicate the main results of the research. The discussion of the amount and nature of the errors, of the comparison of the present results with those deducible from the experiments of other men, and of the resulting figure of the earth, together with some other matters, are postponed for a subsequent report.

The acceleration of gravity is one of those quantities which it is the business of a geodetic survey to measure. So it has always been considered; and it is usage which fixes the meaning of the word "survey" in its geodetical sense. The geodesist is expected to do more than make a map of the country. He not only determines, for instance, the declination of the magnetic needle, which may be laid down on the chart, but also the other magnetic constants which cannot be so laid down. Were he to omit to determine the total force of magnetism, he would be held by all scientific men to have neglected a part of his duty. Now, in the same relation in which this constant stands to magnetical declination and inclination, just so stands the acceleration of gravity to the latitude and longitude; and by as much more as the latitude and longitude are essential to a survey relatively to the direction of magnetism, by so much more is the measurement of gravity indispensible in comparison with that of the magnetic force. The very first duty of the geodesist—paramount even to the drawing of a map—is the study of the figure of the earth; and an operation of surveying in which this problem was left out of view would neither merit nor receive the name of geodetical work. But it was the variation of gravity with the latitude which first proved the earth's ellipticity; and it may very well yet turn out that this method is the best way of determining it. At all events, the study of local variations of vertical attraction will find an application in the measurement of the level surface of the earth by triangulation. It is, also, quite certain that the solution of some high problems of geology must be facilitated by the integrated soundings which the pendulum virtually makes of the earth's interior.

While the absolute amount of the acceleration of gravity is, no doubt, a geodetical constant necessary to be determined, a very precise knowledge of it can, in the present state of science, find no practical nor theoretical application. What is chiefly of importance is the relative gravity at different places and times. This is also a quantity far easier to measure. To determine the acceleration absolutely, we must accurately measure both an interval of time and a length; to determine it relatively, we have only to carry the same rigid piece of metal from place to place and determine the duration of some phenomenon in which gravity is chiefly concerned. Moreover, we can fix in some measure the probable error of relative determinations. Most of the conditions of the experiments other than the amount of gravity itself are alike at the different stations. If they were precisely so, no constant errors could affect the relative result except in the second order of magnitudes. Now the accidental errors of observations, the only ones which would remain, can readily be determined by the method of least squares. It is not quite true that no conditions other than gravity vary from station to station. The temperature, for example, varies; and in such a manner that an erroneous coëfficient of expansion will produce errors in the relative gravity of stations near the poles and near the equator in a constant direction and similar amount; and so will slightly affect the deduced compression of the earth. So an error in the coëfficient of atmospheric effect will produce a constantly similar error in the relative gravity of an elevated and a depressed station, and may thus lead to an extremely erroneous value of the absolute modulus of gravitation and of the mean density of the earth. There are, also, various conditions relating to the installation of the instruments which are different at different stations, and which give rise to errors which least squares will fail to detect. Such errors are, however, slight in comparison with those which may affect absolute determinations of gravity, into which the constant errors enter to their full amount. A source of error affecting all modern determinations was lately pointed out by the writer of this paper, which had produced errors in the accepted results amounting to one four or five thousandth part of the quantity measured, and in some cases even to more.

The value of gravity-determinations depends upon their being bound together, each with all the others which have been made anywhere upon the earth. In considering how the necessary connections should be made for our work, it seemed to you, sir, and to Prof. Benjamin Peirce, the consulting geometer, as it did to the writer, that to trust to absolute determinations and to the transportation of metres would be more than hazardous, notwithstanding that such had been the recent practice in continental Europe. Your instructions were accordingly issued for the oscillation of the same pendulum at those fundamental stations of Europe where the chief absolute determinations had been made and whence pendulum-expeditions had set out, and at a station in America which would become the initial one for this continent. Similar action followed on the part of the European surveys; for at the meeting of the International Geodetic Congress in Paris, in 1875, it was resolved, at the suggestion of the writer, that the different states should carry their pendulums to Berlin and swing them in the Eichungsamt there. This has already been done by Switzerland and Austria, and will be done hereafter by every survey which is not willing to sacrifice the solution of a great problem to forms of action based on national exclusiveness. Geodesy is the one science the successful prosecution of which absolutely depends upon international solidarity.

STATIONS.

The stations occupied were as follows:

1. *Geneva.*—The pendulum was swung in the observatory, nearly in the same spot, and on the same wooden stand (see illustration No. 26) used for the purpose by Professor Plantamour, whose advice in regard to the conduct of the experiments was invaluable. His pendulum was set up at the east end and ours at the west end of the main hall. The floor of this hall is (as I remember it) not a metre above the level of the ground which, according to Dufour's map, is 407 metres above the level of the sea. The experiments here were made in

August and September, 1875. The station must be pronounced unfavorable for accurate pendulum-work, both from its exposure to changes of temperature and from the slight stability of the asphalt floor and of the tripod.

After the experiments at Geneva the pendulum was injudiciously intrusted to a company in Plainpalais, of whom a vacuum chamber had been ordered. It suffered grave injury in consequence, and was repaired by MM. Brunner, in Paris. In this way the operations at Geneva are completely separated from those at the other stations, and are deprived of much of their value.

2. *Paris.*—M. Wallon, minister of worship and public instruction, authorized pendulum-experiments at the Observatory of Paris. The observations were made in the Hall of the Meridian, in the alcove at the north end. The centre of the pendulum-stand was 89 cm east of the meridian-mark, and opposite the reading 2738 cm on the meridian-mark. The pendulum-stand stood directly on the floor, and the centre of gyration of the pendulum was 29 cm above the floor. A pendulum-stand, believed to be that of Biot, is by measurement 735 cm east of the meridian, and opposite 450 cm on the meridian-mark. Its fulcrum is 171 cm above the floor. On this subject, M. C. Wolf, astronomer at the observatory, to whose politeness throughout the occupation of the station the writer is much indebted, kindly communicates the following details:

"Borda a opéré dans une salle et contre un mur qui n'existent plus; la hauteur du sol de cette salle au-dessus du niveau de la mer est 67 mètres. L'annuaire du bureau des longitudes donne 65 mètres.

"J'ai pris sur un ancien plan de l'observatoire la position du mur de Borda: son centre était à 14.72 toises (28.69 mètres) de la ligne de la méridienne, à l'est, et au sud à $9^{t}89$ ($19^{m}28$) du puits de l'observatoire, dont l'axe est sur la méridienne.

"Les coordonnées de la station de Biot par rapport à la même ligne et au même puits sont:

Distance à la méridienne	$7^{m}34$ est
" au puits suivant la méridienne	$10^{m}23$ sud

"Vous étiez placé vous-même presque sur la méridienne, à moins d'un mètre à l'est, et à $12^{m}70$ du puits vers le nord.

"La hauteur des deux dernières stations [no doubt the floor is meant] au-dessus de celle de Borda est $7^{m}05$, par conséquent $74^{m}05$ au-dessus du niveau de la mer."

The level of the ground in the middle of the south face of the observatory above that of the sea is, according to the general staff, 58.8 metres. M. Biot gives as the elevation of his station above the sea 70.25 metres. He erroneously states that Borda's was at the same level. De Freycinet gives 72.28 metres as the altitude of his station. Our experiments at Paris were made during the months of January and February, 1876.

The station at Paris was favorable in regard to the uniformity of temperature, but unfavorable from the excessive instability of the floor.

3. *Berlin.*—The pendulum was swung in Berlin in the large comparison-room of the Imperial Eichungsamt in the garden of the observatory. Plans of this building have been promised by the director, Professor Förster, for this report; but as they have not yet arrived the precise point occupied will be stated in an appendix. The station was very near that of Bessel, but about three metres higher. The experiments were made from April 15 to June 12, 1876. This station was favorable in regard to stillness and stability, but unfavorable owing to changes of temperature. The writer here enjoyed the inestimable advantage of the counsel of the Nestor of geodesy, General Baeyer, and also that of great interest in the experiments and attention to everything which could affect the success of them on the part of Professor Förster.

4. *Kew.*—In England, the pendulum was swung at the Kew Observatory in the old deer-park at Richmond, Surrey. The observatory is a meteorological station kept up by a committee of the Royal Society, but is apparently as fundamental a station as there is available in England. The ground is 24 feet above the level of the sea and our pendulum was nearly at the level of the ground. The experiments were made in July, 1876. It proved an excellent place both for steadiness of temperature and for stability. Fortunately, the director of the observatory, Mr. Whipple, thoroughly understands the art of oscillating the pendulum, and was most obliging in furthering the investigation in many ways.

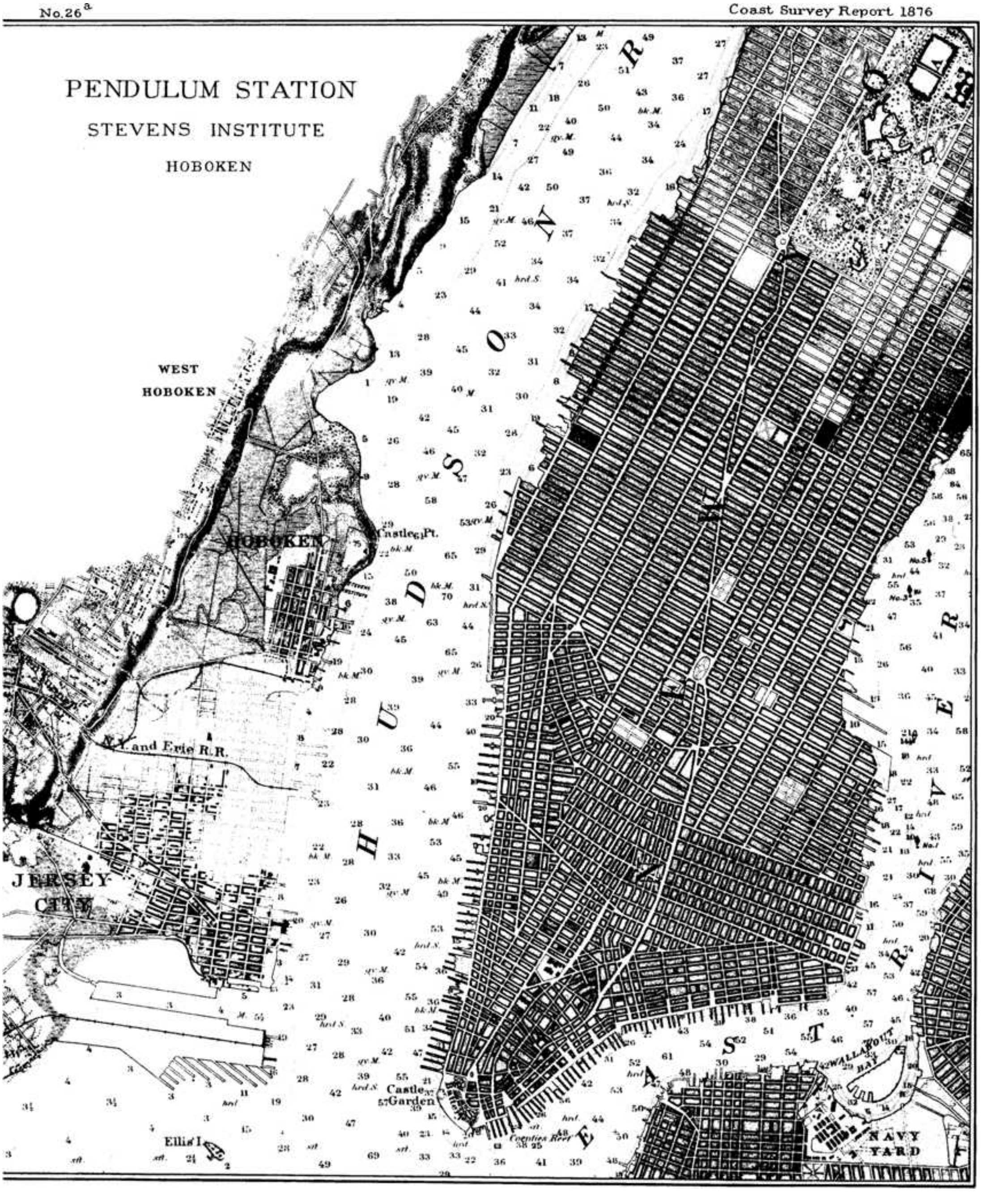
5. *Hoboken.*—The pendulum was swung in a dark chamber in the cellar of the Stevens Institute of Technology. Notwithstanding the kindness of the authorities of the institute in permitting and facilitating the experiments in various ways, and the advantage of the counsels of the eminent physicists resident there, especially those of my friend Professor A. M. Mayer, this station is objectionable from its being situated in a private institution. Otherwise, however, it is a suitable place, except that it is impossible to measure the length of the pendulum there with any accuracy owing to effects of tempera-

ture. The latitude of the station is 40°44′.5, the longitude is 74°02′ west, and the height above the mean sea-level is about 10 metres. The position in reference to the harbor of New York is shown in the illustration No. 26*a*.

INSTRUMENTS.

The chief instrument was a Bessel reversible pendulum of one metre length between the knife-edges, admirably constructed by Messrs. Repsold, and nearly an exact copy of the Prussian instrument described by Bruhns in his account of Dr. Albrecht's experiments. One-half this pendulum is shown in illustration No. 27. Its mass is 6308 grammes. The dimensions of its principal parts are as follows:

	Centimetres.
Height of cone at end	0.5
Length of little cylinder	1.2
Diameter of little cylinder	1.0
Diameter of collar outside bob	4.9
Height of collar	0.9
Diameter of bob, heavy	11.48
Diameter of bob, light	11.42
Height of bob, heavy	3.25
Height of bob, light	3.18
Diameter of collar below bob	4.78
Height of collar below bob	2.4
Diameter of stem	4.33
Distance (nearest) bob to bob	115.25
Length of knife	9.55
Height of knife	1.8
Thickness of knife	1.4
Height from bottom of brass oblong to top of knife	3.4
Thickness of brass piece	1.32
Height of tops of thumb-screws above top of knife	2.05
Breadth of brass for screws	2.35
Length of upper projection on stem below knife	1.4
Diameter of upper projection on stem below knife	5.0
Length of lower projection on stem below knife	1.4
Diameter of lower projection on stem below knife	5.0
Length of hole for tongue	7.5
Breadth	2.6
Thickness of metal	0.18

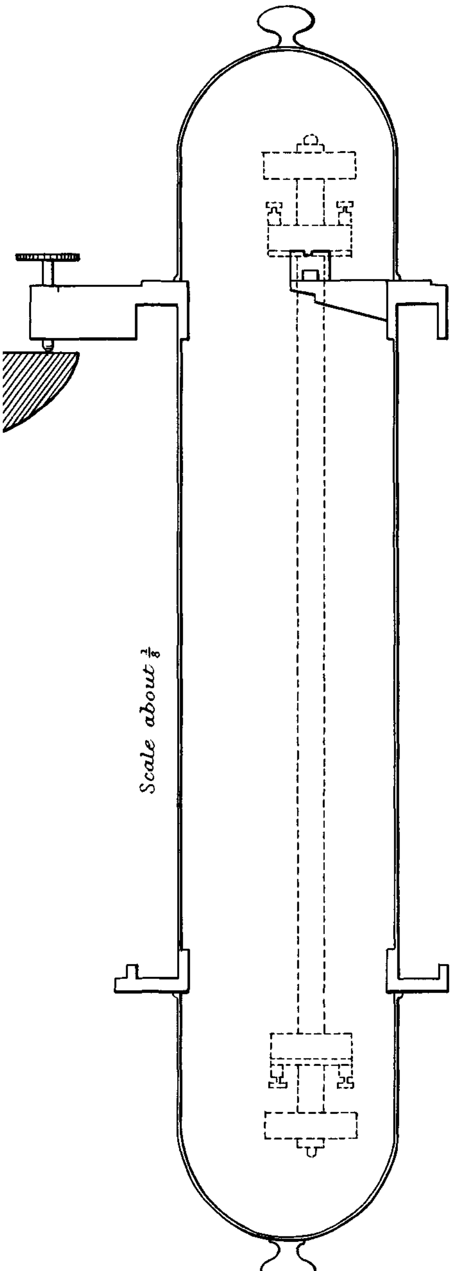

No. 26[a]
Coast Survey Report 1876
PENDULUM STATION
STEVENS INSTITUTE
HOBOKEN
WEST
HOBOKEN
HOBOKEN
Castle Pt.
N.Y. and Erie R.R.
JERSEY
CITY
HUDSON RIVER
EAST RIVER
Castle Garden
Ellis I.
Coenties Reef
WALLABOUT BAY
NAVY YARD

In using the vertical comparator which forms a part of the instrument, the intention of the makers seems to have been that the pendulum-metre should be set vertical by means of a spirit-level on a brass straddle provided for the purpose. Instead of this, a plumb-line has been used. This is so made as to be capable of movement in any horizontal direction. The point of support can also be raised or lowered and the whole can be rotated on a vertical axis. In this way, glass scales may be observed, which form part of the plumb; and any error in their verticality is eliminated by reversal. The instrument is first approximately adjusted; the axis of rotation of the comparator is made accurately vertical and the upper microscope is focused on the knife-edge. Then the vertical wire of the lower microscope is made to bisect the plumb-line and the upper microscope is turned about the vertical axis until it also bisects the same vertical line. Afterward, the plumb-line having been brought into the focus of the upper microscope, the lower one is advanced or retracted until it is in focus on the plumb-line below. The microscopes having been so adjusted the metre is adjusted by means of them.

A separate pendulum-support, with a vacuum chamber, constructed under my direction by the Plainpalais company, and called the Geneva support, was used at Hoboken. A vertical section of this support (suppressing various details) is shown on illustration No. 28. The supporting part consists essentially of a solid brass ring with three projections for screw-feet and a tongue to receive the knife-edge cast in one piece. The vacuum chamber is a metallic cylinder covered with bell-glasses at the two ends.[1] Each screw-foot is furnished with two powerful binding-screws. Only that part of the tongue below the level of the upper surface of the brass ring is a part of the same casting. The upper part is fixed by screws. The instrument is provided with apparatus for raising the pendulum off the knife-edges and letting it down again, another for setting it in motion, supports for thermometers, graduated arc, &c. The graduated arc is divided into thousandths of the radius. Messrs. Stackpole and Brothers, of New York, have made this troublesome graduation with extreme accuracy, upon the arc now in use, and have generously presented it to the survey.

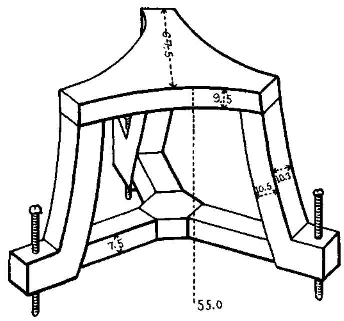
1. The leakage of the chamber increased the pressure by about that of a tenth of a millimetre of mercury per hour.

No. 28



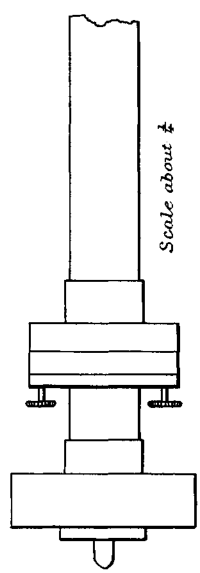
Geneva Pendulum Support.

No. 26



Geneva wooden stand.
(measures in centimeters.)

No. 27



Bessel Reversible Pendulum.

Various other instruments were used which will be described in giving an account of the observations made with them.

OBSERVATIONS OF THE DURATION OF AN OSCILLATION.

The duration of oscillation was ascertained by chronographing 100 transits of the point of the pendulum over the vertical wire of a reading-telescope. Equal numbers of transits were taken from right to left and from left to right, so that any effect from the wire not being at the equilibrium-point was eliminated. At least two seconds elapsed between successive records. The time-keeper was generally a chronometer breaking every two seconds. The chronometer-breaks and signals of pendulum-transits were recorded by the same pen, and interferences were avoided by choosing among the following four methods:

A. 25 transits to the right; 50 to the left; 25 to the right.
B. 50 transits to the left; 50 to the right.
C. 25 transits to the left; 50 to the right; 25 to the left.
D. 50 transits to the right; 50 to the left.

The chronograph is a fillet instrument regulated by a reed and constructed by Breguet. It has three pens. The fillets have been measured to tenths of seconds and the hundredths have been estimated, except in the second readings of the Berlin fillets, where the hundredths were measured with a scale devised and constructed for the purpose.

Observations of the transits were taken when the pendulum reached an arc of oscillation of 2°, $1\frac{1}{2}°$, 1°, and $\frac{1}{2}°$, on each side of the vertical. The object of the intermediate transits at $1\frac{1}{2}°$ and 1° will be seen in the second part of the report, which treats of the errors of the results. By taking the transits at fixed arcs, the condition that the pendulum should be equally affected by the air with heavy end up and heavy end down was secured with certainty. It has been objected that this plan makes the experiment with heavy end up of too short duration. To remedy this, at Kew the pendulum was swung with heavy end up both before and after every experiment with heavy end down, so that there were twice as many experiments with heavy end up as with heavy end down. The question of the proper arrangement of the experiments in this respect belongs to the theory of the Economy of Research, which is treated in Appendix No. 14.

In order to avoid any possible difference of personal equation in noting the transits when the pendulum was moving rapidly at the beginning and slowly at the end of the experiments, different powers were employed upon the reading-telescope, so that the apparent velocity was about the same at the last as at the first set of transits.

CORRECTIONS.

The observed duration has to receive the following corrections:

1. The correction for the rate of the time-keeper;
2. The correction for amplitude of oscillation;
3. The correction for pressure and temperature of the air;
4. The correction for the expansion of the metal by heat;
5. The correction for the slip of the knives;
6. The correction for the wear of knives;
7. The correction for inequality of knives;
8. The correction for stretching of pendulum by weight of heavy bob when the latter is down;
9. The correction for the flexure of the support;
10. The correction for attractions of sun, moon, and tide;
11. The correction for elevation above sea-level.

*[*206.23 . . . 225.25*]*

CORRECTION FOR ARC.

The factor for reducing the time of oscillation of the pendulum to an infinitesimal arc is best developed according to powers of the arc itself. Such a development is far more convergent than those found in the books. The factor is

$$1-\frac{1}{64}A^2+\frac{1}{49152}A^4-\frac{5}{1179648}A^6+\text{etc.},$$

where A represents the whole amplitude of the oscillation expressed in parts of the radius.

The Repsold pendulum tripod is provided with a metallic arc for reading the amplitude of oscillation. This is divided into spaces of

10′ each. In the experiments on the Geneva support an arc divided into thousandths of the radius was made use of.

At Geneva, the amplitude was read by bringing the vertical wire of the telescope so as to bisect the point of the pendulum at the extremity of an oscillation, the wire having been turned in a direction radial from the line of the knife-edge. The time was noted and the position of the wire between two lines of the graduated arc was estimated at leisure. At the other stations a far better method was used. The wire was placed in exact coincidence with a line of the graduated arc and the time was noted at which the pendulum was bisected by it at the extremity of its oscillation. The arc was so placed that its zero was 1′ or 2′ away from the vertical, so as to permit the observation to be made both to the right and the left.

The Geneva observations of arc were plotted on a curve for each experiment. Then values of the arc were read off at six equal intervals between every two sets of pendulum-transits. These values were squared, and the mean square was obtained by Mr. Weddle's rule—

$$\int_0^{6h} u_x \cdot \mathrm{d}x = \frac{3h}{10}\left\{u_0 + u_2 + u_4 + u_6 + 5(u_1 + u_5) + 6u_3\right\}.$$

To obtain the correction for arc at Paris, Berlin, and Kew, the first step was to tabulate the times of decrement from a fixed value of the arc to each of the others for all cases in which there were good observations both to the right and to the left. The following tables show—first, the minute and second at which the pendulum was observed to reach each amplitude, and, second, the differences of the times from those of reaching the arc of 1°10′ on each side of the vertical.

[226.10 . . . 230.28]

The foregoing tables show the amount of discrepancy between the observations of different days. The point of the pendulum is distant $113\frac{1}{2}$ cm from the knife-edge, so that one minute of arc measures $\frac{1}{3}$ of a millimetre. The reading-telescope was placed at a distance of about 3 metres. It may, therefore, be supposed that a single observation of the half-amplitude would be in error by something like $\frac{1}{4}$ of a minute. The following table shows how much error this would produce in the noted time of attaining the different amplitudes:

Half-amplitude.		Time of decrement of $\frac{1}{4}'$. Heavy end up.	Heavy end down.
°	′	*s.*	*s.*
2	10	2	5
1	50	3	7
1	40	3	8
1	20	5	11
1	10	6	13
	50	9	21
	40	12	27

It will be seen that the observed discrepancies are several times as large as these values, and cannot therefore well be attributed to errors of observation. The daily discrepancies are, however, less than 7 times the numbers just given, that is less than $1\frac{3}{4}$ minutes. Such errors would produce an error in the correction for arc proportional to the arc itself and amounting to only 2 millionths for $\Phi = 2°$. It has, therefore, been judged proper to find one function to express the relation between the amplitude and the rate of decrement, and to apply this to all the observations at ordinary pressures and temperatures for the purpose of finding the correction for arc.

The pendulum being symmetrical in form in reference to its two knife-edges, the air resists its motion with the same force whichever end is up. Consequently, the rate of decrement of the arc (produced by this cause) is in the two positions inversely proportional to the moments of inertia. But it is the property of the reversible pendulum that the moments of inertia about its two knife-edges are proportional to the distances of the centre of mass from those knife-edges. These distances are in our pendulum very nearly in the ratio of 3 to 7. Hence, the times of decrement of the arc (so far as it is the effect of the air) must be in the ratio of 7 with heavy end down to 3 with heavy end up. The same would be true for any proper effect of friction on the knife-edges. But the decrement of the amplitude is no doubt partly caused by the energy of motion of the pendulum itself. For example, the pendulum sets its support in vibration and this vibration is resisted by internal friction, thus exhausting the energy. Such a decrement of the arc will be more nearly equal with heavy end up and with heavy end down, or it may even be greater with heavy end down. In point of fact it will be seen that the times of decrement are a little more nearly equal than if they were in the ratio of the distances of the knife-edges from the centre of mass. This is shown by the following table:

STATION, PARIS.

Decrement.	Time, heavy end down.	Calculated time, heavy end up.	Observed time, heavy end up.	O − C
° ′ ° ′	m.	m.	m.	m.
1 40 to 1 20	12.0	5.2	5.4	+0.2
1 20 1 10	8.9	3.9	3.6	−0.3
1 10 50	21.4	9.3	9.9	+0.6
50 40	15.4	6.7	6.9	+0.2
1 40 to 40	57.7	25.2	25.8	+0.6

STATION, BERLIN.

° ′ ° ′	m.	m.	m.	m.
1 40 to 1 20	12.4	5.4	5.5	+0.1
1 20 1 10	7.8	3.4	3.6	+0.2
1 10 50	22.1	9.6	9.6	0.0
50 40	15.6	6.8	6.9	+0.1
1 40 to 40	57.9	25.2	25.6	+0.4

STATION, KEW.

° ′ ° ′	m.	m.	m.	m.
1 40 to 1 20	12.5	5.5	5.5	0.0
1 20 1 10	8.1	3.5	3.6	+0.1
1 10 50	22.1	9.6	9.8	+0.2
50 40	16.1	7.0	6.9	−0.1
1 40 to 40	58.8	25.6	25.8	+0.2

These numbers, however, show that for the purpose of calculating the correction for arc it will be quite sufficient to assume that the times of decrement are in the ratios of the moments of inertia. In order to obtain the law of decrement, therefore, the times with heavy end up and with heavy end down have been added together; and the means have then been taken for all three stations (Paris, Berlin, and Kew). We thus obtain

Half-amplitude.	Sum of times.
′	s.
130	−2880
110	−2187
100	−1779
80	− 706
70	0
50	+1927
40	+3304

The time for 140′ has been neglected as not having been generally observed with heavy end down.

To satisfy these values a form of equation has been assumed which has been copied from Professor Benjamin Peirce's *Analytic Mechanics,* and which is Coulomb's equation with a constant term added. It is—

$$\mathrm{D}_t \Phi = -a - b\Phi - c\Phi^2.$$

The integral of this equation is

$$\Phi = \sqrt{\frac{a}{c} - \frac{1}{4}\frac{b^2}{c^2}}\cot\left\{c\sqrt{\frac{a}{c} - \frac{1}{4}\frac{b^2}{c^2}}(t - t_0)\right\} - \frac{1}{2}\frac{b}{c}.$$

The values of Φ for the different values of t, as given in the table above, are sufficiently satisfied by putting (for t in seconds of time and Φ in minutes of arc)

$$a = 1547 \times 10$$
$$b = 6418 \times 10^{-8}$$
$$c = 1421 \times 10$$

The errors are shown in the following table:

Sum of times.	Obs. Φ.	Calc. Φ.	(O−C) Φ.
s.	′	′	′
−3191	140	138.82	+1.18
−2880	130	130.03	−0.03
−2187	110	110.33	−0.33
−1779	100	100.00	−0.00
− 706	80	79.97	+0.03
0	70	70.03	−0.03
+1927	50	49.98	+0.02
+3304	40	39.99	+0.01

By least squares, better values of the constants could be obtained; but these are evidently sufficient for our purpose.

The law of decrement of the amplitude having been made out, it was requisite to apply it to the observations. The constant t_0, being different for each experiment, had first to be determined. In doing this, it was desirable to use observations in which the arc had only been noted on the right or on the left. For this purpose it was necessary to calculate the inclination of the zero of the metallic arc to the vertical. This was readily determined from the difference of the time of reaching a given division to the right and to the left. The following tables show the results so obtained.

[232.44 . . . 237]

The inclination of the zero-point having thus been ascertained, the time of each observation of amplitude to the right and to the left was corrected for inclination so as to give the time of reaching an arc on each side of the vertical of an integral number of tens of minutes. The means of the results for right and left were then taken, in cases where observations were made on both sides. A table calculated from the formula was then entered, giving $t - t_0$ for every ten minutes of Φ, and from this the value of t_0 was obtained. The following tables show the result. The *hours* are omitted.

[238.8 . . . 244.3]

It will be seen that the value of t_0, which ought to remain constant for each experiment, frequently undergoes a progressive change. On this account three successive values of the constant have been adopted for each experiment, one from $\Phi = 2°$ to $\Phi = 1\frac{1}{2}°$, a second from $\Phi = 1\frac{1}{2}°$ to $\Phi = 1°$, and a third from $\Phi = 1°$ to $\Phi = \frac{1}{2}°$. Within each of these limits there is in no case any change which could occasion a sensible error in the correction for arc.

The values of t_0 having been obtained, the next step was to get the value of Φ at the mean instant of each set of transits. This was done by subtracting t_0 from the mean time of each set, entering the value of $t - t_0$ in a table, and taking out that of Φ.

The next step was to find the integral $\frac{1}{16}\int \Phi^2 \mathrm{d}t$. The formula, obtained by the integration of the expression for Φ given above, is,

$$\int \Phi^2 \mathrm{d}t = -\frac{1}{c}\Phi - \frac{1}{2}\frac{b}{c^2}\log\sin\left\{\sqrt{ac - \frac{1}{4}b^2}\,(t - t_0)\right\}$$

$$+ \left(\frac{1}{2}\frac{b^2}{c^2} - \frac{a}{c}\right)\left(t - t_0\right).$$

In some of the calculations, these three terms were calculated for the observed values of Φ and $(t - t_0)$. In other cases the integral was taken out of a table constructed for the purpose.

The correction of each interval between the successive sets of transits was separately calculated. The approximate values of the corrections were generally as follows:

Interval.	Correction. Heavy end down.	Correction. Heavy end up.
	s.	*s.*
From $\Phi = 2^\circ$ to $\Phi = 1\frac{1}{2}^\circ$	-0.053	-0.023
From $\Phi = 1\frac{1}{2}^\circ$ to $\Phi = 1^\circ$	-0.043	-0.018
From $\Phi = 1^\circ$ to $\Phi = 1\frac{1}{2}^\circ$	-0.033	-0.014

All the observations of arc at Hoboken, when the Geneva support was employed, were made on a scale divided into decimal parts of the radius. This scale being freely movable, was carefully placed with its zero exactly under the point of the pendulum, when hanging free; so that it was not in general found worth while to observe the arc on more than one side, during the observations taken in June, September, and October—but one allowance for position of zero (0.00043 on September 25) being found necessary in these series. In the observations taken in December and in 1878 less care was taken in placing the scale, and the arc was always observed on both sides, except between 0.024 and 0.010 on the left, when it was hidden from view by one of the supports of the lower platform of the pendulum receiver.

The following tables give the observations of arc in detail, followed by the calculation of inclination, made according to the methods before given.

*[*245.3 . . . 253*]*

The December observations were corrected for inclination by the proper additions to the time of arcs observed on the right side only. For the observations at high temperatures, the observed times on the right side were taken, and the arcs corrected,

In heavy-end-up observations, all days except the first, by	-0.0004
In heavy-end-down observations, May 4, 5, and 6, by	-0.001
In heavy-end-down observations, May 8, by	-0.00035

The differential formula connecting the arc and the time was next found. Only three constants were employed, as has been already stated, the observations not being sufficiently numerous or sufficiently exact to admit of four. The equation, then, is

$$\mathrm{D}_t\varphi = -b\varphi - c\varphi^2.$$

Hence,

$$\varphi = \frac{b}{c}(\text{\textepsilon}^{bt} - 1)^{-1},$$

$$\frac{1}{16}\int \varphi^2 dt = \frac{b}{16c^2}\left\{ bt - \text{Nat log}\,(\text{\textepsilon}^{bt} - 1) - (\text{\textepsilon}^{bt} - 1)^{-1}\right\} + \mathrm{C},$$

$$= \frac{b}{16c^2}\left\{ \text{Nat log}\left(1 + \frac{c}{b}\varphi\right) - \frac{c}{b}\varphi\right\} + \mathrm{C},$$

$$\text{and } t = \frac{1}{b}\,\text{Nat log}\,\left(1 + \frac{b}{c\varphi}\right).$$

The constants b and c, for heavy end up, calculated from the observed times of decrement, but not corrected by least squares, are given in the subjoined table.

From the observations of decrement were deduced, first, an equation of the form

$$b + c\varphi_m = n,$$

φ_m denoting the mean value of φ; and, second, a value of c. As c has been supposed proportional to the density of the air, we find

$$c = 0.03125\,\frac{p}{\tau}.$$

By substituting these values in the equations $b + c\varphi_m = n$, we obtain new values of b, called "second reduction" in the table; the "first reduction" having been obtained with the original values of c. These second values answer to the formula

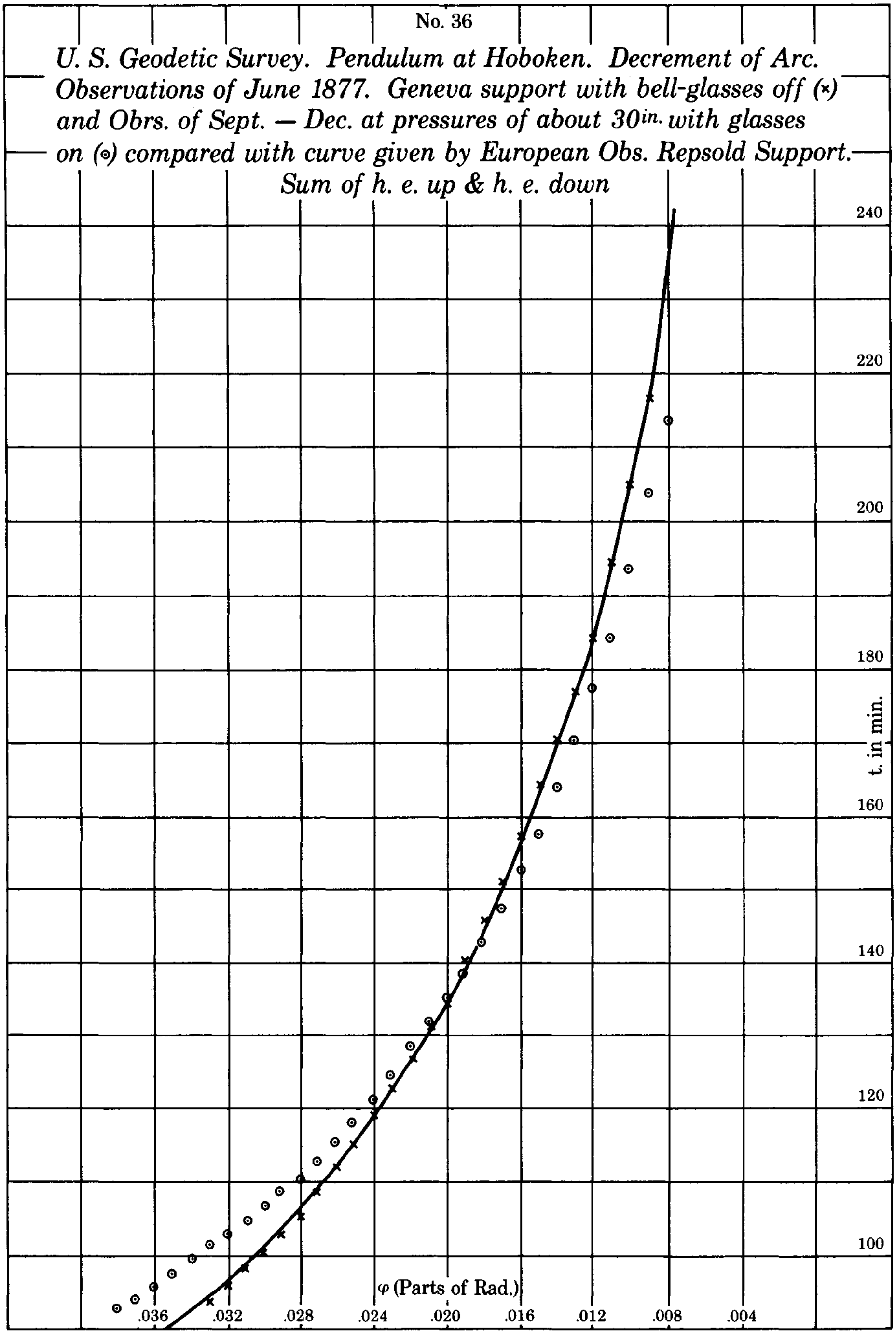
$$b = 0.0013\,\tau^{\frac{3}{4}} + 0.00435\,p^{\frac{1}{2}}\,\tau^{-\frac{1}{8}}.$$

For the observations in which the bell-glasses were on the receiver, these alone being comparable with one another, the values of b, thus calculated, are given in the table, along with the values from the reduction of observations of decrement. Also in illustration No. 36, before alluded to.

[255 . . . 260.32]

REDUCTION TO A VACUUM.

It is not usual to reduce observations with the reversible pendulum to a vacuum, because the formula for combining the results with heavy end up and heavy end down, namely,


No. 36
U. S. Geodetic Survey. Pendulum at Hoboken. Decrement of Arc.
Observations of June 1877. Geneva support with bell-glasses off (×)
and Obrs. of Sept. — Dec. at pressures of about 30in. with glasses
on (⊙) compared with curve given by European Obs. Repsold Support.
Sum of h. e. up & h. e. down
240
220
200
180
t. in min.
160
140
120
100
φ (Parts of Rad.)
.036
.032
.028
.024
.020
.016
.012
.008
.004

$$T = \frac{T_d h_d - T_u h_u}{h_d - h_u},$$

is supposed to eliminate the atmospheric effect; and it really does so if the hollow bob is staunch. Nevertheless, there is a great advantage in reducing to a vacuum, for in that way we may use the reversible pendulum as two invariable pendulums, combining them by the formula

$$T = \frac{T_d h_d + T_u h_u}{h_d + h_u}.$$

In our pendulum $h_d : h_u = 7:3$; hence, the usual formula gives a T which is subject to $\frac{7}{4}$ of the error of T_d and $\frac{3}{4}$ of the error of T_u, while the proposed formula gives a T which is subject to only $\frac{7}{10}$ of the error of T_d and $\frac{3}{10}$ of the error of T_u, so that its error is only $\frac{4}{10}$ of that of the other. It is true that it is somewhat difficult to ascertain the amount of the atmospheric correction which must be applied before the second formula can be used, but it will be shown that it is not impossible. Nor will it be subject to any uncertainty which will be sensible unless under changes of pressure amounting to a considerable fraction of an atmosphere.

But if this is so, it may be asked, why use a reversible pendulum at all? The reply is, that we have thus a continual check upon our work; and also a means of studying knife-edge effects, etc. Moreover, the fatal effect of any accident to the pendulum is thus insured against. Another advantage of the reversible pendulum is that the centre of oscillation can be ascertained with exactitude, its position being a necessary datum for calculating the effect of the atmosphere.

The presence of the air lengthens the period of oscillation of the pendulum in no less than four distinct ways: 1st, by its buoyancy; 2nd, by being carried along within inclosed parts of the pendulum; 3rd, by the hydrodynamic effect of its pressure; and 4th, by its viscosity.

In reckoning the buoyancy of the air, it will make a good deal of difference whether the hollow bob is open or tightly closed. It should be quite staunch; otherwise, the atmospheric effect is not eliminated. If it be so, the air it contains is to be considered as a part of the pendulum; otherwise, not. It is believed that our hollow bob is tight. To ascertain the volume of this air, we have to consider that if the hollow were filled with brass the centre of mass would be at the centre of figure. The existing brass whose centre is at a distance $\frac{1}{2}(h_d - h_u)$ from the centre of figure and the brass which would be required to fill the hollow bob and which would be at the distance, say o (which can be measured), from the centre of figure, would then be in equilibrium about the centre of figure. Hence, their volumes are in the ratio of $\frac{1}{2}(h_d - h_u)$ to o. It is necessary to assume a density for the air in the bob. Then the mass of the pendulum obtained by weighing and corrected for the buoyancy of brass in air must be further increased by the mass of air in the hollow bob, by multiplying it by

$$\left(1 + \frac{\frac{1}{2}(h_d - h_u)}{o}\,\frac{\rho_3}{\rho_1}\right),$$

where ρ_3 is the supposed density of this air and ρ_1 is that of brass; and this corrected mass of the pendulum must be used in all the corrections affecting the inertia. The value of $h_d - h_u$ must also be diminished by the ratio of the mass of air in the bob to the corrected mass of the pendulum multiplied by $(2o + h_d - h_u)$. These corrections being applied we shall have (putting ρ_2 for the density of the circumambient air)

$$T_d = \eth\sqrt{\frac{l}{g}}\cdot\left(1 + \frac{1}{4}\frac{h_d + h_u}{h_d}\left[1 + \left(1 - \frac{\rho_3}{\rho_1}\right)\frac{h_d - h_u}{2o}\right]\frac{\rho_2}{\rho_1}\right),$$

and a similar formula will hold for T_u replacing h_d by h_u in the denominator.

Besides the air in the hollow bob, a large volume is inclosed within the open tube which forms the stem of the pendulum. The volume has to be calculated from the measured dimensions of the tube. Let this volume be σ_2 and let its radius of gyration about the centre of mass be γ_2; then the formula for the period will be, with heavy end down,

$$T_d = \eth\sqrt{\frac{l}{g}}\cdot\left(1 + \frac{\sigma_2\rho_2\left[\gamma_2^2 + \frac{1}{4}(h_d + h_u)^2\right]}{Mlh_d}\right),$$

and with heavy end up the same formula will hold after substituting h_u for h_d in the denominator. The lh_d in the denominator is best replaced by $(\Gamma^2 + h_d^2)$ where Γ is the radius of gyration of the pendulum about its centre of mass. This quantity can be ascertained by an approximate reduction of the times of oscillation to a vacuum.

Thirdly, the ordinary pressure of the atmosphere, due to its weight, makes the pendulum carry air with it, and increases its inertia. This effect was first discovered by du Buat, but particular attention was brought to it by Bessel. It has been subjected to mathematical analysis by Green, who has given the formula for the period of oscillation of any ellipsoid making oscillations, very small as compared with its own dimensions, in an infinite incompressible fluid. The air is sufficiently incompressible under the gentle movement of the pendulum, and the limitation to very small oscillations, though particularly insisted on by Green, is probably immaterial. His formula is as follows:

Let the equation of the ellipsoid be

$$\frac{x^2}{a^2} + \frac{y^2}{b^2} + \frac{z^2}{c^2} = 1$$

and let x be in the direction of the oscillations. Then calculate the quantity

$$\mathrm{F} = \frac{1}{2} abc \int_0^\infty \frac{df}{\sqrt{(a^2 + f)^3 (b^2 + f)(c^2 + f)}}$$

and the effect in question will be as if the inertia of the ellipsoid were increased by $\frac{\mathrm{F}}{1 - \mathrm{F}} \cdot \frac{\rho_2}{\rho_1}$. When the ellipsoid is one of revolution, the integration can be effected in finite terms.

If $a = c$ (that is, if the oscillation is in the equatorial plane) put $\frac{b}{a} = \cos\omega$, and we have

$$\mathrm{F} = \frac{1}{2} \operatorname{cotan}^2\omega \left(\frac{2\omega}{\sin 2\omega} - 1 \right).$$

This formula is suitable for an oblate spheroid. If it is prolate, imaginaries are avoided by putting $\frac{a}{b} = \cos\omega$, when

$$\mathrm{F} = \frac{1}{2} \operatorname{cosec}^2\omega \left(1 - \cos\omega \cdot \operatorname{cotan}\omega \cdot \log\tan\left(\frac{\partial}{4} + \frac{\omega}{2} \right) \right).$$

If $b = c$ (that is, if the oscillations are on the polar axis), put $\frac{b}{a} =$ $\cos\omega$, and we have

$$F = \cot^2\omega \left(\operatorname{cosec}\omega \cdot \log \tan \left(\frac{\partial}{4} + \frac{\omega}{2}\right) - 1\right).$$

This formula is suitable for a prolate spheroid; for an oblate one, put $\frac{a}{b} = \cos\omega$ and we have,

$$F = \operatorname{cosec}^2\omega \left(1 - \frac{\omega}{\tan\omega}\right).$$

When the ellipsoid becomes an infinite cylinder swinging transversely, we readily see by the second equation that $F = \frac{1}{2}$, and the coëfficient of the effect is unity. In the case of a sphere, all the equations have indeterminate forms. We may consider the last as being the simplest. When ω is made infinitesimal this becomes

$$F = \frac{1}{\omega^2(1 - \frac{1}{6}\omega^2)^2}\left(1 - \frac{\omega}{\omega(1 + \frac{1}{3}\omega^2)}\right) = \frac{1}{3}.$$

Hence, the coëfficient of the effect is $\frac{1}{2}$. In the case of a plane oscillating tangentially, the first equation shows that F is a zero of the second order, so that not merely the coëfficient, but also the whole effect, vanishes. The same is true of a cylinder oscillating longitudinally. In the case of a circular disk oscillating normally, we have to put in the last equation $\omega = \frac{\partial}{2}$. It will be convenient to substitute $\varphi = \frac{\partial}{2} - \omega$ and then put φ infinitesimal. This gives us

$$F = \sec^2\varphi\left(1 - \left(\frac{\partial}{2} - \varphi\right)\tan\varphi\right) = 1 - \frac{\partial}{2}\varphi.$$

Then, the coëfficient of the effect is

$$\frac{F}{1 - F} = \frac{2}{\partial}\frac{1}{\varphi}.$$

If R be the radius of the disk, the volume of the infinitesimally thin spheroid is $\frac{4}{3}R^3\varphi$. Hence, the effect is $\frac{2}{\partial}\frac{4}{3}R^3$. The effect for the sphere erected on the circle is $\frac{1}{2}\frac{4}{3}R^3$. So that we find that the effect of the circular disk is $\frac{4}{\partial}$ that of the sphere erected on it.

The following little table will show the run of the function:

	EQUATORIAL OSCILLATIONS.		POLAR OSCILLATIONS.	
Ratio of polar to equatorial diameter.	Ratio of hydrodynamic effect to buoyancy.	Ratios of successive coëfficients.	Ratio of hydrodynamic effect to buoyancy.	Ratios of successive coëfficients.
0	0.000		. ∞	
$\frac{1}{4}$	0.174		2.374	
		1.78		$\frac{1}{2.13}$
$\frac{1}{2}$	0.310		1.115	
		1.61		$\frac{1}{2.23}$
1	0.500		0.500	
		1.14		$\frac{1}{2.38}$
2	0.704		0.210	
		1.22		$\frac{1}{2.56}$
4	0.860		0.082	
∞	1.000		0.000	

On examining this table, we see that, in reference to equatorial oscillations, 1st, the flatter the spheroid the less the resistance not only absolutely but relatively to the displacement (or cross-section, which, in this case, is in the same proportion); 2nd, that this change of the coëfficient with a change of shape of the spheroid is greater and greater the flatter the spheroid and less and less the longer it is, until it must soon become insensible. This shows that a moderately long cylinder may be treated as infinitely long; nay, more, that a moderately long ellipsoid may be treated as an infinite cylinder, the small amount of air which flows over its ends not relieving the flow of the rest, perceptibly. But the flatter the ellipsoid the sharper becomes its edge, which quickly sheds the air. A short ellipsoid thus bears but a very slight resemblance to a short cylinder which has no such edge. Yet a large proportion of air must flow over the ends of a short cylinder. Accordingly, in the absence of any mathematical analysis, it is difficult to treat an object of this form. The effect of the shedding of the air is shown in the table relating to polar oscillations. We see here that the more prolate spheroids not only resist less proportionally to their bulk (the cross-section remaining the same), but also less absolutely. The ratios of successive numbers here for bodies nearly spherical are about the squares of those in the other table. Here, as before, the sharper the points the greater and greater is the effect of further sharpening, and *vice versa.* When the spheroid is moderately flat the absolute hydrodynamic effect is nearly the same as for a circular disk; when it is moderately pointed the effect, relative to its volume, is very small, but, in comparison to its own magnitude, is very variable.

A rough estimate of the effect on a short cylinder oscillating transversely may be obtained as follows: First, compare the effects on a sphere and a cylinder having the same volume and resisting section. The ratio of the diameter to the altitude of such a cylinder will be $\frac{9}{128}\partial^3$, or nearly 1.09. This cylinder will undoubtedly have a greater resistance than the sphere and less than a circular disk equal to the diametral section of the sphere. But the effect on the disk is only $\frac{4}{\partial}$, or about 1.27; so that if we take the effect on the cylinder as 1.18 (for it must be somewhat nearer that on the disk), we cannot be very far out. This would be supposing it to carry 0.59 of its displaced air. This is about the same that is carried by a prolate ellipsoid whose axes are in the ratio of $\sqrt{2}:1$. A shorter cylinder must carry less air and a longer one more, relatively to the volume. But the difference cannot be so great as the difference of ellipsoids from the sphere. We may consistently suppose that every cylinder carries as much air as a spheroid of the same volume, the ratio of whose polar axis to its equatorial diameter is $\sqrt{2} \times 1.09$, or 1.54 that of the ratio of the altitude to the diameter of the cylinder.

Stokes has shown that the hydrodynamic effect is largely increased by the walls of the vessel containing the pendulum. In the case of a sphere of radius a in a spherical vessel of radius b, the ratio of increment is $\frac{b^3 + 2a^3}{b^3 - a^3}$. In the case of an infinite cylinder in a concentric vessel, the ratio is $\frac{b^2 + a^2}{b^2 - a^2}$. For large values of b these expressions coincide. The case of an ellipsoid in a cylindrical vessel has not been solved; but an estimate of the effect may be made, as follows: A cylindrical vessel would probably act on an oscillating sphere to one-half the amount of a spherical vessel of the same diameter; but if the oscillating body is an oblate spheroid, whose polar axis coincides with that of the vessel, the vessel has relatively less effect, because most of the air escapes in that direction. In the case of a sphere, half the air escapes toward the sides and half to the top and bottom. If the ellipsoid, instead of having the coëfficient of the hydrodynamical effect $\frac{1}{2}$, like the sphere, has only $\frac{1}{3}$, we may say that only $\frac{1}{3}$ escapes in the equatorial direction and is affected by the cylindrical envelope. In general, therefore, we may estimate this small quantity sufficiently by multiplying the correction for the sphere in a spherical envelope by the coëfficient of the hydrodynamic effect.

In the fourth place, even if the air had no weight and consequently no statical pressure, it would still affect the motion of the pendulum in virtue of its *viscosity.* This effect forms the subject of a fine investigation by Stokes. He shows that the viscosity of the air causes a decrement of the amplitude in a constant ratio. This is the cause of the phenomenon represented by the second term of the equation

$$\mathrm{D}_t\Phi = -a - b\Phi - c\Phi^2.$$

In the case of an oscillating sphere this part of the decrement consists entirely of two terms, one proportional to the square root of the viscosity and the other to the viscosity itself. In the case of an infinite cylinder, two similar terms constitute the bulk of the effect. In the case of a plane oscillating tangentially, only the term proportional to the square root of the viscosity appears. In all three cases the formulæ of Professor Stokes exhibit a remarkable relation between the effect on the decrement of the arc and the effect on the period of oscillation; namely, that that term of the former which is proportional to the square root of the viscosity is identical with the only considerable term of the latter. In fact, the viscosity introduces into the differential equation of the motion a term in $\frac{\mathrm{d}s}{\mathrm{d}t}$ and a term in $\frac{\mathrm{d}^2s}{\mathrm{d}t^2}$. The former of these has a part which varies as the square root of the viscosity, and the coëfficient of this part is equal to the coëfficient of the term in $\frac{\mathrm{d}^2s}{\mathrm{d}t^2}$.

By the viscosity is here meant what Stokes terms the index of internal friction and Maxwell the kinematical viscosity. It is the quotient of the retardation of the velocity at any point of the fluid caused by the excess of the velocity at this point over the mean velocity in the neighborhood divided by this excess. Analytically defined, it is

$$\mu^1 = \frac{\dot{v}}{\left(\frac{d^2}{dx^2} + \frac{d^2}{dy^2} + \frac{d^2}{dz^2}\right)v}.$$

The dependence of the viscosity of air upon its pressure and temperature has been one of the chief objects of physical inquiry in our day. That it is inversely proportional to the pressure is fully established, this being equally the prediction of the kinetical theory of gases and the result of all the experiments, whether those by Graham and Meyer and Springmühl on transpiration, or those of Maxwell and others on the oscillation of plane surfaces. In respect to the dependence upon temperature, opinion does not seem to be quite unanimous. Nevertheless, the experiments of von Obermayer, of Kundt and Warburg, of Wiedemann, of Holman, and of Puluj, all concur in showing that, in the case of air, the viscosity as here defined varies nearly as the $\frac{7}{4}$ power of the absolute temperature.

Inasmuch as the function of the viscosity always appears in the formulæ multiplied by the density of the air, it follows that the logarithmic part of the decrement will appear as two terms. Namely, for heavy end down

$$b_d = \frac{k_1}{lh_d}\tau^{\frac{3}{4}} + \frac{k_2}{lh_d}\frac{\sqrt{p}}{\sqrt[8]{\tau}},$$

and for heavy end up,

$$b_u = \frac{k_1}{lh_u}\tau^{\frac{3}{4}} + \frac{k_2}{lh_u}\frac{\sqrt{p}}{\sqrt[8]{\tau}}.$$

And the effect of the resistance on the period of oscillation will be for heavy end down

$$\Delta T = \frac{T}{\eth}\frac{k_2}{lh_d}\frac{\sqrt{p}}{\sqrt[8]{\tau}},$$

and for heavy end up,

$$\Delta T = \frac{T}{\eth}\frac{k_2}{lh_u}\frac{\sqrt{p}}{\sqrt[8]{\tau}}.$$

It must be remembered that this supposes b to be expressed in parts of the radius, it being an angular quantity.

The theory of Stokes supposes that the friction between the air and the pendulum is not infinitely less than that between two layers of air, so that no slipping of the air over the pendulum can take place. He adduces some facts in support of this hypothesis, but Messrs. Kundt and Warburg showed in 1875 that this is not true. It is, however, easy to show from their observations that the amount of slipping is insignificant. They find, in fact, that the coëfficient of slipping, which is the depth to which the solid must be considered to be composed of air in order to account, on the hypothesis of no slipping, for the degree of motion on its surface, is inversely proportional to the density and is at ordinary pressure and at the freezing-point of water equal to $\frac{1}{10}$ of a micron. Consequently, even at a pressure of only $\frac{1}{3}$ of an inch it would only amount to $\frac{1}{100}$ mm. Now, as the radius of the stem of the pendulum is 2 cm, the friction could not be reduced by this slipping more than $\frac{1}{1000}$ part, which is quite imperceptible.

It has been rendered probable by Stokes that a large envelope about the pendulum will not much affect this correction, but this will not probably be the case when the pendulum approaches near to the envelope in swinging, and the rate of decrement is our only guide in the matter.

It is, now, necessary to apply the general formulæ to the calculation of the values of the coëfficients of the atmospheric corrections.

The effect of the buoyancy of the brass is half the ratio of the moment of gravity on the displaced air to its moment on the pendulum. All these calculations will be made in C. G. S. absolute units. We are, however, unacquainted with the absolute unit of temperature, and it becomes necessary to adopt one. The unit chosen will be $273° + 15° = 288°$ of the Centigrade scale; so that the absolute zero being taken as zero, 15° C will be a temperature of unity. This choice is made because 15° C is the mean temperature at which it is desirable to make pendulum-experiments. Centigrade degrees will always be spoken of, but τ in the formulæ and calculation will denote the absolute temperature divided by 288 Centigrade degrees above the absolute zero. The coëfficients of pressure will relate to ONE ABSOLUTE ATMOSPHERE, by which is meant one million grammes per centimetre (second)^2. This may be converted into the pressure of a height of mercury by a calculation like the following:

Names of quantities.	Numbers.	Logarithms.
Density mercury at 0° C compared with water at 4°	13.5959	1.133405
Absolute density of water at 4°	$0.99999\frac{\text{gr}}{(\text{cm})^3}$	-4
∴ Multiplying, Absolute density of mercury at 0°	$13.5957\frac{\text{gr}}{(\text{cm})^3}$	1.133401

Gravity at Paris (60^m elevation)	$980.88 \frac{cm}{s^2}$	2.991616
∴ Multiplying, Absolute specific gravity of mercury at Paris	$13335.7 \frac{gr}{cm(s)^2}$	4.125017
One absolute atmosphere	$1000000 \frac{gr}{cm(s)^2}$	6.000000
∴ Dividing, One absolute atmosphere expressed in centimetres, pressure of mercury at 0° at Paris at elevation of 60^m	74.986	1.874983

This is equal to 29.63 inches pressure at Hoboken at 15° C. At London it is less by 0.03 inch, a quantity which produces hardly a perceptible effect in the time of oscillation of the pendulum.

To find the density of the air under this pressure, we have, according to the experiments of Regnault (Wüllner's *Experimentalphysik,* 3ter Band, 3te Auflage, S. 133), the absolute density of dry air free from CO_2 at 0° C and pressure 76 cm at Paris (60^m elevation), 0.0012932. This supposes a slightly different absolute density of water from that above assumed; but that is a matter of no consequence. Then we have

		Log.
Density dry pure air at Paris, standard atmosphere	.0012932	7.11167
Centimetres, pressure in Paris, standard .	76	1.88081
Centimetres in absolute atmosphere. . . .	74.986	1.87498
∴ Density pure dry air at 0° C under one absolute atmosphere	.0012760	7.10584
Absolute temperature of 0° C $= \frac{273}{288}$		9.97677
∴ Density pure dry air at 15° C under one absolute atmosphere	.0012095	7.08261
Correction for usual amount $CO_2 (1 + .529 \times 4 \times 10^{-4})$		0.00009
∴ Density common dry air at 15° C under one absolute atmosphere	.0012097	7.08270

The moisture was not observed during the pendulum-experiments but is believed to have been as much as would be contained in air at a little less than $\frac{1}{2}$ saturation at 15°; or say that the density was diminished by 3 thousandths. The density of the air taken may then be taken at .001206 at 15° C under a pressure of one absolute atmosphere.

Our pendulum never having been weighed in water, it is necessary to estimate its density. The brass of which it is composed may be supposed to be of the same density as that of the Prussian pendulum-metre, which density is given by Bruhns as 8.5. But the pendulum contains a certain amount of steel. The knives have as their dimensions $9.55 \times 1.8 \times 1.4$, the product being 24 (cm)^3. A part is beveled off, but other steel about the pendulum will make up nearly the amount. The entire volume of steel would be therefore 48 (cm)^3. We may take it at 45 (cm)^3. Assigning to steel the density 7.82 the mass of steel would be 352 gr. The entire mass of the pendulum has been ascertained by weighing to be 6308 grammes. Subtracting the mass of the steel, we have for that of the brass 5956 grammes. Dividing this by the assumed density of the brass we have 701 (cm)^3 as the volume of brass, and 746 (cm)^3 as the total volume of the metal in the pendulum. Then for the density of the metal we have $\frac{6308}{746}$ or $8.46 \frac{\text{gr}}{(\text{cm})^3}$.

But a part of the pendulum is the air within the hollow bob. To find the volume of this, we have the following data, obtained by measurement:

$$\begin{aligned} h_d - h_u &= 39.392 \\ o &= 59.25 \end{aligned}$$

We then calculate as follows:

$\frac{1}{2}(h_d - h_u)$	19.696	1.29438
o	59.25	1.77269
Ratio volume hollow to that of metal . .	0.3324	9.52169
Volume metal	746	2.87276
Volume hollow.	248	2.39443

The accuracy of this calculation can be checked by another. The hollow of the bob is a cylindrical ring. The thickness of the metal is said to be 0.1 cm. The exterior diameter of the bob is by accurate measurement 11.4 cm. Its interior diameter would, therefore, be 11.2 cm. The exterior diameter of the stem on which this ring fits is 4.35. Then the inner diameter of the hollow ring would be 4.55 cm. The exterior height of the bob is 3.175 cm; then, its inner height would be 2.975. Calculation from these data gives as the volume of the hollow 243 (cm)^3; thus confirming, in some measure, the density of brass assumed.

The total volume of the pendulum, so calculated, is 994 (cm)^3. Then the absolute density of the whole is $6.35 \frac{\text{gr}}{(\text{cm})^3}$. And the ratio of the density of air at 15° and under pressure of one absolute atmosphere to that of the pendulum is $\frac{.001206}{6.35} = .0001900$. To get the effect of buoyancy with heavy end down and up, this ratio is to be multiplied by one-fourth the distance between the knife-edges (which is 100.01 cm), divided by the distance of the centre of mass from the point of support. This gives, for heavy end down and up, .0000682 T_d, and .0001565 T_u. Even should the density of the brass be in error by 2 per cent., the resulting error in the relative gravity of two stations, where the barometric difference was 5 inches, would be inappreciable with heavy end up (much more with heavy end down) in the sixth place of decimals.

All the remaining parts of the atmospheric effect are inversely proportional to the moment of inertia of the pendulum. Experiments to be described below show that in vacuo at Hoboken at 15° C.

$$\begin{aligned} T_d^2 &= 1.012045 \\ T_u^2 &= 1.011465 \\ T_d^2 - T_u^2 &= .000580 \\ \tfrac{1}{2}(T_d^2 + T_u^2) &= 1.011755 \end{aligned}$$

Then it follows that the square of the radius of gyration about the centre of mass is

$$h_d h_u \left(1 + \frac{2}{\dfrac{T_u^2 + T_d^2}{T_u^2 - T_d^2} - \dfrac{h_d - h_u}{h_d + h_u}} \right).$$

From this we find the two radii of gyration to be $\sqrt{6963.0}$ and $\sqrt{3033.3}$ centimetres; and the two moments of inertia, the mass being 6308 gr, are 4392×10^4 gr (cm)^2 and 1913×10^4 gr (cm)^2.

To find the effect of the air in the tube forming the stem of the pendulum, we have the following data:

	Centimetres.
Diameter of tube	3.99
Length	123.6
Distance centre to knife.	50.00
Square of radius of gyration of the pendulum about its centre of mass parallel to knife-edges	2111

This gives for the solid contents of the tube 1515 (cm)^3, for the square of the radius of gyration is—

$$(50.00)^2 + \frac{1}{4}\left(\frac{(3.99)^2}{4} + \frac{(123.6)^2}{3}\right) = 3774 \text{ (cm)}^2.$$

The two moments of inertia of the pendulum are, with heavy end down, 4387×10^4 gr. $\times$ (cm)^2, and with heavy end up 1911×10^4 gr $\times$ (cm)^2. Whence the effect of this air is to add to the times of oscillation .0000786 T_d and .0001807 T_u, respectively.

We come now to the hydrodynamic effect. The greater part of this is due to the stem of the pendulum. This is a cylinder whose dimensions are—

	Centimetres.
Length	123.8
Diameter	4.33

From these data the solid contents are found to be 1823 (cm)^3. Its radius of gyration being slightly greater than that of the tube, may be taken at $\sqrt{3779}$ cm. Then, the air having the standard density, the moment of inertia is 8310 gr (cm)^2, and the effects on the periods of oscillation will be

$$.0000946\ T_d \text{ and } .0002169\ T_u.$$

When the pendulum is on the Geneva stand, the effect of the walls of the cylinder have to be taken into account. If the bells are not on, only the 90 middle centimetres of the stem are affected. The square of the radius of gyration is thus reduced to 3176 (cm)^2, only 84 per cent. of that of the whole cylinder. The solid contents are only .727 of the whole, and hence the affected moment of inertia is only $.727 \times .84 = .61$ of the whole. The diameter of the cylinder is 25 cm. Hence, the coëfficient of correction is $2\dfrac{(4.33)^2}{(25)^2 - (4.33)^2} \times .61 = .0377$; which gives for heavy end down and up

$$.0000036\ T_d \text{ and } .0000081\ T_u.$$

But when the bells are on the whole is affected, and the corrections are

$$.0000058\ T_d \text{ and } .0000134\ T_u.$$

The two bobs are not precisely of the same size. Their dimensions are

	SOLID BOB.	HOLLOW BOB.
	Centimetres.	Centimetres.
Diameter	11.48	11.42
Height	3.25	3.18

Their solid contents are

$$336.3\ (\mathrm{cm})^3 \qquad 325.0\ (\mathrm{cm})^3.$$

But a part of this volume has already been reckoned as a part of the stem of the pendulum. Owing to the influence of the re-entrant angle at the junction of the bob and stem, which must cause an increase of the effect, it will be better to leave this core as a part of the stem than to include it in the bob. The volume of this core is

$$\ominus\ (4.33)^2\ (3.18) = 46.8\ (\mathrm{cm})^3 \text{ for the light bob}$$
$$\text{and } \ominus\ (4.33)^2\ (2.25) = 47.9\ (\mathrm{cm})^3 \text{ for the heavy one.}$$

Subtract these from the volumes already obtained, we get as the true volumes

$$278.2\ (\mathrm{cm})^3 \text{ for the light bob}$$
$$\text{and } 288.4\ (\mathrm{cm})^3 \text{ for the heavy one.}$$

The squares of the radii of gyration of these bobs about their centres of mass, parallel to the knife-edges, are

$$\frac{1}{4}\left(\frac{(4.33)^2 + (11.42)^2}{4} + \frac{(3.18)^2}{3}\right) = 10.16\ (\mathrm{cm})^2$$

$$\frac{1}{4}\left(\frac{(4.33)^2 + (11.48)^2}{4} + \frac{(3.25)^2}{3}\right) = 10.29\ (\mathrm{cm})^2.$$

The centre of each bob is distant 9.283 cm from the nearest knife-edge. Hence, the squares of the radii of gyration about the near knife-edges are

$$(9.283)^2 + 10.16 = 96.33\ (\mathrm{cm})^2 \text{ for the light bob}$$
$$(9.283)^2 + 10.29 = 96.46\ (\mathrm{cm})^2 \text{ for the heavy bob.}$$

And about the far edges the value for both is

$$(109.28)^2 + 10 = 11954\ (\mathrm{cm})^2.$$

Then, using the standard density of air, we find for the moment of inertia of the air displaced by both bobs, with heavy end down, 4190 gr $(\mathrm{cm})^2$, and with heavy end up, 4045 gr $(\mathrm{cm})^2$.

To calculate what proportion of the displaced air is to be considered as carried along, we first find the ratios of the axes of both cylinders. These are—

For the light bob	.278
For the solid bob	.283

These are to be multiplied by 1.54, to find the cosine ω. We thus find ω for light bob 64°.6 and for the heavy bob 64°.1. Hence, we find, by Green's formula, for the coëfficient of the effect, .278 for the solid bob and .272 for the hollow one. This gives for the effective moments of inertia,

With heavy end down.	1244 gr $(cm)^2$
With heavy end up	1185 gr $(cm)^2$.

Whence, the effects on the time of oscillation are

$$.0000141\ T_d \text{ and } .0000310\ T_u.$$

Putting 27 cm as the diameter of the bell-glasses of the Geneva support, it appears that their effects upon the correction for the bob are

$$.0000021\ T_d \text{ and } .0000046\ T_u.$$

We have now taken account of the following amounts of displaced air:

Displaced by the stem.	1823 $(cm)^3$
Displaced by the light bob	278
Displaced by the heavy bob	288
Total	2389 $(cm)^3$

But the whole amount displaced by the pendulum may be reckoned thus—

Displaced by metal	746 $(cm)^3$
Displaced by hollow of bob	248
Contents of tube of stem	1533
Inclosed in frames	16
Total	2543 $(cm)^3$

There remain, therefore, 154 $(cm)^3$ displaced by the knives, and apparatus for holding them, by the collars which secure the bobs, and by the little cylinders at the ends of the pendulum. The position of the centre of mass of this air may be estimated as 3 cm outside the knife-edges and its radius of gyration about its centre as 2 cm. The hydrodynamic effect should be taken as equivalent to carrying the displaced air, as a part of this air enlarges the cylindrical stem and the rest is so shaped as to offer very great resistance. Hence the effects are

$.0000105\ T_d$ and $.0000241\ T_u$.

If we, now, add together the various parts of the effects of buoyancy, of inclosed air, and of air carried outside, we have the total calculable effect proportional to the atmospheric density, as follows:

	Heavy end down.	Heavy end up.
Buoyancy	682×10^{-7}	1565×10^{-7}
Air within stem	795	1826
Air within frames	24	70
Air without stem.	946	2169
Air without bobs.	141	310
Air without knives, etc..	105	241
Sums	2693	6181

When the pendulum swings upon the Geneva support without the bell-glasses we have to increase these effects by

Effect of cylinders.	36	71
Sums	2729	6252

When the bell-glasses are in place we have in addition

Effect of bells on stem	22	53
Effect of bells on bobs	21	46
Sums	2772	6351

The result of this calculation is probably a little too small, owing to neglected terms, and can hardly be too large.

The calculation of the effect of viscosity on the time of oscillation depends on the variation of the decrement of the arc with the pressure. Experiments were made upon the Geneva support at Hoboken at various pressures. The observations of arc made during these observations, as has been explained above, were reduced according to the formula

$$\dot{\Phi} = -b\Phi - c\Phi^2,$$

a being supposed zero in order to diminish the number of unknown quantities. The coëfficient c was supposed proportional to the density and one factor was taken for all the experiments, while b was left to be determined independently for each. The result is that all the abnormal variations of the decrement which are considerable are thrown upon b, so that the latter presents an appearance of greater irregularity than properly belongs to it. The results of these experiments are shown in illustration No. 37*c*. Those with heavy end down have been brought to heavy end up by multiplying them by $\frac{h_d}{h_u}$. The time is expressed in minutes; the pressure in inches pressure at 15° C at Hoboken. It will be seen that the observations satisfy sufficiently well the formula

$$b = .0013\ \tau^{\frac{3}{4}} + .00435\ p^{\frac{1}{2}}\ \tau^{-\frac{1}{8}}.$$

When the bell-glasses were removed, the time of decrement was noticeably increased; but this is partly due to the change in the value of c. Upon the Repsold support there is scarcely any sensible difference between the time of decrement from that in experiments of the Geneva support with the bells removed. This is shown on illustration No. 36. To compare the observations of arc at Paris, Berlin, and Kew, with those at Hoboken, they were recalculated with only three constants. These as corrected by least squares are

$b = .0001082$ (units: one second of time and one minute of arc.)
$c = .000001125$

t_0, reckoning from $1°10' = 8001^s$. The agreement of these values with observation is shown below:

φ (Obs.)	φ (Calc.)	C–O
130′	129′.96	−0′.04
110	109.85	− .15
100	100.17	+ .17
80	80.05	+ .05
70	69.89	− .11
50	49.91	− .09
40	40.12	+ .12

Reduced to decimal parts of radius, minutes of time and heavy end up, these values become

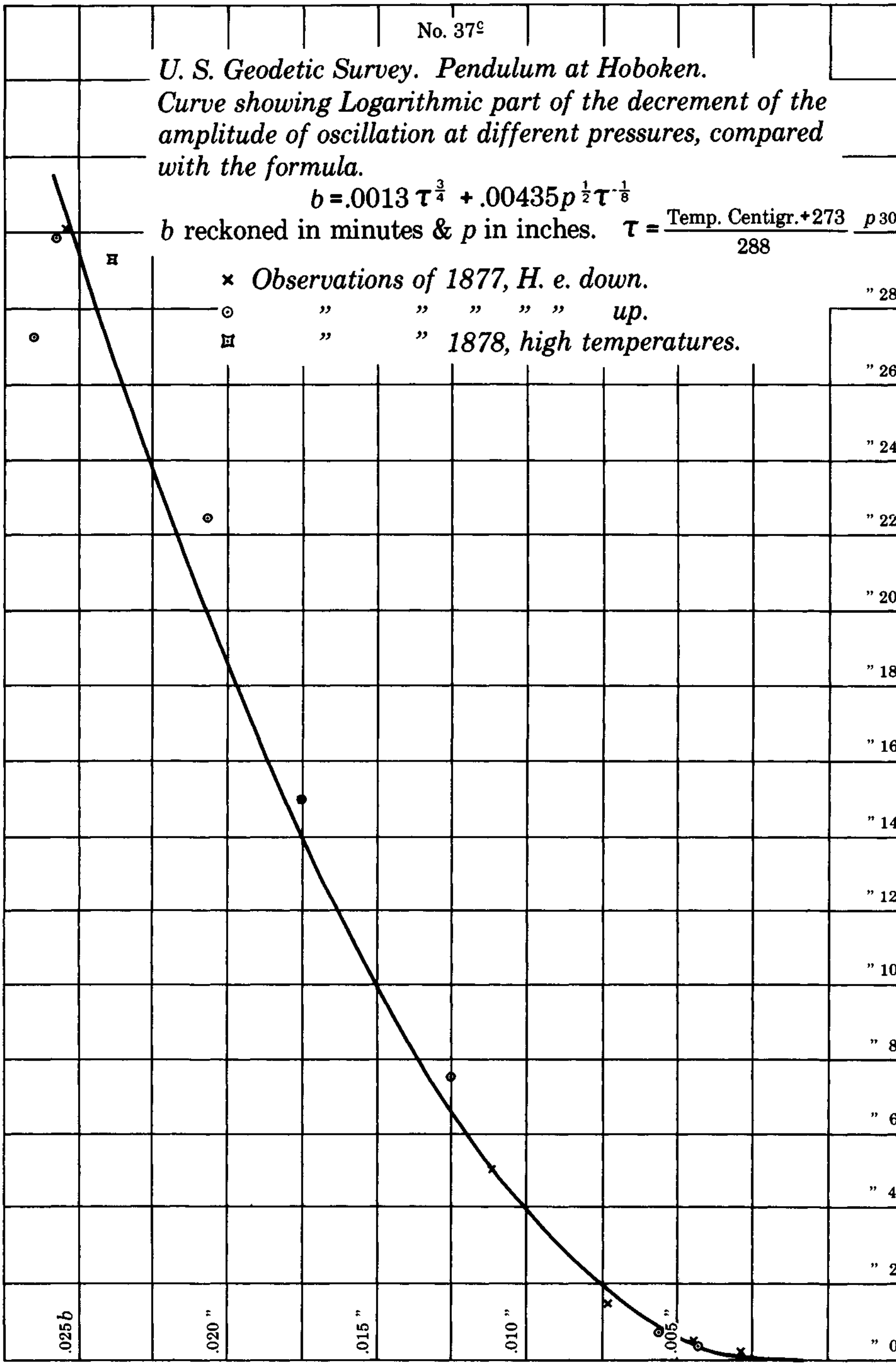
$$b = 0.0214, \qquad c = 0.76.$$

Observations of June, 1877, at Hoboken, give

$$b = 0.0242, \qquad c = 0.58.$$

Allowing the European observations a weight of 3 and combining the values of c, we have

$$c = 0.72.$$


No. 37º
U. S. Geodetic Survey. Pendulum at Hoboken.
Curve showing Logarithmic part of the decrement of the amplitude of oscillation at different pressures, compared with the formula.
$b = .0013\,\tau^{\frac{3}{4}} + .00435\,p^{\frac{1}{2}}\tau^{-\frac{1}{8}}$
b reckoned in minutes & p in inches. $\tau = \frac{\text{Temp. Centigr.} + 273}{288}$
× Observations of 1877, H. e. down.
⊙ " " " " " up.
⌑ " " 1878, high temperatures.
p 30
" 28
" 26
" 24
" 22
" 20
" 18
" 16
" 14
" 12
" 10
" 8
" 6
" 4
" 2
" 0
.025 b
.020 "
.015 "
.010 "
.005 "

Substituting this value, we find for b at

Paris, Berlin, and Kew	0.0224
And at Hoboken	0.0212
b (weighted mean)	=0.0221

The curve drawn in illustration No. 36 is calculated from the European coëfficients, and the agreement of the observations taken at Hoboken before the bell-glasses were put on is shown by the near coincidence with it of the points distinguished by crosses. The points distinguished by circles are obtained from a combination of all the observations taken in 1877 with the bells on, when the pressure was about 30 inches. The t_0 taken in each case was the one that made the mean excess over the curve equal to zero. The influence of the bell-glasses in arresting the motion of the pendulum is thus very strikingly shown. The value of b, with the bells on, under a pressure of 30 inches, appears by the above formula to be 0.0251 for a minute of time. The mean of the experiments with bells off, as just shown, gives $b = .0221$; so that we may assume that the viscosity effect is one-seventh larger with the bells on than off.

To calculate the effect on the period of oscillation, we take the coëfficient .00435, we multiply it by $\sqrt{29.63}$ to bring it to one absolute atmosphere, we divide it by 60 to bring it to seconds, and finally we divide by ∂, and we get as the effect, with heavy end up, .0001256 T_u. To find the effect with heavy end down, we simply multiply by $\frac{h_u}{h_d}$, which gives .0000548 T_d. When the bells are off, $\frac{7}{8}$ of these values are to be taken.

At excessively low pressures the whole theory of atmospheric viscosity fails, because the fundamental hypotheses are then violated; and, therefore, the real effect of viscosity at $\frac{1}{4}$ inch pressure will probably be somewhat smaller than calculation would make it.

Experiments have been made at Hoboken on the Geneva support, in order to determine the effect of atmospheric pressure *à posteriori.* A series of experiments were made in September, 1877, with heavy end down, and another in December, 1877, with heavy end up. The duration of each experiment was generally long, and the agreement of the results is all that could be expected. These observations were made by Mr. Farquhar. The temperature, during the September experiments, was about 20° C; that during the December experiments was about 10° C. They have been corrected so as to bring them exactly to these temperatures. The results of these experiments are exhibited on illustrations Nos. 37*a* and 37*b*. It will be seen that the sidereal time of oscillation, with heavy end down, satisfies the formula

$$T_d \overset{s.}{=} 1.006072 + .00000985\, p + .0000081 \sqrt{p};$$

and those with heavy end up, the formula

$$T_u = 1.005740 + .00002264\, p + .0000234 \sqrt{p},$$

where p is the pressure in inches at 15° C.

Taking one absolute atmosphere, or 29.63 inches, as the unit of pressure, and reducing the coëfficients to 15° C, we have the general formulæ,

$$T_d \overset{s.}{=} 1.006027 + .0002969 \frac{p}{\tau} + .0000442 \frac{\sqrt{p}}{\sqrt[8]{\tau}},$$

$$T_u = 1.005785 + .0006598 \frac{p}{\tau} + .0001271 \frac{\sqrt{p}}{\sqrt[8]{\tau}}.$$

The values which we have obtained *à priori* are

$$T_d = x \qquad + .0002789 \frac{p}{\tau} + .0000551 \frac{\sqrt{p}}{\sqrt[8]{\tau}},$$

$$T_u = y \qquad + .0006388 \frac{p}{\tau} + .0001263 \frac{\sqrt{p}}{\sqrt[8]{\tau}}.$$

The difference between observation and *à priori* calculation is perhaps not greater than ought to be expected. The values which have been used in the reductions are

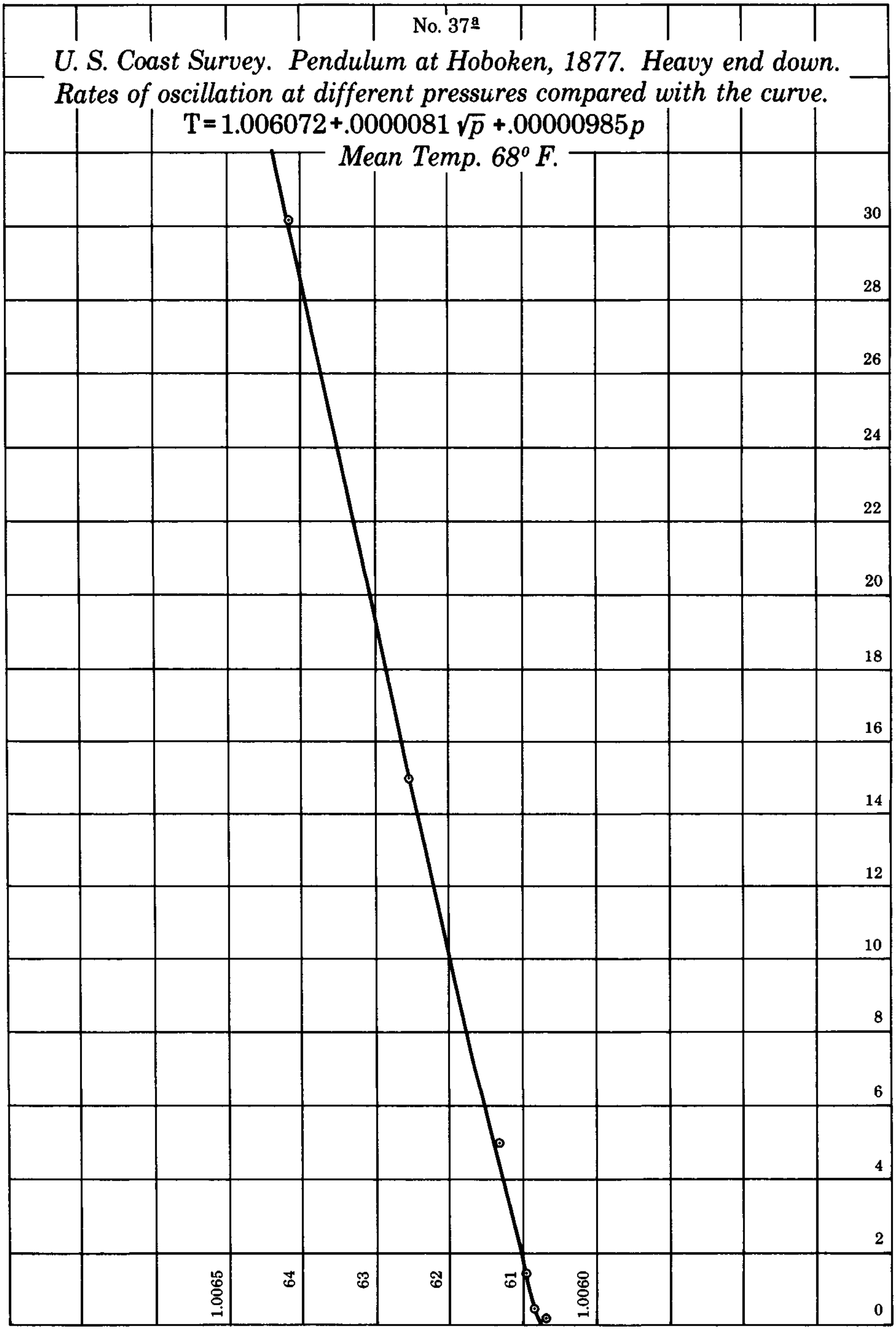
$$T_d = x \qquad + .0002917 \frac{p}{\tau} + .0000512 \frac{\sqrt{p}}{\sqrt[8]{\tau}},$$

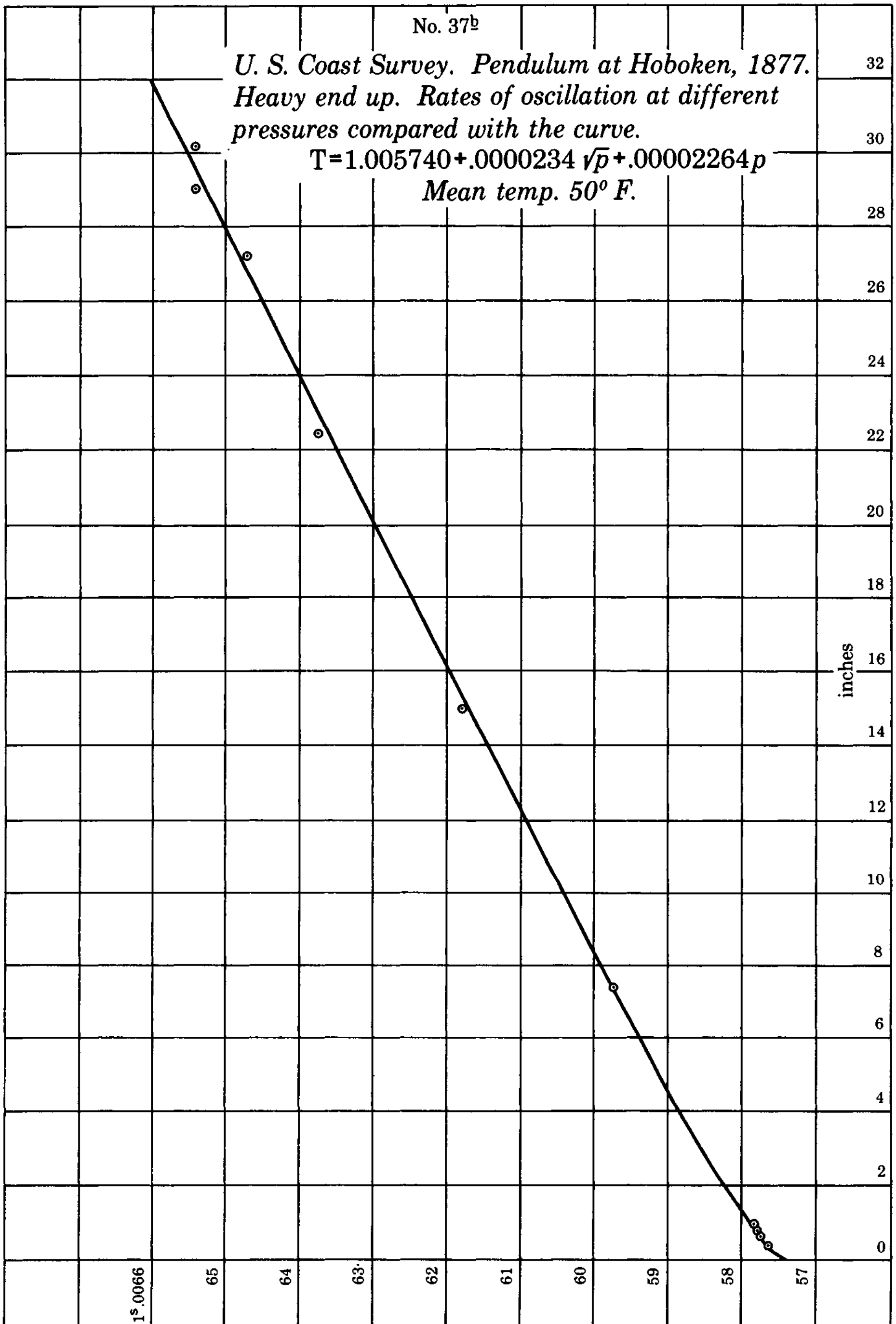
$$T_u = y \qquad + .0006694 \frac{p}{\tau} + .0001175 \frac{\sqrt{p}}{\sqrt[8]{\tau}}.$$

These values were used before the last calculations of the *à priori* values were completed, and it was not thought worth while to change them; but the *à priori* values are preferred.

COËFFICIENT OF EXPANSION.

The coëfficient of expansion of the pendulum has been determined, by comparing it directly with a metre obtained from the German Imperial Eichungsamt and there designated as Normal


No. 37a
U. S. Coast Survey. Pendulum at Hoboken, 1877. Heavy end down.
Rates of oscillation at different pressures compared with the curve.
T = 1.006072 + .0000081 √p + .00000985p
Mean Temp. 68° F.
30
28
26
24
22
20
18
16
14
12
10
8
6
4
2
0
1.0065
64
63
62
61
1.0060


No. 37b
U. S. Coast Survey. Pendulum at Hoboken, 1877.
Heavy end up. Rates of oscillation at different
pressures compared with the curve.
T=1.005740+.0000234 √p+.00002264p
Mean temp. 50° F.
32
30
28
26
24
22
20
18
16
14
12
10
8
6
4
2
0
inches
1s.0066
65
64
63
62
61
60
59
58
57

Meter No. 49, and also by assuming it to have the same coëfficient as the pendulum-metre, the pendulum-metre having also been compared at different temperatures with No. 49. The coëfficient of expansion of No. 49 has been absolutely determined by comparison with a metre made for the purpose and marked "U. S. C. S.—C. S. P.—1878.—A." The comparisons have been made

Between No. 49 at 13° and A at 3°
Between No. 49 at 3° and A at 13°
Between No. 49 at 4° and A at 4°
and between No. 49 at 18° and A at 18°

The two metres were compared by means of the vertical comparator belonging to the reversible pendulum. They stood in two vertical brass tubes, $4\frac{1}{2}$ cm in diameter and $1\frac{1}{4}$ m long, polished on the exterior and closed at the bottom by a foot terminating in a conical point. One of them rested in the step designed for the pendulum-metre, and was held at the top in a stirrup, movable upon a screw in such a way as to vary its distance from the microscope. The metre rested at the bottom of the tube in a species of trap, in which it was compressed just sufficiently to hold it in place. At about $\frac{3}{4}$ m from the bottom it was lightly held, by springs on its four sides, into a frame which was capable of being moved in any direction, by means of four horizontal screws penetrating the walls of the brass tube. India-rubber washers and nuts kept the screw-holes water-tight. Opposite the lines at the top and bottom of the metre, two windows were inserted in the brass tube, setting into little sashes formed of brass casting. These windows were made of plate-glass, about 3 mm thick, which was carefully selected with a view to the parallelism of its sides, and placed in the sash in such a way that the slope of the wedge should be horizontal. These glass windows were kept water-tight by rubber washers, having a brass washer over them screwed down by four thumb-screws. At the opposite side of the tube, a little above the middle of the metre, was a third window through which a thermometer placed within the tube could be read. These tubes were furnished with stop-cocks at the bottom, and a rapid current of water was kept running through them during the measures and for at least an hour previous. The comparator, tubes, and metres having been put into perfect adjustment, the tubes were fixed tightly in their places by screws, and remained unmoved during the whole of the experiments. Particular care was taken that they should not turn on their axes, so as to alter in any degree the effect of refraction in the glass windows. The comparator was not fixed, since it was necessarily turned from one metre to the other, and also changed in length by a fraction of a millimetre, when the temperature was changed, a screw being provided for the purpose. The two metres were separated from one another and from the comparator by means of screens about 4 mm thick, consisting of light wooden frames with tin-plate on the two sides, and loosely filled in the interior with cotton batting.

The coëfficient of expansion of No. 49 is checked by comparisons at various temperatures with the platinum metre of the German Eichungsamt; for the coëfficient of expansion of platinum has been accurately determined by Fizeau.

As one of the most prominent living metrologists has stated his conviction that the coëfficient of expansion of a metre may be expected to be different in the vertical and horizontal positions it is proper to examine this question. Let the metre be of brass and let its cross-section be 2 $(\mathrm{cm})^2$. The solid contents of this metre will be 200 $(\mathrm{cm})^3$ and its mass may be taken at 1680 gr. The modulus of elasticity of brass, according to Wertheim, is $9 \times 10^{11} \frac{\mathrm{gr}\,(\mathrm{s})^2}{\mathrm{cm}}$. This is not the same at all temperatures, however, and judging from the analogy of copper we may suppose it $\frac{1}{11}$ part smaller at 100° than at 0°. If the metre be set up on end the mean pressure of its own weight is $\frac{1}{4}\ 1680 \times 981 \frac{\mathrm{gr}\,(\mathrm{s})^2}{\mathrm{cm}}$. Let the expansion from 0° to 100° be x' in the horizontal position. Let the length of the bar in the horizontal position at 0° C be 1 metre. Let it be heated to 100° C in this position; its length will then be $1 + x'$. Let it next be placed in the vertical position; then, its length will be reduced by its weight to

$$(1 + x')\ (1 - \frac{11}{10} \times 420 \times 109 \times 10^{-11}).$$

Let it, next, be cooled to 0° and its length will be

$$(1 + x')\ (1 - \frac{11}{10} \times 420 \times 109 \times 10^{-11})\ (1 - x).$$

Let it, next, be brought to the horizontal position, and its length will be

$$(1 + x')\ (1 - \frac{11}{10} \times 420 \times 109 \times 10^{-11})\ (1 - x)\ (1 + 420 \times 109 \times 10^{-11}).$$

But it is, now, in the original condition so that this length = 1. This gives the equation

$$x' - x = 42 \times 109 \times 10^{-11},$$

a quantity too small to be detected.

The following is a summary of the results of the comparisons between metres A and 49:

Temp. of 49.	Temp. of A.	A − 49.
°	°	μ
+13	+ 3	+130.5
3	13	−247.3
4	4	− 58.8
18	18	− 57.7

These results are all satisfied to the last place of decimals by taking the coëfficient of expansion

$$\begin{aligned} &\qquad\quad \mu \\ \text{of A } &= 18.95 \text{ for } 1^\circ \text{ C} \\ \text{of } 49 &= 18.83 \text{ for } 1^\circ \text{ C, and} \\ \text{A} - 49 \text{ at } 0^\circ \text{ C} &= -59.3 \end{aligned}$$

The comparisons are given in detail in the subjoined table. [Table on p. 123.]

The comparisons made between the German Normal Meter No. 49 and the platinum metre of the Eichungsamt give

$$\text{No. } 49 - \text{Pl. M} = -24^{\mu}_{.}4 + 10^{\mu}_{.}09\,\tau_1,$$

where τ_1 denotes the temperature Centigrade; and the following table shows the agreement of this formula with the observations communicated by Professor Förster:

τ_1	No. 49 − Pl. M.	Same, calc.
°	μ	μ
+ 3.25	+ 8.2	+ 8.4
6.28	39.2	39.0
23.55	213.1	213.2

Fizeau's coëfficient of expansion for platinum is at 0° C, $8^{\mu}_{.}68$ for 1° Centigrade; at 11° C, the mean temperature of these comparisons, this coëfficient becomes $8^{\mu}_{.}72$. Hence the coëfficient of expansion for Meter No. 49, as deducted from these comparisons, is $18^{\mu}_{.}81$. This agrees very well with the absolute determination above given, which may therefore be adopted.

The comparisons between the pendulum-metre and No. 49 are not very satisfactory in their results, but they show that the coëffi-

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[t_A temperature Centigrade of A. t_{49} temperature of 49.]

Date.	Time.	½ $(t_A + t_{49})$	½ $(t_A - t_{49})$	A − 49	½ Σ to 8°	½ Δ to +5°	Corrected A − 49	Diff. from mean (+130.5)
	h. m.	°	°	μ	μ	μ	μ	
May 14	......	7.57	+5.32	+138.7	0.0	−12.1	+126.6	−3.9
14	0 40	8.54	+4.68	+120.0	0.0	+12.1	+132.1	+1.6
14	......	8.65	+4.80	+123.6	−0.1	+7.6	+131.1	+0.6
14	......	8.30	+5.25	+142.1	0.0	−9.5	+132.6	+2.1
14	......	8.36	+5.24	+140.8	0.0	−9.1	+131.7	+1.2
14	......	8.38	+5.24	+141.5	0.0	−9.1	+132.4	+1.9
14	......	8.79	+4.99	+132.5	−0.1	+0.4	+132.8	+2.3
14	......	8.91	+5.11	+137.8	−0.1	−4.2	+133.5	+3.0
15	......	9.11	+4.33	+105.5	−0.1	+25.3	+130.7	+0.2
15	......	8.72	+4.80	+121.1	−0.1	+7.6	+128.6	−1.9
15	......	8.22	+5.30	+139.9	0.0	−11.3	+128.6	−1.9
15	......	8.26	+5.29	+139.8	0.0	−11.0	+128.8	−1.7
15	......	8.40	+5.10	+133.5	0.0	−3.8	+129.7	−0.8
15	......	8.50	+5.05	+131.6	0.0	−1.9	+129.7	−0.8
15	......	8.61	+5.06	+133.4	−0.1	−2.3	+131.0	+0.5
15	9 00	8.94	+4.84	+121.8	−0.1	+6.0	+127.7	−2.8
					½ Σ to 8°	½ Δ to −5°		(−247.3)
16	21 00	7.90	−4.16	−216.6	0.0	−31.8	−248.4	−1.1
16	22 00	7.28	−4.95	−245.3	+0.1	−1.9	−247.1	+0.2
16	23 00	7.42	−5.02	−247.7	+0.1	+0.8	−246.8	+0.5
16	......	7.58	−5.09	−251.4	0.0	+3.4	−248.0	−0.7
16	0 30	7.72	−4.99	−247.9	0.0	−0.4	−248.3	−1.0
16	3 30	8.94	−5.14	−251.5	−0.1	+5.3	−246.3	+1.0
16	......	8.92	−5.22	−257.4	−0.1	+8.3	−249.2	−1.9
16	......	8.85	−5.32	−258.4	−0.1	+12.1	−246.4	+0.9
16	8 30	9.84	−4.73	−235.2	−0.2	−10.2	−245.6	+1.7
					½ Σ to 4°	½ Δ to 0°		(−58.8)
17	22 00	3.45	+0.02	−59.2	+0.1	−0.8	−59.9	−1.1
17	22 30	4.04	−0.04	−62.1	0.0	+1.5	−60.6	−1.8
17	23 30	4.58	+0.04	−55.5	−0.1	−1.5	−57.1	+1.7
17	......	6.00	0.00	−61.0	−0.2	0.0	−61.2	−2.4
17	......	6.46	+0.03	−58.1	−0.3	−1.1	−59.5	−0.7
18	......	4.06	−0.02	−58.5	0.0	+0.8	−57.7	+1.1
18	3 15	4.05	−0.01	−61.5	0.0	+0.4	−61.1	−2.3
18	3 30	3.96	0.00	−57.8	0.0	0.0	−57.8	+1.0
18	3 50	4.04	−0.02	−56.4	0.0	+0.8	−55.6	+3.2
18	4 08	4.14	0.00	−58.2	0.0	0.0	−58.2	+0.6
18	4 20	4.22	0.00	−60.4	0.0	0.0	−60.4	−1.6
18	4 50	4.34	+0.02	−56.9	0.0	−0.8	−57.7	+1.1
18	5 15	4.46	0.00	−57.4	0.0	0.0	−57.4	+1.4
					½ Σ to 18°	½ Δ to 0°		(−57.2)
20	21 30	18.19	0.00	−57.1	0.0	0.0	−57.1	+0.1
20	21 45	18.26	+0.01	−57.0	0.0	−0.4	−57.4	−0.2
20	22 00	18.32	0.00	−56.8	0.0	0.0	−56.8	+0.4
20	22 20	18.45	0.00	−57.4	0.0	0.0	−57.4	−0.2
20	23 30	18.65	+0.01	−55.3	−0.1	−0.4	−55.8	+1.4
20	23 45	18.70	0.00	−57.2	−0.1	0.0	−57.3	−0.1
20	1 45	18.72	+0.01	−56.8	−0.1	−0.4	−57.3	−0.1
20	2 15	18.76	+0.02	−56.1	−0.1	−0.8	−57.0	+0.2
20	3 50	18.75	+0.03	−57.1	−0.1	−1.1	−58.3	−1.1

cient of expansion of the former is certainly the smaller of the two. The following table shows the observed results as compared with the formula

$$\text{U. S. Pendulum-Metre} - \text{No. } 49 = -13^{\mu}\!.3 - 0^{\mu}\!.448\ \tau_1.$$

Date. 1878. March	τ_1	U. S. – No. 49.	Same, calc.	Calc. – Obs.
	°	μ	μ	μ
20	20.25	−25.0	−22.4	+2.6
21	19.68	21.2	22.1	−0.9
22	20.14	22.9	22.3	+0.6
23	19.93	22.0	22.2	−0.2
24	16.84	19.1	20.8	−1.7
25	13.37	19.5	19.3	+0.2
26	8.11	17.1	16.9	+0.2
26	14.07	18.5	19.6	+0.9
27	10.58	16.6	18.0	−1.4

The pendulum itself, compared with No. 49 at 35° C, and at 10° C, gives the following equation:

$$\text{Pendulum} - \text{No. } 49 = -191^{\mu}\!.5 - 0^{\mu}\!.67\ \tau_1.$$

The coëfficient of expansion of the pendulum, therefore, as given by these comparisons, is

$$18^{\mu}\!.83 - 0^{\mu}\!.67 = 18^{\mu}\!.16.$$

The coëfficient of the pendulum-metre, just found, is $18^{\mu}\!.83 - 0^{\mu}\!.45 = 18^{\mu}\!.38$. The mean of these two values is $18^{\mu}\!.27$.

The coëfficient of expansion of the pendulum was also determined from its rate of oscillation at different temperatures, a special series of experiments at high temperatures being taken for the purpose. The results are given below:

	HEAVY END DOWN.		HEAVY END UP.	
	τ_1	T_d	τ_1	T_u
	°	s.	°	s.
Observed values	35.8	1.006537	36.3	1.006719
	19.4	1.006400	10.2	1.006536
Differences	16.4	.000137	26.1	.000183
Correction for atmospheric temperature	....	+ 16	....	+ 58
		.000153		.000241
Effect for 1° C	....	.0000093	....	.0000092

These give as the coëfficient of expansion—

From oscillations with heavy end down	$18^{\mu}5$
From oscillations with heavy end up	$18^{\mu}3$

The value $18^{\mu}3$ has been used in the reductions.

CORRECTIONS FOR THE WEARING DOWN AND ROUNDING OFF OF THE KNIFE-EDGES.

If 5 kilogrammes' weight be put upon an absolutely sharp knife-edge of steel hardened in oil and having a bearing length of 2 centimetres, the steel edge will be crushed until the breadth of the bearing surface is 1 micron. Accordingly, from the very beginning, a knife-edge will wear down and round off. The wearing down and the blunting will have very different effects upon the period of oscillation.

The removal of the point of support of a pendulum from its centre of mass, will have an effect which is readily calculated, thus:

$$\mathrm{d}\,T^2 = \mathrm{d}\cdot\frac{\partial^2}{g}\left(\frac{\gamma^2}{h}+h\right) = \frac{\partial^2}{g}\left(-\frac{\gamma^2}{h}+h\right)\frac{\mathrm{d}\,h}{h}.$$

For a reversible pendulum,

$$\gamma^2 = h_d\, h_u.$$

Hence, we have,

$$\mathrm{d}\,T_d^2 = T_d^2\,\frac{h_d - h_u}{h_d + h_u}\cdot\frac{\mathrm{d}\,h_d}{h_d}$$

$$\mathrm{d}\,T_u^2 = -T_u^2\,\frac{h_d - h_u}{h_d + h_u}\cdot\frac{\mathrm{d}\,h_u}{h_u}.$$

If a pendulum rolls upon a cylindrical surface of radius ρ, the instantaneous axis of rotation is the instantaneous line of contact; and a velocity of rotation about this axis is equivalent to the same velocity of rotation about the line of contact in the equilibrium position of the pendulum combined with such a translation velocity along the length of the pendulum as is necessary to fix the instantaneous axis; this is $2\rho\sin\frac{1}{2}\varphi\cdot\frac{\mathrm{d}\varphi}{\mathrm{d}\,t}$. It follows that the amount by which the *vis viva* of the pendulum is affected by a cylindricity of the knife-edge is of the order of $\rho^2\,\varphi^2$ and may consequently be neglected. The moment of gravity is, however, obviously the same as if the axis of the cylinder were the axis of rotation, that is, it is multiplied by $\left(1 + \frac{\rho}{h}\right)$. Hence we have

$$\delta \mathrm{T}^2 = -\mathrm{T}^2 \frac{\rho}{h}.$$

If the section is not circular, then obviously some sort of a mean radius of curvature must replace ρ. If the section is flatter than a circle, that is, if the lower parts in repose have the greater radii of curvature, then the mean radius, and consequently the effect on the period of oscillation, will be greater for small arcs than for large ones; while if the section is somewhat pointed downwards the reverse will be the case.

We know too little of the laws of crushing and grinding to be able to calculate the radius of curvature from the amount worn off. In fact, the ratio would probably depend on the hardness of the material. Neither can the radius be measured directly with any accuracy. But it may obviously be very large. When the pendulum is first brought down to rest on the edge, why may not the blunted surface be nearly flat? If it were so, the small oscillation through 4° or 5° could not round the edges enough to make the ratio of the radius to the wearing down at all small. Under these circumstances it is a question deserving consideration and experimental examination whether it would not be better to substitute for knife-edges cylinders of measurable diameter—say of 5 millimetres.

Our own experiments always began with a half-amplitude of 2° and ended with a half-amplitude of $\frac{1}{2}^\circ$; but the time of intermediate transits of the pendulum across the vertical was observed when the half-amplitude reached $1\frac{1}{2}^\circ$ and 1°. The intervals of time between the 2° and $1\frac{1}{2}^\circ$ observations were too short to found any conclusions upon; but if we compare the mean period of oscillation while the arc is descending from 2° to 1° with the mean period while the arc is descending from 1° to $\frac{1}{2}^\circ$, we do not find, after the usual correction for arc, any decided difference between them. This is shown in the following table:

Station.	Position of heavy end.	PERIODS OF OSCILLATION. (Corrected for arc, pressure, temperature, and rate of time-keeper.) Arc, 2° to 1°.	Arc, 1° to $\frac{1}{2}$°.	DIFFERENCE. Heavy end down.	Heavy end up.
		s.	*s.*		
Paris. . .	Down . .	1.006053	1.006050	−3	
	Up. . . .	1.006195	1.006198		+3
Berlin . .	Down . .	1.005901	1.005897	−4	
	Up. . . .	1.006028	1.006033		+5
Kew . . .	Down . .	1.005931	1.005930	−1	
	Up. . . .	1.006054	1.006056		+2
Hoboken	Down . .	1.006355	1.006358	+3	
	Up. . . .	1.006550	1.006548		−2

There are, it is true, slight indications here of a correction varying with the amplitude and different for the Repsold support, used at the European stations, and for the Geneva support, used at Hoboken; but nothing can be concluded with certainty.

The measures of length show, in each case, an increase in the distance between the knife-edges from station to station. Thus we have—

	μ
Increase of length from Paris to Berlin	4.1
Berlin to Kew.	3.1
Kew to Hoboken	8.9

This increase cannot have been in any degree due to the wearing down of the knife-edges, for the reason that the measures were made between the centres of the edges, which portions do not bear upon the support and appear, even now, nearly sharp. The increase is most probably due to accumulations of cocoa-butter, etc., under the steel of the knives where they bear on the brass. The effect is, therefore, that of a wearing down without any blunting.

The only indication which we have as to the actual wearing down and blunting is derived from the difference between the periods of oscillation with heavy end down and up. The difference will be

$$C - \frac{20}{21}\,\delta h - \frac{2}{21}\,\rho.$$

It is highly objectionable to infer the values of δh and ρ from the numbers which are to be used to determine the force of gravity. It is fortunate, therefore, that the whole of the wearing and blunting which it is necessary to take account of for the European stations, took place, all at once, at Berlin, after just one-half of the work there had been done. The change is therefore deducible from the Berlin observations alone, and that from the difference of the first half and last half of them, independently of the mean of the whole. The differences of T, with heavy end up and heavy end down, at Berlin, are as follow:

	First four days.	Last four days.
	s.	*s.*
	0.000153	0.000123
	0.000136	0.000126
	0.000131	0.000131
	0.000140	0.000123
Mean	0.000140	0.000126

When these means are corrected for the measured change of length from that at Paris, they become

0.000142 0.000128.

Now, the same difference at Paris is—

	s.
First four days	0.000144
Second four days	0.000140
Mean	0.000142

which agrees exactly with the first four days at Berlin; and the same difference at Kew, after correction for the measured change of length from Paris, is—

	s.
First four days	0.000128
Second four days	0.000129
Mean	0.000128

which agrees exactly with the last four days at Berlin. We thus find that it is quite unnecessary to take account of any other blunting in Europe than that which took place at that time.[2] To separate the effects on T_d and T_u, we have

2. It will be observed that this comparison of the difference of T_d and T_u at different stations could not have been made if the atmospheric corrections had not first been applied.

BERLIN.

	T_d	T_u
	s.	s.
First days	1.0058980	1.0060378
Last days	1.0058988	1.0060246
Changes	+08	−132

The negatives of half these changes are, therefore, to be applied to the observations at Kew, and only the first 4 days' observations at Berlin are available. The deduced values of the wearing down and of the radius of curvature of the section are

$$\delta h = 11^{\mu}.5 \quad \rho = 32^{\mu}.$$

These results agree well with direct observations of the edges, which have been made under a high-power microscope, with illumination through the objective.

The regular set of observations at Hoboken were made upon the Geneva support, instead of the Repsold support, which had been used before. Now, it is a matter of observation that the edges do not even yet, after all their wearing, rest over their whole length upon the Repsold support, but only near their ends. On the other hand, on the Geneva support, they rest nearer the middle. The consequence is that when the Geneva support was used, quite new and unblunted parts of the edges came into play, so that the edges should have been in the same state as at Paris; and this will be assumed to have been the case, though the differences of the supports in other respects prevent a very close comparison.

The two knives cannot be assumed to be alike, either in respect to the distances of the edges from the planes of the bearings of the steel on the brass, or in the figure of the edges; but inasmuch as they are interchanged and equal numbers of experiments made in the two positions, this inequality can have no effect on the final result. But to exhibit the agreement of single days' experiments, the inequality should first be allowed for. In the Paris experiments and the first experiments at Berlin, there was no perceptible difference between them. Thus, we find

Excess of time on oscillation on knife 1 *over that on knife* 2.

	Heavy end down.	Heavy end up.
	s.	*s.*
Paris	+0.000002	−0.000008
Berlin (first days)	−0.000002	+0.000014
Weighted mean	+0.000001	+0.000001

But for the last days at Berlin, and for Kew, the difference is quite perceptible.

Excess of time of oscillation on knife 1 *over that on knife* 2.

	s.	*s.*
Berlin (last days)	+0.000006	+0.000010
Kew	+0.000004	+0.000005
Weighted mean	+0.000004	+0.000006

Half these amounts will be applied with their appropriate signs in exhibiting the results of single days. At Hoboken this correction disappears again.

CORRECTION FOR THE SLIP OF THE KNIFE-EDGES.

There is, according to Bessel, another effect due the knife-edges, which has reverse signs, with heavy end up and down, and which is consequently not eliminated in the formula for the reversible pendulum, although it is eliminated when the periods of oscillation are combined by the formula suitable in considering it as two invariable pendulums, to wit:

$$\mathrm{T} = \frac{\mathrm{T}_d h_d + \mathrm{T}_u h_u}{h_d + h_u}.$$

This action may be termed the slip of the knife-edge, as it is supposed to be due to a motion which the knife is compelled to make, and which no elastic force resists.

In order to detect the existence of such an effect, a stiff steel knitting-needle was slipped through the notch in the support of the knife-edge, while the pendulum was in position, and was then brought up into contact with the edge by copper wires fastened to its extremities and also to a frame mounted on the head of the pendulum-support. These wires had considerable length, so that the knitting-needle was free to move in the direction of its length, while the friction on the knife-edge was very great, so that if the edge had any slip the knitting-needle must oscillate in the direction of its length. In order to observe any such motion a bit of convex spectacle-lens of long focus was attached to one end of the needle, and a plane piece of glass was brought up nearly against it; with the aid of a lamp burning alcohol with salt, Newton's rings were produced and were observed with a microscope, according to the well-known method of Fizeau. The result of this experiment was that, with the largest oscillation of the pendulum, not the least slip could be detected in this way, so that it seemed that there could be no slip as great as $\frac{1}{20}$ of the wave-length of the D line; that is, none amounting to $\frac{1}{20}$ of $\frac{4}{10}$ of a micron.

As this result was unexpected, not to say surprising, the apparatus was critically studied; but it seemed impossible that any slip should occur at the point of contact of the edge with the needle without showing itself in this way.

It might be, however, that, instead of a true slip, the edge was turned at each oscillation, so as to produce a motion similar to a slip; for such an effect would not be detected by our experiment.

CORRECTION FOR SHORTER LENGTH WITH HEAVY END UP.

When the heavy end is down the pendulum is stretched by a greater length. Calculating from the known coëfficient of elasticity of brass, this stretching is found to amount to $1^{\mu}.0$. In the measures of length this amount has accordingly uniformly been added to the length with heavy end up. There is, therefore, a correction of $+10$ in the seventh place to be added to T_u^2 to bring it to what it would be if the pendulum were as long as the reduced measures make it.

THE CORRECTION FOR THE FLEXURE OF THE SUPPORT.

The work upon this correction has been so extensive that it is thought best to reserve the full account of it for a separate paper. It has been shown by the writer that if a horizontal force equal to the weight of the unit of mass deflects the point of support through a distance S, and if M is the mass of the pendulum, h the distance of the centre of mass from the point of support, and l the length of the corresponding simple pendulum, then the effect of the swaying of the stand with the movement of the pendulum is to lengthen the square of the period of oscillation by $T^2 M \frac{Sh}{l^2}$; and the effect on λ the length of the seconds pendulum found with the reversible pendulum is to make it too short by $M S \frac{\lambda}{l}$. This supposes the support to be perfectly elastic, and without any difference between statical and dynamical elasticity.

The quantity S has been found from a long series of experiments to be equal to $0^{mm}.0340$ for the Repsold support. At Paris a larger value ($0^{mm}.0371$) was found, but the larger value has not been used in the reductions. Possibly it should have been used.

When the Repsold tripod rests on a flexible support of any kind the value of S is of course increased. At Hoboken there was no such increase. It is not believed that this increase was a considerable quantity at either Berlin or Kew, but the first occasion will be seized of measuring it. At Paris it may account for the larger value of S there obtained. At Geneva the pendulum was swung on a wooden support. The effect of which may be estimated as follows: Professor Plantamour finds that the correction $M\,S\,\frac{\lambda}{l}$ for his pendulum swinging on this wooden support at Geneva was $0^{mm}.1724$, and at Berlin on a pier similar to that on which the Coast Survey pendulum tripod rested the same correction was $0^{mm}.1357$. The difference, or $0^{mm}.036$, may be taken as the effect of the flexure of the wooden stand. Now, the following are the data concerned, which differ for his experiments and for ours:

	Swiss pendulum.	American pendulum.
Mass of pendulum	3050 gr	6308 gr
Ratio of seconds pendulum to length between knives	1.77	1
Height of edge above feet	1 m	1.3 m

It follows that the effect of the flexibility of the wooden table would be greater for the Coast Survey pendulum in the ratio of $\frac{6308}{3050} \times \frac{1}{1.77} \times (-\,1.3) = 1.98$; so that the correction for our pendulum swinging on this table will be $0^{mm}.217 + 0^{mm}.071 = 0^{mm}.287$.

For the Geneva support, set up as it was at Hoboken, the total value of S (for metallic part and piers) was found to be $0^{mm}.00405$. For the stiffest support the total value was $0^{mm}.0031$.

M. Plantamour has observed a phenomenon which he supposes to be due to the pendulum-support not yielding so much under the oscillations of the pendulum as the amount calculated from the statical flexure. For a wooden support the hypothesis is certainly admissible. For a metallic support it should only be admitted with extreme caution. Elaborate experiments at Hoboken seem to show the ratio of statical to dynamical flexure to be as 263 to 257. Whether or not, supposing the difference to exist, the statical or dynamical flexure ought to be used in calculating the correction has not been made out by any mathematical analysis. In the corrections applied in this research the statical value has been used.

LENGTH OF THE PENDULUM.

The distance between the knife-edges of the pendulum depends upon the comparisons shown in the following scheme:

Pendulum.

United States Pendulum-Metre.

German Pendulum-Metre. German Normal Meter No. 49.

German Normal Meter No. 1605. German Platinum Metre.

The comparisons between the German metres were made in the Imperial Eichungsamt at Berlin, and the results communicated by Professor Förster. In addition, Bruhns's Report on Professor Albrecht's Pendulum Experiments gives a comparison between the German pendulum-metre and what he calls "Der Normalmeter der Eichungskommission," also made at the Eichungsamt in Berlin; but as it is impossible to tell specifically what he means by this expression, as the German Eichungskommission states that it never had any intention of establishing an independent standard of its own, and as the result given by Bruhns disagrees entirely with the results of other comparisons, this comparison must be excluded. It is possible that the German pendulum-metre received some injury between the time of Bruhns's comparison and that with No. 1605, communicated by Professor Förster. If such was the case, the injury probably occurred before the first comparison between it and the United States pendulum-metre, for, as then noticed, when the position had been adjusted by means of the spirit-level, the scales at the top and bottom of the tube were not in the same vertical line.

One set of comparisons between the two pendulum-metres was made in 1875, and another in 1877, by means of the vertical comparator belonging to the United States apparatus.

Meter No. 49 in its comparison with the pendulum-metre, and with the pendulum, was supported at the bottom on a foot made for the purpose.

Comparisons between the pendulum and pendulum-metre were made before and after every swing, at first; but at Kew and Hoboken it was thought sufficient to make one comparison before and one after each change of knife-edges, in addition to those made at the beginning and end of the series.

The order in which micrometrical readings were taken in these comparisons was as follows:

1. Metallic thermometer.
2. Metre below.
3. Metre above.
4. Pendulum above, bright edge.
5. Pendulum above, dark edge.
6. Pendulum below, bright edge.
7. Pendulum below, dark edge.
8. Metre above.
9. Metre below.
10. Metallic thermometer.

The method of adjustment of the stand and comparator has been already described in the first part of this report, under the head of "Instruments." The length of the comparator was made nearly a mean between those of the pendulum and pendulum-metre, and its middle brought to a level with the middle of the pendulum. The middle of the metre was then raised to the same height, its foot moved until the vertical lines at its two ends were simultaneously in coincidence with the vertical webs of the two microscopes, and both ends brought into perfect focus. The adjustment of the comparator, once made, was never disturbed; that of the focus on the standard was often repeated.

The lines of the metallic thermometer observed on were the three nearest the three lines one-tenth of a millimetre apart at the beginning of the metre-scale. Care was taken, however, to observe on the same three lines in every comparison at the same station. When the metallic thermometer was below, the reading of the metre taken in conjunction with it was not repeated; when it was above, at Berlin and Paris, the metre-scale above was read twice. At Kew, the metre-scale below was read first and last, and the scale above with the thermometer but once. A calibrated mercurial thermometer was read to hundredths of a degree Centigrade before and after each observation of the metallic thermometer. The corrections of this thermometer will be given in another place. The mark at the end of one metre, and the two lines at the distance of one-hundredth of a centimetre on each side, were observed on at Paris and Berlin; at Kew and Hoboken the lines observed on were those at distances of $99^{\text{cm}}_{\cdot}97$, $99^{\text{cm}}_{\cdot}98$, and $99^{\text{cm}}_{\cdot}99$ from the zero of the scale. Three readings on each line of the metre-scale, or nine in all, were taken at Paris; two readings on each line at Berlin and Kew, and but one at Hoboken, it being found advisable to finish the comparison as quickly as possible, that the apparatus might not be affected by the heat of the body. In the readings on the pendulum, the method followed at Geneva was used at all other stations. The order given above, of observing on edge bright and edge dark, was not at first strictly observed.

In reducing observations of length, account must be taken of the value of a revolution of the micrometer-screw, of the distance between the lines on the metre-scales (if the same ones were not always observed on), and of the varying compression of the scales compared when they supported varying weights.

In calculating the last-mentioned allowance, the coëfficient of elasticity of brass given by Wertheim, namely,

$$927100000$$

for one gramme of weight supported and one square cm of cross-section, was used.

Measures of the pendulum give as the cross-section of its tube 2.35 $(\mathrm{cm})^2$. The additional weight carried with heavy end down is 2095g; hence the pendulum is then longer by $\frac{2095}{2.35 \times 927100000} = 1^{\mu}_{\cdot}0$.

Taking the same cross-section for the tube of the pendulum-metre, and the weight of the metallic thermometer equal to that given by Bruhns for the thermometer of the German metre, 2213 g, its compression, when the thermometer is above, is $1^{\mu}_{\cdot}0$.

The cross-section of metre No. 49 is 1.80 $(\mathrm{cm})^2$ and half its weight 757 g; hence it is longer when measured horizontally than when measured vertically by $0^{\mu}_{\cdot}5$.

*[*282.33 . . . 301.38*]*

RESULTS OF OBSERVATIONS OF LENGTH.

*[*301.40 . . . 307.28*]*

The measures of length of the pendulum, as compared with the pendulum-metre, corrected for the value of micrometer-screw revolutions, reduced to heavy end down for the pendulum, and to metallic thermometer down and mean of lines 99.99, 100.00, and 100.01 of the metre, give for Pendulum *minus* U. S. pendulum-metre—

	μ
At Geneva	−198.7
At Paris	−181.2
At Berlin	−177.1
At Kew	−174.0
At Hoboken	−165.1

The injury the pendulum received between the Geneva observations and those at Paris, and the work necessary to remedy this injury, account for the great discrepancy between the length at Geneva and that found elsewhere. Taking the remaining measures, however, their discordance will be seen to be far greater than can be accounted for by errors of observation, while the progressive character of this disagreement renders it unlikely that it is due to the greater tightness of the screws holding the knife-edges at one place than at another. It is no greater than may be probably ascribed to accumulations of oxide, &c., under the bearing surfaces of the knives. The comparisons in detail are given in the following tables:

[308 . . . 312]

CONCLUDED LENGTH OF THE PENDULUM.

The indirect comparisons of the U. S. pendulum-metre with the German metre No. 49 give the following result, when reduced to 15° C:

	μ
U. S. metre − German pendulum-metre	+131.6
German pendulum-metre − No. 1605	−103.1
No. 1605 − No. 49	− 40.7
U. S. metre − No. 49	− 12.2
Direct comparison, U. S. metre − No. 49	− 20.0

The mean of these values, or $-16^{\mu}.1$, is taken, the likelihood of error of the two methods being estimated as equal. The bad temperature conditions at New York have prevented a more accurate determination of this quantity; but a new determination will be undertaken at the first opportunity.

But we have, for 15° C:

No. 49 − M = +277.2, hence
U. S. pendulum-metre − M = + 261.1

The length of the pendulum at the different stations is, therefore, as follows:

	cm.
Geneva	100.00624
Paris	100.00799
Berlin	100.00840
Kew	100.00871
Hoboken	100.00960

CENTRE OF MASS.

The quantity $h_d - h_u$, or twice the distance of the centre of mass of the pendulum from the centre of figure, was observed at the beginning and end of each series of experiments, and also before and after each transposition of knife-edges. The apparatus, method of observing, etc., have been elsewhere described.[3] Comparisons at the U. S. Office of Weights and Measures show that the 39 centimetres on the staff of the balance, from 17 to 56, are 0.14 mm too long. This correction applied, we obtain for $h_d - h_u$,

	cm.
At Geneva	39.312
At other stations	39.320

These are the values used. The separate observations are shown in the following table:

*[*314 . . . 315*]*

PERIODS OF OSCILLATION AND VALUES OF GRAVITY.

The pendulum was swung in Hoboken in various ways, to wit:

1. The regular set was made on the Geneva support with the bells off. This set cannot be compared with others on the Repsold support, if the reductions be made on the principle of the reversible pendulum, owing to the different ways in which the knives rest on the two supports. The comparison may, however, be made on the principle of the invariable pendulum, so as to eliminate this effect.

2. Sets of experiments were made at various pressures on the Geneva support with the bells on. The knife-edges not having been interchanged, these are strictly only comparable among themselves.

3. Half a regular set was made on the Geneva support with the bells on at about 35° C. There were a few additional experiments at this temperature with heavy end up at different pressures.

3. The idea of determining the centre of mass of a reversible pendulum, instead of moving a weight upon it, belongs exclusively to Bessel.

4. The pendulum was swung on the Repsold support and also on a very stiff support having the head of the Repsold support as a part of it (so as to have the same bearing on the knives). The object of these experiments was to determine the effect of flexure of the support.

A conspectus of all these experiments is given in the following table:

Periods of oscillation of the pendulum at Hoboken; reduced to one absolute atmosphere and to 15° *C, and to values on rigid support without bells or cylinder.*

Heavy end down.					Heavy end up.					
Pressure.	Temp. C.	T_d	No. days.	No. thousand oscillations.	Pressure.	Temp. C.	T_u	No. days.	No. thousand oscillations.	
				ON GENEVA SUPPORT.						
				With bells off.—Regular set to determine gravity.—Knives interchanged.						
in.	°	*s.*			*in.*	°	*s.*			
30	20	1.006344	8	42	30	20	1.006537	8	18	
				With bells on.—At high temperatures.						
in.	°	*s.*			*in.*	°	*s.*			
30	35	1.006346	4	22	30	35	1.006544	4	19	Knives interchanged.
					30	34	1.006530	2	6	Knife No. 1.
					2 ¼	38	1.006533	1	5	Knife No. 1.
					1 ¼	34	1.006521	1	2	Knife No. 1.
				With bells on.—At various pressures.—Knife No. 1 *at heavy end.*						
in.	°	*s.*			*in.*	°	*s.*			
30	18	1.006349	3	16	30	10	1.006539	3	13	
					29	11	1.006560	2	7	
					27	10	1.006537	1	4	
					22 ½	11	1.006549	1	2	
15	18	1.006337	2	20	15	10	1.006545	1	3	
					7 ½	10	1.006533	2	10	
5	20	1.006336	1	15						
1 ½	20	1.006342	1	21						
					1	10	1.006524	1	11	
					¾	9	1.006530	4	53	
					4/10	9	1.006532	4	73	
				ON REPSOLD SUPPORT.						
				One day, knife No. 1 *at heavy end; two days at light end.*						
in.	°	*s.*			*in.*	°	*s.*			
30	14	1.006355	3	4	30	14	1.006516	3	4	
				ON STIFFEST SUPPORT.						
				Knife No. 1 *at light end.*						
in.	°	*s.*			*in.*	°	*s.*			
30	14	1.006366	1	2	30	15	1.006544	1	2	

The reductions in the following table have been made with the *à priori* constants of atmospheric effect, and with the coëfficient of expansion, $18^{\mu}38$ per degree Centigrade. A correction of $+ 73 \times 10$ for inequality of knives has been applied to the three last results with heavy end up at high temperatures.

The agreement of the several experiments of the regular set is shown by the following table of the observed periods (uncorrected for the effect of the cylinders and of flexure):

Hoboken.—Regular set.

	Obs. T_d	Obs. T_u
	s.	*s.*
	1.006352	1.006559
	352	560
	361	546
	360	558
	358	544
	350	539
	363	551
	356	534
Mean. . .	1.006357	1.006548

The agreement of the several experiments of the half set at high temperatures is shown in the following table:

Hoboken.—Half set at high temperatures.

Obs. T_d	Obs. T_u
s.	*s.*
1.006533	1.006709
541	713
536	708
534	708
	706
	716
	708
	709

It will be seen that the mean results of the experiments of the regular set agree as well as could be expected with those made at high temperatures; which shows both that the coëfficient of expansion is correct, and also that the correction for bell-glasses is happily not in error. Further to compare these two sets of experiments we may calculate the mean T^2 which is to be used when the reduction is made on the principle of the reversible pendulum, and also the mean T^2 which is to be used when the reduction is made on the principle of the invariable pendulum. Denoting the former by [T^2 Rev.] and the latter by [T^2 Inv.] we have algebraically

$$[T^2 \text{ Rev.}] = \frac{T_d^2 h_d - T_u^2 h_u}{h_d - h_u} \qquad [T^2 \text{ Inv.}] = \frac{T_d^2 h_d + T_u^2 h_u}{h_d + h_u}.$$

The values will be

	[T^2 Inv.] s^2	[T^2 Rev.] s^2
From regular set	1.012846	1.012410
From $\frac{1}{2}$ set at high temperatures	1.012850	1.012399
Difference, in seconds, per day	0ˢ.3	0ˢ.5

We may next examine the experiments at various pressures. Their concordance with one another is very good; but the reader will hardly desire to see the single experiments set forth here; particularly as all the means are given in the tables at the end of this paper. We may exhibit [T^2 Inv.] and [T^2 Rev.] for pairs of experiments under nearly the same pressure but in reversed positions. But these results can have no value in the determination of gravity; nor can they be expected to accord with those just obtained, for, not to speak of the non-interchange of knife-edges, the observations in the two positions were taken at intervals of months, under conditions very different in many respects, and were never intended to be used for obtaining gravity, but only to show the variation of the period and decrement of arc with the pressure.

	[T^2 Inv.]	[T^2 Rev.]
From experiments at 30 inches down and 30 inches up	1.012855$(s)^2$	1.012425$(s)^2$
From experiments at 15 inches down and 15 inches up	1.012842	1.012376
From experiments at 5 inches down and $7\frac{1}{2}$ inches up	1.012832	1.012389
From experiments at $1\frac{1}{2}$ inches down and 1 inch up.	1.012836	1.012424
From mean of experiments at $\frac{1}{2}$ and $\frac{1}{4}$ inch down and experiments at $\frac{4}{10}$ inch up. . . .	1.012841	1.012415

The agreement is sufficient to show that the coëfficient of atmospheric pressure is well determined.

We pass now to the experiments on the Repsold support and on the stiffest support. These were not very carefully made, being only intended to show that the effect of flexure was really what calculation had predicted. Upon these supports the knife-edge rested on steel instead of glass, and consequently the reductions on the principle of the reversible pendulum are not comparable with results of experiments on the Geneva support until the slip shall have been measured on both stands. The reductions on the principle of the invariable pendulum are, however, comparable.

	[T^2 Inv.]	[T^2 Rev.]
From experiments on stiffest support	$1.012879(s)^2$	$1.012512(s)^2$
From experiments on Repsold support	1.012855	1.012514
From regular set	1.012846	
Δ Repsold and stiffest support, in seconds, per day	$1^s.0$	$0^s.1$
Δ Repsold support and regular set	$0^s.6$	

We may now proceed to compare the results at the European stations. First, the results of the single experiments at each station will be compared, with only such corrections as vary from day to day. Next, the values of [T^2 Inv.] and [T^2 Rev.] will be given for each station after correcting them for the wear of the edges so as to reduce them to what they would have been for Paris, just after the knives had been ground. Lastly, we shall use the determinations by the principle of the reversible pendulum of the absolute length of the seconds pendulum (still uncorrected for slip) at each station, in combination with the determinations of relative gravity on the principle of the invariable pendulum, in order to find four independent values of the length of the seconds pendulum at each station. These, being corrected for elevation, will be comparable with the results of other experiments. The Hoboken experiments on the Geneva support cannot be used to determine [T^2 Rev.] until the slip has been ascertained; those made at Hoboken, on the Repsold support, must, therefore, be used in place of them for the present.

Paris.—Periods of oscillation.

	Heavy end down.	Heavy end up.
	s.	*s.*
	1.006051	1.006192
	048	210
	047	190
	048	195
	048	185
	052	185
	062	208
	053	213
Mean. . .	1.006051	1.006197

The results on the last two days at Paris were affected by excessive damp.

Berlin.—Periods of oscillation.

	Heavy end down.	Heavy end up.
	s.	*s.*
	1.005899	1.006052
	901	037
	896	026
	890	036
	901	037
	896	034
	899	046
	895	034
Mean. . .	1.005898	1.006038

Kew.—Periods of oscillation.

	Heavy end down.	Heavy end up.
	s.	*s.*
	1.005935	1.006077
	30	68
	29	73
	27	64
	25	70
	31	71
	28	64
	31	66
	29	70
	30	66
		59
		63
		72
		73
		66
		74
		77
		67
Mean. . .	1.005930	1.006069

The following table shows the results of Paris, Berlin, and Kew in comparison:

	[T^2 Inv.]	[T^2 Rev.]	Diff.
Paris	1.0121042$(s)^2$	1.0116956$(s)^2$	4086
Berlin	1.0117925	1.0113934	3991
Kew	1.0118560	1.0114548	4012

These differences involve double the square roots of the sums of the squares of the errors of the periods of oscillation with heavy end down and with heavy end up. Therefore, the Berlin and Kew differences are remarkably close together, while that at Paris is rather divergent. The experiments at Hoboken show a wide discrepancy in this difference, owing to the use of the Geneva support. Consequently the [T^2 Rev.] cannot be used.

Now, reducing the values of [T^2 Rev.] to mean solar time and dividing into the length of our pendulum at Paris we obtain the length of the seconds pendulum at the several stations. The ratios of the [T^2 Inv.] at the different stations, being inversely as the length of the seconds pendulum, may be used to obtain the length of the seconds pendulum at any station from the value deduced from the [T^2 Rev.] at any other station. So that at each station we shall have not only a value of the seconds pendulum deduced from the [T^2 Rev.] of that station but also two other values deduced from the [T^2 Rev.] of the two other stations. These three values will have nearly equal weight. They are as follows:

For Paris.	*m.*
From Paris observations	.9939390
From Berlin observations	02
From Kew observations	20
Mean	.9939337
Reduction to sea-level	+ 163
Seconds pendulum at Paris reduced to sea-level	.9939500

For Berlin.	*m.*
From Paris observations	.9942362
From Berlin observations	.9942452
From Kew observations	.9942382
Mean	.9942399
Reduction to sea-level	+ 83
Seconds pendulum at Berlin reduced to sea-level	.9942482

For Kew.	*m.*
From Paris observations	.9941757
From Berlin observations	.9941830
From Kew observations	.9941740
Mean	.9941776
Reduction to sea-level	+ 14
Seconds pendulum at Kew reduced to sea-level	.9941790

No comparison can be attempted between these results and those of previous experimenters until the former have been corrected for the slip of the knives and the latter have been reduced anew according to modern methods. These matters will be treated in the second part of this report with results which will be found satisfactory.

The pendulum at Geneva was virtually a different pendulum from that used at the other stations, because of the accident that befel it after the Geneva experiments. These experiments can, therefore, only be reduced on the principle of the reversible pendulum. The concordance of the single experiments is shown in the following table:

GENEVA.

T_d^2	T_u^2
1.012599	1.012814
581	775
589	797
607	793
593	789
580	767
582	763
598	783

The resulting value of the length of the seconds pendulum after correcting for flexure in the manner explained under that heading is

Length of seconds pendulum at Geneva.

	m.
Experiments of Coast Survey	0.993556
Professor Plantamour's result	0.993550

The appended tables show the details of the experiments at the different stations.

Respectfully submitted.

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Assistant.

[321 . . . 416]

A large number of repetitions of similar trials

MS 355: Winter-Spring 1880

Each trial may turn out *yes* or *no*

p is the probability of one trial being yes i.e. is the proportion of *yes*'s in an infinite number of trials

s is the number of trials

A possible difference of *proportion* of these trials from the theoretic *proportion* p is

$$d = t\sqrt{\frac{2p(1-p)}{s}}$$

Then θt is the probability of the actual difference being as small as that difference

But in practice $\frac{1}{2}$ must be added to $d \times s$ or $\frac{1}{2s}$ to d.

A die is thrown 60 times

What is the probability that there will be just 10 sixes?

$$p = \frac{1}{6} \qquad s = 60$$

$d \times 60$ is the difference of the *number* of sixes from 10, the theoretic number.

$$d \times 60 = \frac{1}{2} \qquad d = \frac{1}{120} = t\sqrt{\frac{2p(1-p)}{s}} = t\frac{1}{\sqrt{216}} = t\frac{2}{30}$$

This gives $t = \frac{30}{2}\frac{1}{120} = \frac{1}{8} = .125$

$$\theta t = .14$$

So that the probability of throwing just 10 sixes in 60 throws is .14 or $\frac{1}{7}$.

What is probability that there will be either 9 10 or 11 sixes?

$$d \times 60 = 1\frac{1}{2} \qquad d = \frac{3}{120} = \frac{1}{40} = t\frac{2}{30}$$

$$\therefore \ t = \frac{30}{2}\frac{1}{40} = \frac{3}{8} = .375$$

$$\theta t = .40$$

The probability of throwing either 9 10 or 11 sixes in 60 throws is 0.40 or $\frac{2}{5}$
Then the probability of throwing either 9 or 11 sixes is .26
Then the probability of throwing 9 sixes (= prob. of throwing 11) = .13

What is prob of throwing 8 9 10 11 or 12 sixes

$$d \times 60 = 2\frac{1}{2} \qquad d = \frac{5}{120} = \frac{1}{24} = t\frac{1}{15}$$

$$t = \frac{15}{24} = \frac{60}{96} = .62$$

$\theta t = .62$ which is the answer

Then prob of throwing either 8 or 12 sixes is $.62 - .40 = .22$
Prob of throwing 8 sixes (= prob throwing 12) = .11

What is prob of throwing either 7 8 9 10 11 12 or 13 sixes

$$60 \times d = \frac{7}{2} \qquad d = \frac{7}{120} = t\frac{1}{15}$$

$$t = \frac{7 \times 15}{120} = \frac{7}{8} = .875$$

$\theta t = .78$ which is answer

Prob of 7 or 13 sixes is $.78 - .62 = .16$
Prob of 7 sixes = .08

What is prob of 6 to 14 sixes?

$$t = \frac{9}{8} = 1.125$$

$$\theta t = .89\text{—which is answer}$$

Prob of 6 or 14 sixes = .89 – .78 = .11 —
Prob of 6 sixes = .05

What is prob of 5 to 15 sixes

$$t = \frac{11}{8} = 1.375$$

$$\theta t = .95 \text{ which is answer}$$

Prob of 5 or 15 sixes = .95 – .89 = .06
Prob of 5 sixes = .03

What is prob of 4 to 16 sixes

$$t = \frac{13}{8} = 1.625$$

$$\theta t = .98\text{—answer}$$

Prob of 4 or 16 sixes = .98 – .95 = .03 —
Prob of 4 sixes .01

On the Value of Gravity at Paris

P 256: **Coast Survey Report 1881,** ***461–63***

The very good agreement between the figures given by Borda and Biot for the value of gravity at Paris, and the quantity found by Kater at London, reduced to Paris by means of the transportation of invariable pendulums, gives us great confidence in the exactness of the result.

The three values for the length of the seconds pendulum are as follows:

	mm.
Borda	993.827
Biot	993.845
Kater	993.867

However, it might be supposed that this agreement was merely the result of chance. It is known, in fact, that none of these numbers have received the correction for the inertia of the air drawn along by the pendulum, a correction which was first made by Bessel. Now, it is not necessarily to be supposed, before having made the computations, that this correction should be the same for all three pendulums; Borda's being composed of a platinum ball and an iron wire 4 metres long; Biot's being the same platinum ball, to which was attached a copper wire $0^{m}.6$ long; and that of Kater being made of brass, and irregular in form. But the effect of the atmosphere upon a sphere suspended by a thin cylinder can be exactly calculated by the formulæ which Mr. Stokes has given in his important memoir on this subject. Two elements unite in producing this effect; one arises from simple atmospheric pressure, and the other from that property of the air which the English physicists call *viscosity*, and the Germans *internal friction.* To calculate this last element we must take the value of the viscosity of the air given by modern experiments, those of Maxwell, for instance. Stokes adopted a value for the viscosity much too small. This affects especially the values expressing the effects of the *viscosity* on the suspended wires, and this is why his comparisons between observation and theory do not show the true value of the latter. The atmospheric effect on the caps attaching the platinum ball, and on the sides of the chamber in which the pendulums of Borda and Biot were swung, can be approximately calculated. Of course these corrections are confirmed as well by the observations of the pendulums at different pressures as by analysis.

Biot's observations were also affected by the oscillation of the supports. In regard to the supports used by Borda, according to his description, I believe them to have been very solid, and the correction to be applied to the value of gravity, being inversely proportional to the length of the pendulum employed, must be small in this case. Biot's supports are still at the observatory; however, they have received two modifications: 1st, the sides have been strengthened by two cross-pieces; 2nd, the piece which held the pendulum has been replaced by another, which is very solid. With the kind permission of Admiral Mouchez I took off the cross-pieces and measured the flexure of the supports (still provided with the new head), subjected to the effect of a force of 2 kilos. and 5 kilos., applied in a horizontal direction.

The following are the measures—

	Displacement with 2 kilos.		Displacement with 5 kilos.
	μ		μ
	13.5		34.8
	12.9		34.8
			35.5
Mean,	13.2		35.6
Per kilo.,	6.6		35.2
		Mean,	35.2
		Per kilo.,	7.0

In order to appreciate the effect produced, not by the large support, but by the little piece which supported the pendulum in Biot's experiments, a careful experimental study will be necessary, aided with the application of a theory entirely different from that which is applicable to elastic supports. For the present I neglect this effect.

Applying the other corrections I get the following numbers:

	Borda.	Biot.
Length given.	993827	993845
	μ	
Hydrodynamic effects.	31.4	31.4
Viscosity (sphere) . . .	35.0	23.1
Viscosity wire	22.6	1.8
Effect of caps.	2.1	6.2
Effect of sides	0.2	0.2
Flexure (known part) .	—	5.0
Corrected length . .	993918.0	993913.0
New measure	993934	

If we adopt seven microns as the effect of the unknown part of the flexure of Biot's support, it will be seen that far from weakening our confidence in the exactness of the observations of those illustrious physicists, our corrections only bring into agreement their results. The number expressing the result of my experiments (993.934) differs sensibly from the others. Nevertheless, a careful study of all sources of error has convinced me that it is correct within 10 microns.

The length of the seconds pendulum at Paris, calculated from the experiments of Kater, is $0^{\text{m}}_{.}99387$; that is, shorter than my determination by $0^{\text{mm}}_{.}07$. If we place confidence in the experiments of General Sabine, made with Kater's pendulum at different pressures, we must add to his measure a correction not less than $0^{\text{mm}}_{.}16$, which is two times too great for the agreement of the determinations.

But General Sabine made too few experiments to establish so improbable a result. We can, therefore, assert nothing from the experiments of Kater. In any case, I think, I have sufficiently proved by what precedes, that the number heretofore given for the value of gravity at Paris must be increased by its 1 ten-millionth part.

JUNE 14, 1880.

NOTE.

In my report upon the "Measurement of Gravity at Initial Stations," the unit of measure used is derived from the German Normal Meter No. 49. But Professor Förster has communicated new data with reference to the correction of that bar, in consequence of which it appears that the assumed metre of my publication was 16.6 microns too short. In an article in the *American Journal of Science,* Vol. XX, October, 1880, it is stated that the United States Office of

Weights and Measures makes the same metre 19.2 microns too short. But this statement assumed the committee metre to be correct. According to Barnard and Tresca, however, this metre is 3.4 microns too long. The metre of my paper is, therefore,

By the German comparisons, 16.6
By the American comparisons, 15.8

too short. Applying the mean of these corrections, my value of the seconds pendulum at Paris becomes $0^{\text{m}}.9939175$, which is substantially identical with the value from Borda's corrected experiments, and is probably very close to the correct conclusion from Biot's work.

[On the State of Science in America]

MS 363: June–July 1880

Gentlemen—In those fourth of July reunions of Americans on foreign soil it is usual to cast up our accounts and see how our nation prospers and what entire liberty with a boundless & teeming soil has done for us. It is usual enough to indulge in these occasions in self-glorification at our successes and it is equally useful to submit ourselves to a little self-humiliation at our shortcomings.

It falls to me this evening to report to you how Science has prospered in the United States since the Declaration of Independence, —and it is not an altogether agreeable report to make or to hear. I do not think that any observant sojourner in many lands can fail to perceive that the Americans are not merely an intelligent but a positively intellectual people, and yet some how or other modern science which has been the most glorious result of the past hundred years,—one of the most, perhaps the most glorious work of man—a great part of which has been performed in the century since the Declaration, in this great work of building up modern science America has had almost no part or lot at all.

Monsieur de Candolle who has devoted a thick volume to a careful impartial and masterly appreciation of the rank of the different nations in science places America not only after Switzerland, Belgium, & Holland,—after France, Germany, and England,—but even after Italy, and only a little in advance of Spain and Russia.

When the revolution broke out we had two eminent scientific men: Benjamin Franklin, and Benjamin Thompson afterward Count Rumford. The former as you know was one of the creators of the science of statical electricity. The other was the one undoubted father of the great mechanical theory of heat,—after that of gravitation the greatest discovery that has been made in physics. What a part the theories of electricity and still more of heat have played in the science of the nineteenth century! And how little Americans have done for them since Franklin & Rumford.

Of the magnificent series of discoveries in electricity until within a year we could only claim one as our own. Notwithstanding that the electric telegraph was invented & so widely used in America, the laws of its action were all discovered elsewhere. So with heat. From the study of the steam engine, has sprung the modern science of heat. In America where so many engines were made one would have expected its theory to have been found out—but not so. Although Rumford left a liberal fund for the prosecution of such experiments not a particle of the glory of this great theory belongs to us.

Now, gentlemen, you may think that this is not the occasion to say so much ill of America; you may doubt its truth. It *is* true; so true that no man qualified by knowledge & absence of prejudice to give an opinion, can tell you otherwise. And for my part, gentlemen, my faith in & love for America is not of so Skin-deep a Kind that I am afraid to look her faults in the face. America!—that is you & I; and I have no admiration for the character that does not wish to look his own faults in the face.

I have not called attention to our backwardness in science merely to complain of it, but in order to seek the cause of it & to point out the cure for it,—a cure which is in the hands of the general public more than in those of the scientific men.

Why then has science made so little progress in America? One can certainly *not* attribute it to our popular institutions. For the most scientific nation of ancient times was Greece & that of modern times is Switzerland. It is common to say that it is because we are so young a people. That we have had to clear the fields & to commence society & that things have had to be done in the rough at first. That would be a very plausible excuse certainly. But in point of fact in spite of our having had to hew our forests and to mould the beginnings of civilization, we do *not* work in the rough, at all; it is one of our greatest triumphs that nowhere is machinery & all artisanship so perfect as in the United States. Now physical science is nothing but the theory of artisanship, machinery, and so forth; and it is surprizing that that does not advance where the mechanical execution is so perfect.

But I think I can tell you why it is, that science has not gone hand in hand with mechanism in our country. It is that the people have made a distinction in their minds between *practical* & *theoretical* persons, the former experienced, not in the thing they undertake to do, but in business generally & densely ignorant about all science,—the latter a sort of pedants who never succeed in getting any thing really done, who do not even advance the science they profess.

This is the peculiarly American distinction between practical and theoretical men,—a distinction perfectly unknown in France, for example, because men do not really fall into those classes but on the contrary the highest men of theoretical science are the most practical of men,—their theory & practice perpetually helping one another. But in America the distinction is drawn because it exists. It exists because the scientific men are attached to Colleges and because the Colleges of our country are pedantical & pedagogical institutions where the prosecution of original scientific researches far from being required of a Professor or from raising his standing in the College is positively frowned upon as tending to interfere with his proper pedagogic activity. I know how it is in Harvard where I was brought up and where I well remember the inaugural address of the present president—a recognized enemy of science—setting forth those ideas most clearly. Columbia I also know of,—and I know that science, I mean real scientific research, is barely tolerated there,—or not tolerated where it can be got rid of. In New Haven, the Scientific school was run for years as a private enterprize of the professors and the college only took charge of it when it began to be pecuniarily profitable.

//If you ask why our colleges have been in this state, the answer is very simple. It is that they have been in the hands of the clergy who in all ages and in all countries have comprehended the nature of science,—with its single eye for truth—as little as they have the worldly code of honour.

/If you ask why our colleges have been in this state, the answer is very simple. It is that they have been in the hands partly of clergymen partly of other unscientific persons, to whom the spirit of science with its single eye for the truth, without partisanship, prepossession, or other passion than that of research, was perfectly foreign and unknown.//

The exception is said to prove the rule. And it will be found that the exceptions to the general low state of science in America are precisely the cases not covered by the explanation which I offer you of it. For instance, Astronomy is a science in which America has stood high. To speak only of discoveries which are generally intelligible, over $\frac{2}{5}$ of all the minor planets of our system discovered in the last twenty years and all the moons of Planets except one since Herschel's discoveries have been discovered in America. The true account of Saturn's rings, the most accurate description of the texture of the Sun's surface and the best drawings of celestial objects all come from America. Now the chief reason why we have been so successful in astronomy is that though the observatories are usually attached to colleges yet they are loosely attached. They are chiefly managed by the Astronomers who are not overburdened with teaching.

One university in our country, the Johns Hopkins university of Baltimore has been carried on upon principles directly contrary to those which have governed the other colleges. That is to say, it has here alone been recognized that the function of a university is the production of knowledge, and that teaching is only a necessary means to that end. In short, instructors and pupils here compose a company who are all occupied in studying together some under leading strings and some not. From this small institution, with half a dozen professors and a hundred and fifty students, I am unable to tell you how much valuable work has emanated in the 4 years of its existence in philology & biology. A great deal I am sure. With its work in mathematical & physical science I am better acquainted & I am proud to say—because it shows the real capability of America for such work—that in those 4 short years the members of this little university have published some 100 original researches, some of them of great value,—fairly equal to the sum of what all the other colleges in the land have done (except in Astronomy) in the last 20 years. One discovery alone by Mr. E. H. Hall of an entirely new effect of electricity, is distinctly the most fundamental addition to physical science which has anywhere been made in many years.

In whatever branches of science are touched by the works of the Scientific Institutions of our Government such as the Smithsonian Institution, the Coast Survey, the Geological Surveys, the National Observatory, the Signal Bureau, the Fish Commission, the Army Medical Museum, the Office of Weights & Measures etc. our science stands in the very first rank,—and we are teaching the world.

Nobody has ever thought that these institutions needed civil service reform, for politics has never meddled with them at all. When we shall create a great government university to be governed as these institutions are, as one of these days we must, then shall American science for the first time at length begin to take the standing which we ought to expect it to take. One of the great parties has nominated for the presidency a man who by the exactitude of his studies,—though they are not of a physical nature—is entitled to the name of a man of science. The inauguration of a great national university, fit crown of the work of a great party of enlightenment, could not be more appropriately performed than under the auspices of President Garfield!

Letter, Peirce to Hervé Faye

P 215: **Verhandlungen der Europäischen Gradmessung**
(Berlin: Georg Reimer, 1881), pp. 84–86

Cambridge, le 23 Juillet 1880.

Cher Monsieur,

Je reçois de la santé de mon père des nouvelles tristes qui m'obligent à repartir immédiatement pour l'Amérique. Il m'est donc impossible d'assister à la conférence de Munich. Voulez-vous me permettre de vous prier de lire pour moi la note suivante à la séance de l'association et de la remettre aux secrétaires pour la faire insérer dans les Comptes-rendus.

Le bureau central a demandé qu'on lui communiquât ce que l'on pensait au sujet du meilleur appareil pour l'établissement du pendule. Pendant quelque temps mon opinion a été que pour déterminer la pesanteur absolue, le mieux était des pendules longs et courts oscillant dans le vide, et ça m'a été une grande satisfaction d'apprendre que vos recherches vous avaient conduit au même résultat. Mais pour déterminer la pesanteur relative je préfère un pendule invariable. La raison en est bien simple: c'est qu'avec ce procédé on n'a besoin de déterminer qu'une seule quantité au lieu de deux; je pourrais même dire trois. En effet, avec les pendules longs et courts, comme avec le pendule à réversion, il faut déterminer deux périodes d'oscillation et une longueur. Or, l'expérience m'a appris que c'est la mesure de la longueur qui dans les stations de campagne est souvent la partie la plus difficile de l'opération, à cause de la difficulté qu'on éprouve à avoir à la fois une température constante et un éclairage suffisant. Les objections contre l'emploi du pendule invariable sont, je crois, les suivantes:

1° Il exige une étude des influences atmosphériques;
2° La comparaison des pendules invariables et la vérification de leur invariabilité sont difficiles.

Mais tout moyen d'éviter l'étude des influences atmosphériques implique des combinaisons de quantités observées multipliant les erreurs. Je crois donc que l'étude des influences atmosphériques vaut bien la peine qu'on y donne. L'expérience m'a également montré qu'il n'y avait pas le moindre inconvénient à employer la chambre pneumatique dans les stations de campagne. Si même l'appareil fonctionne mal dans quelque station, les coëfficients des influences atmosphériques auront déjà été donnés par les expériences faites à d'autres stations. Je donnerais au pendule une forme cylindrique régulière de façon à pouvoir calculer aisément les effets de la pression et de la viscosité de l'atmosphère.

Ce n'est pas, que je sache, un trait particulier aux pendules invariables, qu'il soit nécessaire de les comparer entre eux; car les résultats obtenus au moyen des instruments différents employés pour obtenir la pesanteur absolue doivent aussi être comparés entre eux, et comme chacun sait, ils présentent parfois des écarts considérables. Le danger d'accidents pouvant modifier les pendules invariables est une difficulté sérieuse. Mais j'y obvie en faisant mon pendule à la fois invariable et à réversion. Toute altération du pendule serait immédiatement révélée par le changement dans la différence des deux périodes d'oscillation dans les deux positions. Une fois découverte, il en serait tenu compte au moyen de nouvelles mesures de la distance entre les soutiens. Il faudrait peut-être prendre ces mesures à toutes les dix stations. En résumé, il me semble que si le pendule à réversion n'est peut-être pas le meilleur instrument pour déterminer la pesanteur absolue, c'est, à la condition au moins qu'il soit réellement invariable, le meilleur pour déterminer la pesanteur relative. Je voudrais qu'il fût formé d'un tube de laiton tiré de $0^{m}03$ de diamètre avec des bouchons lourds de laiton également tiré. Le cylindre serait terminé par deux hémisphères, les couteaux seraient attachés à des bagues fixées près des extrémités du cylindre. Le centre de gravité doit être cinq fois plus éloigné d'un couteau que de l'autre.

J'offrirai quelques remarques sur les détails spéciaux touchant la construction d'un appareil de pendule.

M. *Villarceau* et moi, nous avons séparément conseillé de faire tourner le pendule sur des cylindres de $0^{m}005$ de diamètre. J'ai eu quelques cylindres de ce genre merveilleusement construits, mais qui m'ont entièrement désappointé. Le pendule suspendu de cette façon s'arrête très-promptement et le cylindre perd vite son poli au point de contact. Mes expériences m'ont fait rejeter absolument cette idée si séduisante à première vue.

Je persiste à croire que le couteau doit faire partie intégrante des supports et que les plans doivent faire partie du pendule.

Tous les modes dont j'ai essayé d'observer la période d'oscillation laissent quelque chose à désirer. J'ai fait un grand nombre d'expériences sur différentes manières d'observer les coincidences, et les résultats ont été, dans tous les cas, défavorables. On ne peut en effet observer avec précision une coincidence qu'à l'aide de dispositions plus ou moins compliquées qui exigent la construction de nouveaux piliers. L'éclairage momentané du pendule par l'étincelle électrique à chaque battement du chronomètre ou de l'horloge est rarement commode. La principale objection à l'enregistrement chronographique des passages observés est que la lecture des signaux exige un travail considérable. L'instrument automatique construit pour moi par M. *Breguet* m'a donné d'excellents résultats, et je l'employerais d'ordinaire si j'avais un bon chronoscope de Hipp.

Si j'avais pu aller à Munich, j'aurais demandé à donner lecture à l'association d'un mémoire sur la flexion des supports.

Ce mémoire rend compte de nombreuses sortes d'expériences sur la flexion statique et dynamique de divers supports et sur les périodes d'oscillation des pendules y attachés. J'examine les moyens propres à mesurer la flexion et je montre la supériorité de la méthode optique. Je démontre que la différence entre la flexion statique et la flexion dynamique est insignifiante quand les supports ont été judicieusement établis, et que la flexion statique représente aussi bien que la flexion dynamique l'effet produit sur la durée de l'oscillation; que c'est une erreur de croire que la flexion soit sensiblement modifiée par la suspension d'un poids médiocre ou considérable. Je décris les curieux effets qu'on obtient en serrant ou en relâchant les écrous qui relient les pièces du trépied Repsold et comment le relâchement peut, dans certaines circonstances, diminuer l'effet de la flexion. Il y a un rapport entre ce fait et celui que j'avais établi dans ma première communication, à savoir que la flexion n'est pas rectiligne, de sorte qu'elle diffère sensiblement pour des parties de la tête des supports éloignées d'un petit nombre de millimètres. Les déterminations obtenues sans qu'on eût tenu compte de cette circonstance, doivent être répétées.

Il sera difficile de se procurer en campagne un support sur lequel le fléchissement ne produise pas d'effet sensible. Il serait certainement imprudent d'admettre que sur un certain support cet effet est nul, sans avoir essayé d'en obtenir quelque preuve expérimentale. Un pendule renversé de Hardy suffira peut-être pour cela, mais il me paraît plus sûr de mesurer la flexion.

Je travaille depuis longtemps à une recherche sur l'importance relative des différentes sources d'erreur dans les expériences de pendule.

Veuillez, je vous prie, Monsieur, être l'interprète auprès de l'association de ma gratitude pour le concours aimable et empressé que j'ai rencontré chez tous ses membres et agréez vous-même l'assurance de mon profond respect.

A Monsieur *Faye,*
Membre de l'Institut
etc. etc. etc.

C. S. Peirce,
Assistant U. S.
Coast & Geod. Survey.

On the Colours of Double Stars

P 179: Nature *22 (29 July 1880): 291–92*

If any light whatever has its intensity increased the effect on the eye is to add to the sensation a certain yellow element which I have accurately defined by experiment (*Am. Jour. Sci.,* April, 1877, vol. xiii. p. 247). A red light brightened becomes yellower, a green light yellower, a yellowish white less white, a blue or violet light whiter. The phenomena are described at length in Prof. Rood's *Modern Chromatics.* The fact that an incandescent body becomes less red and more yellow when it is heated is probably due to this physiological principle. That the incandescent body ultimately becomes white is probably owing to some not understood modification of the principle for excessively bright lights.

It follows that if two stars are of unequal brightness they will appear of different colours unless the qualities of the two lights have a peculiar relation to one another; and the brighter star will usually be the yellower. Accordingly, if we refer to Mr. Burnham's lists of binaries recently published by Prof. Holden (*Am. Jour. Sci.,* June, 1880, vol. xix. p. 467) we find that although differences of colour are so little distinguished that three-quarters of all the pairs are considered to be of the same colour, yet of the twenty-four pairs which differ in brightness by two magnitudes or over, not one is considered to have components of the same colour. And of the forty-two pairs which are said to be of different colour all but two have more yellow in the brighter, so much so indeed that it is possible to suppose that the difference of brightness is the chief cause of the difference of colour. The two exceptions are:—

No. 23.	ϵ *Boötis*	A. eq. Cærulea	B. eq. Cærulea
No. 42.	OΣ 507	A. Blanche	B. Cendriolivatre

There is evidently some error about No. 23. Either the colours are wrong, or it is wrongly stated to have differently-coloured components. In No. 42 it is difficult to say which component is more yellow. Although, then, it is certain that other causes largely affect the colours of stars, yet differences of brightness seem to have the greatest effect in producing the apparent differences in the colours of double stars.

Prof. Holden compares the colours of bright and faint stars to those of a more or less hot incandescent body. But in the latter case the dimmer light is accompanied with redness. We know that this is not the case with the light of our own sun; for of a white surface, upon part of which the sun shines, while the rest is in shadow, the darker part is bluer. In the same way, of the forty binaries of which the brighter component is the yellower, there are thirty-seven in which the fainter is bluer, and only three in which it is distinctly redder. It appears, therefore, that most double stars do not differ greatly in colour from our sun, and do not shine with the strongly red light of an incandescent solid.

Paris, July 20

On the Algebra of Logic

P 167: **American Journal of Mathematics**
3 (1880): 15–57

CHAPTER I.—SYLLOGISTIC.

§1. *Derivation of Logic.*

In order to gain a clear understanding of the origin of the various signs used in logical algebra and the reasons of the fundamental formulæ, we ought to begin by considering how logic itself arises.

Thinking, as cerebration, is no doubt subject to the general laws of nervous action.

When a group of nerves are stimulated, the ganglions with which the group is most intimately connected on the whole are thrown into an active state, which in turn usually occasions movements of the body. The stimulation continuing, the irritation spreads from ganglion to ganglion (usually increasing meantime). Soon, too, the parts first excited begin to show fatigue; and thus for a double reason the bodily activity is of a changing kind. When the stimulus is withdrawn, the excitement quickly subsides.

It results from these facts that when a nerve is affected, the reflex action, if it is not at first of the sort to remove the irritation, will change its character again and again until the irritation is removed; and then the action will cease.

Now, all vital processes tend to become easier on repetition. Along whatever path a nervous discharge has once taken place, in that path a new discharge is the more likely to take place.

Accordingly, when an irritation of the nerves is repeated, all the various actions which have taken place on previous similar occasions are the more likely to take place now, and those are most likely to take place which have most frequently taken place on those previous occasions. Now, the various actions which did not remove the irritation may have previously sometimes been performed and sometimes not; but the action which removes the irritation must have always been performed, because the action must have every time continued until it was performed. Hence, a strong habit of responding to the given irritation in this particular way must quickly be established.

A habit so acquired may be transmitted by inheritance.

One of the most important of our habits is that one by virtue of which certain classes of stimuli throw us at first, at least, into a purely cerebral activity.

Very often it is not an outward sensation but only a fancy which starts the train of thought. In other words, the irritation instead of being peripheral is visceral. In such a case the activity has for the most part the same character; an inward action removes the inward excitation. A fancied conjuncture leads us to fancy an appropriate line of action. It is found that such events, though no external action takes place, strongly contribute to the formation of habits of really acting in the fancied way when the fancied occasion really arises.

A cerebral habit of the highest kind, which will determine what we do in fancy as well as what we do in action, is called a *belief*. The representation to ourselves that we have a specified habit of this kind is called a *judgment*. A belief-habit in its development begins by being vague, special, and meagre; it becomes more precise, general, and full, without limit. The process of this development, so far as it takes place in the imagination, is called *thought*. A judgment is formed; and under the influence of a belief-habit this gives rise to a new judgment, indicating an addition to belief. Such a process is called an *inference;* the antecedent judgment is called the *premise;* the consequent judgment, the *conclusion;* the habit of thought, which determined the passage from the one to the other (when formulated as a proposition), the *leading principle*.

At the same time that this process of inference, or the spontaneous development of belief, is continually going on within us, fresh peripheral excitations are also continually creating new belief-habits. Thus, belief is partly determined by old beliefs and partly by new experience. Is there any law about the mode of the peripheral excitations? The logician maintains that there is, namely, that they are all adapted to an end, that of carrying belief, in the long run, toward certain predestinate conclusions which are the same for all men. This is the faith of the logician. This is the matter of fact, upon which all maxims of reasoning repose. In virtue of this fact, what is to be believed at last is independent of what has been believed hitherto, and therefore has the character of *reality.* Hence, if a given habit, considered as determining an inference, is of such a sort as to tend toward the final result, it is correct; otherwise not. Thus, inferences become divisible into the valid and the invalid; and thus logic takes its reason of existence.

§2. *Syllogism and Dialogism.*

The general type of inference is

$$\begin{array}{c} \mathrm{P} \\ \therefore\ \mathrm{C}, \end{array}$$

where $\therefore$ is the sign of illation.

The passage from the premise (or set of premises) P to the conclusion C takes place according to a habit or rule active within us. All the inferences which that habit would determine when once the proper premises were admitted, form a class. The habit is logically good provided it would never (or in the case of a probable inference, seldom) lead from a true premise to a false conclusion; otherwise it is logically bad. That is, every possible case of the operation of a good habit would either be one in which the premise was false or one in which the conclusion would be true; whereas, if a habit of inference is bad, there is a possible case in which the premise would be true, while the conclusion was false. When we speak of a *possible* case, we conceive that from the general description of cases we have struck out all those kinds which we know how to describe in general terms but which we know never will occur; those that then remain, embracing all whose non-occurrence we are not certain of, together with all those whose non-occurrence we cannot explain on any general principle, are called possible.

A habit of inference may be formulated in a proposition which shall state that every proposition c, related in a given general way to any true proposition p, is true. Such a proposition is called the *leading principle* of the class of inferences whose validity it implies. When the inference is first drawn, the leading principle is not present to the mind, but the habit it formulates is active in such a way that, upon contemplating the believed premise, by a sort of perception the conclusion is judged to be true.[1] Afterwards, when the inference is subjected to logical criticism, we make a new inference, of

1. Though the leading principle itself is not present to the mind, we are generally conscious of inferring on some general principle.

which one premise is that leading principle of the former inference, according to which propositions related to one another in a certain way are fit to be premise and conclusion of a valid inference, while another premise is a fact of observation, namely, that the given relation does subsist between the premise and conclusion of the inference under criticism; whence it is concluded that the inference was valid.

Logic supposes inferences not only to be drawn, but also to be subjected to criticism; and therefore we not only require the form $P \therefore C$ to express an argument, but also a form, $P_i \prec C_i$, to express the truth of its leading principle. Here P_i denotes any one of the class of premises, and C_i the corresponding conclusion. The symbol $\prec$ is the copula, and signifies primarily that every state of things in which a proposition of the class P_i is true is a state of things in which the corresponding propositions of the class C_i are true. But logic also supposes some inferences to be invalid, and must have a form for denying the leading premise. This we shall write $P_i \overline{\prec} C_i$, *a dash over any symbol signifying in our notation the negative of that symbol.*[2]

Thus, the form $P_i \prec C_i$ implies

either, 1, that it is impossible that a premise of the class P_i should be true,

or, 2, that every state of things in which P_i is true is a state of things in which the corresponding C_i is true.

The form $P_i \overline{\prec} C_i$ implies

both, 1, that a premise of the class P_i is possible,

and, 2, that among the possible cases of the truth of a P_i there is one in which the corresponding C_i is not true.

This acceptation of the copula differs from that of other systems of syllogistic in a manner which will be explained below in treating of the negative.

In the form of inference $P \therefore C$ the leading principle is not expressed; and the inference might be justified on several separate principles. One of these, however, $P_i \prec C_i$, is the formulation of the habit which, in point of fact, has governed the inferences. This principle contains all that is necessary besides the premise P to justify the conclusion. (It will generally assert more than is necessary.) We may, therefore, construct a new argument which shall have for its prem-

2. This dash was used by Boole, but not over other than class-signs.

ises the two propositions P and $P_i \prec C_i$ taken together, and for its conclusion, C. This argument, no doubt, has, like every other, its leading principle, because the inference is governed by some habit; but yet the substance of the leading principle must already be contained implicitly in the premises, because the proposition $P_i \prec C_i$ contains by hypothesis all that is requisite to justify the inference of C from P. Such a leading principle, which contains no fact not implied or observable in the premises, is termed a *logical* principle, and the argument it governs is termed a *complete,* in contradistinction to an *incomplete,* argument, or *enthymeme.*

The above will be made clear by an example. Let us begin with the enthymeme,

Enoch was a man,
∴ Enoch died.

The leading principle of this is, "All men die." Stating it, we get the complete argument,

All men die,
Enoch was a man;
∴ Enoch was to die.

The leading principle of this is *nota notae est nota rei ipsius.* Stating this as a premise, we have the argument,

Nota notae est nota rei ipsius,
Mortality is a mark of humanity, which is a mark of Enoch;
∴ Mortality is a mark of Enoch.

But this very same principle of the *nota notae* is again active in the drawing of this last inference, so that the last state of the argument is no more complete than the last but one.

There is another way of rendering an argument complete, namely, instead of adding the leading principle $P_i \prec C_i$ conjunctively to the premise P, to form a new argument, we might add its denial disjunctively to the conclusion; thus,

P
∴ Either C or $P_i \overline{\prec} C_i$.

A logical principle is said to be an *empty* or merely formal proposition, because it can add nothing to the premises of the argument it governs, although it is relevant; so that it implies no fact except such as is presupposed in all discourse, as we have seen in §1 that certain facts are implied. We may here distinguish between *logical* and *extralogical* validity; the former being that of a *complete,* the latter that of an *incomplete* argument. The term *logical leading principle* we may take to mean the principle which must be supposed true in order to sustain the logical validity of any argument. Such a principle states that among all the states of things which can be supposed without conflict with logical principles, those in which the premise of the argument would be true would also be cases of the truth of the conclusion. Nothing more than this would be relevant to the *logical leading principle,* which is, therefore, perfectly determinate and not vague, as we have seen an extralogical leading principle to be.

A complete argument, with only one premise, is called an *immediate* inference. *Example:* All crows are black birds; therefore, all crows are birds. If from the premise of such an argument everything redundant is omitted, the state of things expressed in the premise is the same as the state of things expressed in the conclusion, and only the form of expression is changed. Now, the logician does not undertake to enumerate all the ways of expressing facts: he supposes the facts to be already expressed in certain standard or canonical forms. But the equivalence between different ones of his own standard forms is of the highest importance to him, and thus certain immediate inferences play the great part in formal logic. Some of these will not be reciprocal inferences or logical equations, but the most important of them will have that character.

If one fact has such a relation to a different one that, if the former be true, the latter is necessarily or probably true, this relation constitutes a determinate fact; and therefore, since the leading principle of a complete argument involves no matter of fact (beyond those employed in all discourse), it follows that every complete and *material* (in opposition to a merely *formal*) argument must have at least two premises.

From the doctrine of the leading principle it appears that if we have a valid and complete argument from more than one premise, we may suppress all premises but one and still have a valid but incomplete argument. This argument is justified by the suppressed premises; hence, from these premises alone we may infer that the conclusion would follow from the remaining premises. In this way, then, the original argument

$$\begin{array}{c} \text{P Q R S T} \\ \therefore\ \text{C} \end{array}$$

is broken up into two, namely, 1st,

$$\begin{array}{c} \text{P Q R S} \\ \therefore\ \text{T} \prec \text{C} \end{array}$$

and, 2nd,

$$\begin{array}{c} \text{T} \prec \text{C} \\ \text{T} \\ \therefore\ \text{C.} \end{array}$$

By repeating this process, any argument may be broken up into arguments of two premises each. A complete argument having two premises is called a *syllogism.*[3]

An argument may also be broken up in a different way by substituting for the second constituent above, the form

$$\begin{array}{c} \text{T} \prec \text{C} \\ \therefore\ \text{Either C or not T.} \end{array}$$

In this way, any argument may be resolved into arguments, each of which has one premise and two alternative conclusions. Such an argument, when complete, may be called a *dialogism.*

§3. *Forms of Propositions.*

In place of the two expressions $A \prec B$ and $B \prec A$ taken together we may write $A = B$;[4] in place of the two expressions $A \prec B$ and

3. The general doctrine of this section is contained in my paper, "On the Classification of Arguments," 1867.

4. There is a difference of opinion among logicians as to whether $\prec$ or $=$ is the simpler relation. But in my paper on the "Logic of Relatives," I have strictly demonstrated that the preference must be given to $\prec$ in this respect. The term *simpler* has an exact meaning in logic; it means that whose logical depth is smaller; that is, if one conception implies another, but not the reverse, then the latter is said to be the simpler. Now to say that $A = B$ implies that $A \prec B$, but not conversely. *Ergo,* etc. It is to no purpose to reply that $A \prec B$ implies $A = (A$ that is $B)$; it would be equally relevant to say that $A \prec B$ implies $A = A$. Consider an analogous case. Logical sequence is a simpler conception than causal sequence, because every causal sequence is a logical sequence but not every logical sequence is a causal sequence; and it is no reply to this to say that a logical sequence between two facts implies a causal sequence between some two facts whether the same or different. The idea that $=$ is a very simple relation is probably due to the fact that the discovery of such a relation teaches us that instead of two objects we have only one, so that it simplifies our conception of the universe. On this account the existence of such a relation is an important fact to learn; in fact, it has the sum of the importances of the two facts of which it is compounded. It frequently happens that it is more convenient to treat the propositions $A \prec B$ and $B \prec A$ together in their form $A = B$; but it also frequently happens that it is more convenient to treat them separately. Even in geometry we can see that

$B \mathrel{\overline{-\!\!<}} A$ taken together we may write $A < B$ or $B > A$; and in place of the two expressions $A \mathrel{\overline{-\!\!<}} B$ and $B \mathrel{\overline{-\!\!<}} A$ taken together we may write $A \mathrel{>\!\!\!<} B$.

De Morgan, in the remarkable memoir with which he opened his discussion of the syllogism (1846, p. 380), has pointed out that we often carry on reasoning under an implied restriction as to what we shall consider as possible, which restriction, applying to the whole of what is said, need not be expressed. The total of all that we consider possible is called the *universe* of discourse, and may be very limited. One mode of limiting our universe is by considering only what actually occurs, so that everything which does not occur is regarded as impossible.

The forms $A \mathrel{-\!\!<} B$, or A implies B, and $A \mathrel{\overline{-\!\!<}} B$, or A does not imply B, embrace both hypothetical and categorical propositions. Thus, to say that all men are mortal is the same as to say that if any man possesses any character whatever then a mortal possesses that character. To say, 'if A, then B' is obviously the same as to say that from A, B follows, logically or extralogically. By thus identifying the relation expressed by the copula with that of illation, we identify the proposition with the inference, and the term with the proposition. This identification, by means of which all that is found true of term, proposition, or inference is at once known to be true of all three, is a most important engine of reasoning, which we have gained by beginning with a consideration of the genesis of logic.[5]

Of the two forms $A \mathrel{-\!\!<} B$ and $A \mathrel{\overline{-\!\!<}} B$, no doubt the former is the more primitive, in the sense that it is involved in the idea of reasoning, while the latter is only required in the criticism of reasoning. The two kinds of proposition are essentially different, and every attempt

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to say that two figures A and B are equal is to say that when they are properly put together A will cover B and B will cover A; and it is generally necessary to examine these facts separately. So, in comparing the numbers of two lots of objects, we set them over against one another, each to each, and observe that for every one of the lot A there is one of the lot B, and for every one of the lot B there is one of the lot A.

In logic, our great object is to analyze all the operations of reason and reduce them to their ultimate elements; and to make a calculus of reasoning is a subsidiary object. Accordingly, it is more philosophical to use the copula $-\!\!<$, apart from all considerations of convenience. Besides, this copula is intimately related to our natural logical and metaphysical ideas; and it is one of the chief purposes of logic to show what validity those ideas have. Moreover, it will be seen further on that the more analytical copula does in point of fact give rise to the easiest method of solving problems of logic.

5. In consequence of the identification in question, in $S \mathrel{-\!\!<} P$, I speak of S indifferently as *subject, antecedent,* or *premise,* and of P as *predicate, consequent,* or *conclusion.*

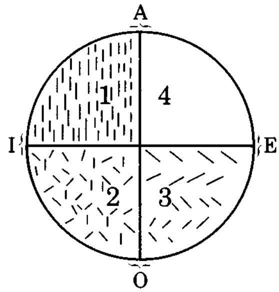
to reduce the latter to a special case of the former must fail. Boole attempts to express 'some men are not mortal', in the form 'whatever men have a certain unknown character v are not mortal'. But the propositions are not identical, for the latter does not imply that some men have that character v; and, accordingly, from Boole's proposition we may legitimately infer that 'whatever mortals have the unknown character v are not men'; yet we cannot reason from 'some men are not mortal' to 'some mortals are not men'.[6] On the other hand, we can rise to a more general form under which $\mathrm{A} \prec \mathrm{B}$ and $\mathrm{A} \mathrel{\overline{\prec}} \mathrm{B}$ are both included. For this purpose we write $\mathrm{A} \mathrel{\overline{\prec}} \mathrm{B}$ in the form $\breve{\mathrm{A}} \prec \overline{\mathrm{B}}$, where $\breve{\mathrm{A}}$ is *some*-A and $\overline{\mathrm{B}}$ is *not*-B. This more general form is equivocal in so far as it is left undetermined whether the proposition would be true if the subject were impossible. When the subject is general this is the case, but when the subject is particular (i.e., is subject to the modification *some*) it is not. The general form supposes merely inclusion of the subject under the predicate. The short curved mark over the letter in the subject shows that some part of the term denoted by that letter is the subject, and that that is asserted to be in possible existence.

The modification of the subject by the curved mark and of the predicate by the straight mark gives the old set of propositional forms, viz.:

A.	$a \prec b$	Every a is b.	Universal affirmative.
E.	$a \prec \bar{b}$	No a is b.	Universal negative.
I.	$\breve{a} \prec b$	Some a is b.	Particular affirmative.
O.	$\breve{a} \prec \bar{b}$	Some a is not b.	Particular negative.

There is, however, a difference between the senses in which these propositions are here taken and those which are traditional; namely, it is usually understood that affirmative propositions imply the existence of their subjects, while negative ones do not. Accordingly, it is said that there is an immediate inference from A to I and from E to O. But in the sense assumed in this paper, universal propositions do not, while particular propositions do, imply the existence of their subjects. The following figure illustrates the precise sense here assigned to the four forms A, E, I, O.

6. Equally unsuccessful is Mr. Jevons's attempt to overcome the difficulty by omitting particular propositions, "because we can always substitute for it [*some*] more definite expressions if we like." The same reason might be alleged for neglecting the consideration of *not*. But in fact the form $\mathrm{A} \mathrel{\overline{\prec}} \mathrm{B}$ is required to enable us to simply deny $\mathrm{A} \prec \mathrm{B}$.



In the quadrant marked 1 there are lines which are all vertical; in the quadrant marked 2 some lines are vertical and some not; in quadrant 3 there are lines none of which are vertical; and in quadrant 4 there are no lines. Now, taking *line* as subject and *vertical* as predicate,

A is true of quadrants 1 and 4 and false of 2 and 3.
E is true of quadrants 3 and 4 and false of 1 and 2.
I is true of quadrants 1 and 2 and false of 3 and 4.
O is true of quadrants 2 and 3 and false of 1 and 4.

Hence, A and O precisely deny each other, and so do E and I. But any other pair of propositions may be either both true or both false or one true while the other is false.

De Morgan ("On the Syllogism," No. I, 1846, p. 381) has enlarged the system of propositional forms by applying the sign of negation which first appears in $A \,\overline{\prec}\, B$ to the subject and predicate. He thus gets

$A \prec B$. Every A is B.	A is species of B.
$A \,\overline{\prec}\, B$. Some A is not B.	A is exient of B.
$A \prec \overline{B}$. No A is B.	A is external of B.
$A \,\overline{\prec}\, \overline{B}$. Some A is B.	A is partient of B.
$\overline{A} \prec B$. Everything is either A or B.	A is complement of B.
$\overline{A} \,\overline{\prec}\, B$. There is something besides A and B.	A is coinadequate of B.
$\overline{A} \prec \overline{B}$. A includes all B.	A is genus of B.
$\overline{A} \,\overline{\prec}\, \overline{B}$. A does not include all B.	A is deficient of B.

De Morgan's table of the relations of these propositions must be modified to conform to the meanings here attached to $\prec$ and to $\overline{\prec}$.

We might confine ourselves to the two propositional forms $S \prec P$ and $S \,\overline{\prec}\, P$. If we once go beyond this and adopt the form $S \prec \overline{P}$, we must, for the sake of completeness, adopt the whole of De Morgan's system. But this system, as we shall see in the next section, is itself incomplete, and requires to complete it the admission of particularity in the predicate. This has already been attempted by Hamilton, with an incompetence which ought to be extraordinary. I shall allude to this matter further on, but I shall not attempt to say how many forms of propositions there would be in the completed system.[7]

§4. *The Algebra of the Copula.*

From the identity of the relation expressed by the copula with that of illation, springs an algebra. In the first place, this gives us

$$x \prec x \tag{1}$$

the principle of identity, which is thus seen to express that what we have hitherto believed we continue to believe, in the absence of any reason to the contrary. In the next place, this identification shows that the two inferences

$$\begin{array}{ccc} x & & \\ y & \text{and} & x \\ \therefore\ z & & \therefore\ y \prec z \end{array} \tag{2}$$

are of the same validity. Hence we have

$$\{x \prec (y \prec z)\} = \{y \prec (x \prec z)\}.^{8} \tag{3}$$

From (1) we have

$$(x \prec y) \prec (x \prec y),$$

whence by (2)

$$\begin{array}{cc} x \prec y & x \\ \multicolumn{2}{c}{\therefore\ y} \end{array} \tag{4}$$

is a valid inference.

By (4), if x and $x \prec y$ are true y is true; and if y and $y \prec z$ are true z is true. Hence, the inference is valid

$$\begin{array}{ccc} x & x \prec y & y \prec z \\ & \therefore\ z. & \end{array}$$

7. In this connection see De Morgan, "On the Syllogism, No. V," 1863.

8. Mr. Hugh McColl ("Calculus of Equivalent Statements, [Second Paper]," 1878, p. 183) makes use of the sign of inclusion several times in the same proposition. He does not, however, give any of the formulæ of this section.

By the principle of (2) this is the same as to say that

$$\begin{array}{c} x \prec y \qquad y \prec z \\ \therefore\ x \prec z \end{array} \tag{5}$$

is a valid inference. This is the canonical form of the syllogism, *Barbara.* The statement of its validity has been called the *dictum de omni,* the *nota notae,* etc.; but it is best regarded, after De Morgan,[9] as a statement that the relation signified by the copula is a transitive one.[10] It may also be considered as implying that in place of the subject of a proposition of the form A $\prec$ B, any subject of that subject may be substituted, and that in place of its predicate any predicate of that predicate may be substituted.[11] The same principle may be algebraically conceived as a rule for the elimination of y from the two propositions $x \prec y$ and $y \prec z$.[12]

It is needless to remark that any letters may be substituted for x, y, z; and that, therefore, $\bar{x}, \bar{y}, \bar{z}$, some or all, may be substituted. Nevertheless, after these purely extrinsic changes have been made, the argument is no longer called *Barbara,* but is said to be some other universal mood of the *first figure.* There are evidently eight such moods.

From (5) we have, by (2), these two forms of valid immediate inference:

$$\begin{array}{c} \mathrm{S} \prec \mathrm{P} \\ \therefore\ (x \prec \mathrm{S}) \prec (x \prec \mathrm{P}) \end{array} \tag{6}$$

and

$$\begin{array}{c} \mathrm{S} \prec \mathrm{P} \\ \therefore\ (\mathrm{P} \prec x) \prec (\mathrm{S} \prec x). \end{array} \tag{7}$$

The latter may be termed the inference of *contraposition.*

9. "On the Syllogism, No. II," 1850, p. 104.

10. That the validity of syllogism is not deducible from the principles of identity, contradiction, and excluded middle, is capable of strict demonstration. The transitiveness of the copula is, however, implied in the identification of the copula-relation with illation, because illation is obviously transitive.

11. The conception of substitution (already involved in the mediæval doctrine of descent), as well as the word, was familiar to logicians before the publication of Mr. Jevons's *Substitution of Similars.* This book argues, however, not only that inference is substitution, but that it and induction in particular consist in the substitution of similars. This doctrine is allied to Mill's theory of induction.

12. This must have been in Boole's mind from the first. De Morgan ("On the Syllogism, No. II," 1850, p. 83) goes too far in saying that "what is called elimination in algebra is called inference in logic," if he means, as he seems to do, that all inference is elimination.

From the transitiveness of the copula, the following inference is valid:

$$\begin{aligned} &(S \prec M) \prec (S \prec P), \\ &(S \prec P) \prec x; \\ \therefore\ &(S \prec M) \prec x. \end{aligned}$$

But, by (6), from $(M \prec P)$ we can infer the first premise immediately; hence the inference is valid

$$\begin{aligned} &M \prec P, \\ &(S \prec P) \prec x; \\ \therefore\ &(S \prec M) \prec x. \end{aligned} \tag{8}$$

This may be called the *minor indirect syllogism.* The following is an example:

All men are mortal,
If Enoch and Elijah were mortal, the Bible errs;
∴ If Enoch and Elijah were men, the Bible errs.

Again we may start with this syllogism in *Barbara*

$$\begin{aligned} &(M \prec P) \prec (S \prec P), \\ &(S \prec P) \prec x; \\ \therefore\ &(M \prec P) \prec x. \end{aligned}$$

But by the principle of contraposition (7), the first premise immediately follows from $(S \prec M)$, so that we have the inference valid

$$\begin{aligned} &S \prec M, \\ &(S \prec P) \prec x; \\ \therefore\ &(M \prec P) \prec x. \end{aligned} \tag{9}$$

This may be called the *major indirect syllogism.*

Example: All patriarchs are men,
If all patriarchs are mortal, the Bible errs;
∴ If all men are mortal, the Bible errs.

In the same way it might be shown that (6) justifies the syllogism

$$\begin{aligned} &M \prec P, \\ &x \prec (S \prec M); \\ \therefore\ &x \prec (S \prec P). \end{aligned} \tag{10}$$

And (7) justifies the inference

$$\begin{aligned} &S \prec M, \\ &x \prec (M \prec P); \\ \therefore\ &x \prec (S \prec P). \end{aligned} \tag{11}$$

But these are only slight modifications of *Barbara.*

In the form (10), x may denote a limited universe comprehending some cases of S. Then we have the syllogism

$$\begin{array}{rl} & M \prec P, \\ & S \overline{\prec} \overline{M}; \\ \therefore & S \overline{\prec} \overline{P}. \end{array} \tag{12}$$

This is called *Darii.* A line might, of course, be drawn over the S. So, in the form (11), x may denote a limited universe comprehending some P. Then we have the syllogism

$$\begin{array}{rl} & S \prec M, \\ & \overline{M} \overline{\prec} P; \\ \therefore & \overline{S} \overline{\prec} P. \end{array} \tag{13}$$

Here a line might be drawn over the P. But the forms (12) and (13) are deduced from (10) and (11) only by principles of interpretation which require demonstration.

On the other hand, if in the *minor indirect syllogism* (8), we put "what does not occur" for x, we have by definition

$$\{(S \prec P) \prec x\} = (S \overline{\prec} P)$$

and we then have

$$\begin{array}{rl} & M \prec P, \\ & S \overline{\prec} P; \\ \therefore & S \overline{\prec} M, \end{array} \tag{14}$$

which is the syllogism *Baroko.* If a line is drawn over P, the syllogism is called *Festino;* and by other negations eight essentially identical forms are obtained, which are called minor-particular moods of the second figure.[13] In the same way the major indirect syllogism (9) affords the form

$$\begin{array}{rl} & S \prec M, \\ & S \overline{\prec} P; \\ \therefore & M \overline{\prec} P. \end{array} \tag{15}$$

This form is called *Bokardo.* If P is negatived, it is called *Disamis.* Other negations give the eight major-particular moods of the third figure.

We have seen that $S \overline{\prec} P$ is of the form $(S \prec P) \prec x$. Put A for $S \prec P$, and we find that $\overline{A}$ is of the form $A \prec x$. Then the principle of contraposition (7) gives the immediate inference

13. De Morgan, *Syllabus,* 1860, p. 18.

$$\begin{array}{rl} & S \prec P \\ \therefore & \bar{P} \prec \bar{S}. \end{array} \tag{16}$$

Applying this to the universal moods of the first figure justifies six moods. These are two in the second figure,

$$\begin{array}{lll} x \prec \bar{y} & z \prec y & \therefore\ x \prec \bar{z}\ (\textit{Camestres}) \\ \bar{x} \prec \bar{y} & z \prec y & \therefore\ \bar{x} \prec \bar{z}; \end{array}$$

two in the third figure,

$$\begin{array}{lll} y \prec x & \bar{y} \prec z & \therefore\ \bar{x} \prec z \\ y \prec x & \bar{y} \prec \bar{z} & \therefore\ \bar{x} \prec \bar{z}; \end{array}$$

and two others which are said to be in the fourth figure,

$$\begin{array}{lll} x \prec y & y \prec z & \therefore\ \bar{z} \prec \bar{x} \\ x \prec \bar{y} & \bar{y} \prec z & \therefore\ \bar{z} \prec \bar{x}. \end{array}$$

But the negative has two other properties not yet taken into account. These are

$$x \prec \bar{\bar{x}} \tag{17}$$

or x is not not-X, which is called the *principle of contradiction;* and

$$\bar{\bar{x}} \prec x \tag{18}$$

or what is not not-X is x, which is called the *principle of excluded middle.*

By (17) and (16) we have the immediate inference

$$\begin{array}{rl} & S \prec \bar{P} \\ \therefore & P \prec \bar{S} \end{array} \tag{19}$$

which is called the conversion of E. By (18) and (16) we have

$$\begin{array}{rl} & \bar{S} \prec P \\ \therefore & \bar{P} \prec S. \end{array} \tag{20}$$

By (17), (18), and (16), we have

$$\begin{array}{rl} & \bar{S} \prec \bar{P} \\ \therefore & P \prec S. \end{array} \tag{21}$$

Each of the inferences (19), (20), (21), justifies six universal syllogisms; namely, two in each of the figures, second, third, and fourth. The result is that each of these figures has eight universal moods; two depending only on the principle that $\bar{A}$ is of the form $A \prec x$, two depending also on the principle of contradiction, two on the principle of excluded middle, and two on all three principles conjoined.[14]

The same formulæ (16), (19), (20), (21), applied to the minor-particular moods of the second figure, will give eight minor-particular moods of the first figure; and applied to the major-particular moods of the third figure, will give eight major-particular moods of the first figure.[15]

The principle of contradiction in the form (19) may be further transformed thus:—

$$\text{If } (\mathrm{P} \therefore \overline{\mathrm{C}}) \text{ is valid, then } (\mathrm{C} \therefore \overline{\mathrm{P}}) \text{ is valid.} \qquad (22)$$

Applying this to the minor-particular moods of the first figure, will give eight minor-particular moods of the third figure; and applying it to the major-particular moods of the first figure will give eight major-particular moods of the second figure.

It is very noticeable that the corresponding formula,

$$\text{If } (\overline{\mathrm{P}} \therefore \mathrm{C}) \text{ is valid, then } (\overline{\mathrm{C}} \therefore \mathrm{P}) \text{ is valid,} \qquad (23)$$

has no application in the existing syllogistic, because there are no syllogisms having a particular premise and universal conclusion. In the same way, in the Aristotelian system an affirmative conclusion cannot be drawn from negative premises, the reason being that negation is only applied to the predicate. So in De Morgan's system the subject only is made particular, not the predicate.

In order to develop a system of propositions in which the predicate shall be modified in the same way in which the subject is modified in particular propositions, we should consider that to say $\mathrm{S} \prec \mathrm{P}$ is the same as to say $(\mathrm{S} \overline{\prec}\, x) \prec (\mathrm{P} \overline{\prec}\, x)$, whatever x may be. That

$$(\mathrm{S} \prec \mathrm{P}) \prec \{(\mathrm{S} \overline{\prec}\, x) \prec (\mathrm{P} \overline{\prec}\, x)\}$$

follows at once from *Bokardo* (15) by means of (2). Moreover, since $\overline{\mathrm{A}}$ may be put in the form $\mathrm{A} \prec x$, it follows that $\overline{\overline{\mathrm{A}}}$ may be put in the form $\mathrm{A} \overline{\prec}\, x$, so that by the principles of contradiction and excluded

14. An oversight has here been committed. For from $\overline{\mathrm{A}} = (\mathrm{A} \prec x)$ follows not merely (16) but also (19), (20), and (21), and thus all the properties of the negative which concern syllogistic. But this does not affect the view taken of the subject, nor the division of the moods according to the properties of the negative on which they depend; for whatever is shown in the text to be deducible from $\overline{\mathrm{A}} = (\mathrm{A} \prec x)$ is in fact deducible from (16).

15. Aristotle and De Morgan have particular conclusions from two universal premises. These are all rendered illogical by the significations which I attach to $\prec$ and $\overline{\prec}$.

middle, A may be put in the form $A \overline{\prec} x$. On the other hand, to say $S \overline{\prec} \bar{P}$ is the same as to say $(S \prec \bar{x}) \prec (P \overline{\prec} x)$, whatever x may be; for

$$(S \overline{\prec} \bar{P}) \prec \{(S \prec \bar{x}) \prec (P \overline{\prec} x)\}$$

is the principle of *Ferison,* a valid syllogism of the third figure; and if for x we put $\bar{S}$, we have

$$(S \prec \bar{\bar{S}}) \prec (P \overline{\prec} \bar{S}),$$

which is the same as to say that $P \overline{\prec} \bar{S}$ is true if the principle of contradiction is true. So that it follows that $P \overline{\prec} \bar{S}$ if $S \overline{\prec} \bar{P}$ from the principle of contradiction. Comparing

$$S \prec P \text{ or } (S \overline{\prec} x) \prec (P \overline{\prec} x)$$

with

$$S \overline{\prec} \bar{P} \text{ or } (S \prec \bar{x}) \prec (P \overline{\prec} x),$$

we see that they differ by a modification of the subject. Denoting this by a short curve over the subject, we may write $\check{S} \prec P$ for $S \overline{\prec} \bar{P}$. We see then that while for A we may write $A \overline{\prec} x$, where x is anything whatever, so for $\check{A}$ we may write $A \prec \bar{x}$. If we attach a similar modification to the predicate also, we have

$$\check{S} \prec \check{P} \text{ or } (S \prec \bar{x}) \prec (P \prec \bar{x}),$$

which is the same as to say that you can find an S which is any P you please. We thus have

$$(S \prec P) \prec (\check{P} \prec \check{S}), \tag{24}$$

a formula of contraposition, similar to (16).

It is obvious that

$$(\check{S} \prec P) \prec (\check{P} \prec S); \tag{25}$$

for, negating both propositions, this becomes, by (16),

$$(P \prec \bar{S}) \prec (S \prec \bar{P}),$$

which is (19). The inference justified by (25) is called the conversion of I. From (25) we infer

$$\check{\check{x}} \prec x, \tag{26}$$

which may be called the principle of particularity. This is obviously true, because the modification of particularity only consists in changing $(A \overline{\prec} x)$ to $(A \prec \bar{x})$, which is the same as negating the copula and predicate, and a repetition of this will evidently give the first expression again. For the same reason we have

$$x \prec \breve{\breve{x}}, \tag{27}$$

which may be called the principle of individuality. This gives

$$(\mathrm{S} \prec \breve{\mathrm{P}}) \prec (\mathrm{P} \prec \breve{\mathrm{S}}), \tag{28}$$

and (26) and (27) together give

$$(\breve{\mathrm{S}} \prec \breve{\mathrm{P}}) \prec (\mathrm{P} \prec \mathrm{S}). \tag{29}$$

It is doubtful whether the proposition $\mathrm{S} \prec \breve{\mathrm{P}}$ ought to be interpreted as signifying that S and P are one sole individual, or that there is something besides S and P. I here leave this branch of the subject in an unfinished state.

Corresponding to the formulæ which we have obtained by the principle (2) are an equal number obtained by the following principle:

The inference

(2′)
$$\begin{array}{c} x \\ \therefore \text{ Either } y \text{ or } z \end{array}$$

has the same validity as

$$\begin{array}{c} x \,\overline{\prec}\, y \\ \therefore\ z. \end{array}$$

(3′) $$\{(x \,\overline{\prec}\, y) \,\overline{\prec}\, z\} = \{(x \,\overline{\prec}\, z) \,\overline{\prec}\, y\}.$$

From (1) we have

$$(x \,\overline{\prec}\, y) \prec (x \,\overline{\prec}\, y),$$

whence, by (2),

(4′)
$$\begin{array}{c} x \\ \therefore \text{ Either } (x \,\overline{\prec}\, y) \text{ or } y. \end{array}$$

This gives

$$\begin{array}{c} x \\ \therefore \text{ Either } x \,\overline{\prec}\, y \text{ or } y \,\overline{\prec}\, z \text{ or } z. \end{array}$$

Then, by (2),

(5′)
$$\begin{array}{c} x \,\overline{\prec}\, z \\ \therefore\ x \,\overline{\prec}\, y \text{ or } y \,\overline{\prec}\, z, \end{array}$$

which is the canonical form of dialogism. The minor indirect dialogism is

(8′) $$x \overline{\prec} (\mathrm{M} \overline{\prec} \mathrm{P})$$
$$\therefore \text{ Either } x \overline{\prec} (\mathrm{S} \overline{\prec} \mathrm{P}) \text{ or } \mathrm{S} \overline{\prec} \mathrm{M}.$$

The major indirect dialogism is

$$x \overline{\prec} (\mathrm{S} \overline{\prec} \mathrm{M})$$
$$\therefore \text{ Either } x \overline{\prec} (\mathrm{S} \overline{\prec} \mathrm{P}) \text{ or } \mathrm{M} \overline{\prec} \mathrm{P}.$$

We have also

(12′) $$(\mathrm{S} \overline{\prec} \mathrm{P}) \overline{\prec} x$$
$$\therefore \text{ Either } (\mathrm{S} \overline{\prec} \mathrm{M}) \text{ or } (\mathrm{M} \overline{\prec} \mathrm{P}) \overline{\prec} x$$

and

(13′) $$(\mathrm{S} \overline{\prec} \mathrm{P}) \overline{\prec} x$$
$$\therefore \text{ Either } (\mathrm{M} \overline{\prec} \mathrm{P}) \text{ or } (\mathrm{S} \overline{\prec} \mathrm{M}) \overline{\prec} x.$$

We have A of the form $x \overline{\prec} \overline{\mathrm{A}}$. And we have the inferences

$$\begin{array}{llll} \mathrm{S} \overline{\prec} \mathrm{P} & \mathrm{S} \overline{\prec} \overline{\mathrm{P}} & \overline{\mathrm{S}} \overline{\prec} \mathrm{P} & \overline{\mathrm{S}} \overline{\prec} \overline{\mathrm{P}} \\ \therefore\ \overline{\mathrm{P}} \overline{\prec} \overline{\mathrm{S}}. & \therefore\ \mathrm{P} \overline{\prec} \overline{\mathrm{S}}. & \therefore\ \overline{\mathrm{P}} \overline{\prec} \mathrm{S}. & \therefore\ \mathrm{P} \overline{\prec} \mathrm{S}. \end{array}$$

CHAPTER II.—THE LOGIC OF NON-RELATIVE TERMS.

§1. *The Internal Multiplication and the Addition of Logic.*

We have seen that the inference

$$x \text{ and } y$$
$$\therefore\ z$$

is of the same validity with the inference

$$x$$
$$\therefore \text{ Either } \overline{y} \text{ or } z,$$

and the inference

$$x$$
$$\therefore \text{ Either } y \text{ or } z$$

with the inference

$$x \text{ and } \overline{y}$$
$$\therefore\ z.$$

In like manner,

$$x \prec y$$

is equivalent to

$$\text{(The possible)} \prec \text{Either } \bar{x} \text{ or } y,$$

and to

$$x \text{ which is } \bar{y} \prec \text{(The impossible)}.$$

To express this algebraically, we need, in the first place, symbols for the two terms of second intention, the possible and the impossible. Let ∞ and 0 be the terms; then we have the definitions

$$x \prec \infty \qquad\qquad 0 \prec x \tag{1}$$

whatever x may be.[16]

We need also two operations which may be called non-relative addition and multiplication. They are defined as follows:[17]

16. The symbol 0 is used by Boole; the symbol ∞ replaces his 1, according to a suggestion in my "Logic of Relatives," 1870.

17. These forms of definition are original. The algebra of non-relative terms was given by Boole (*Mathematical Analysis of Logic,* 1847). Boole's addition was not the same as that in the text, for with him whatever was common to the two terms added was taken twice over in the sum. The operations in the text were given as complements of one another, and with appropriate symbols, by De Morgan ("On the Syllogism, No. III," 1858, p. 185). For addition, sum, parts, he uses aggregation, aggregate, aggregants; for multiplication, product, factors, he uses composition, compound, components. Mr. Jevons (*Pure Logic,* 1864)—I regret that I can only speak of this work from having read it many years ago, and therefore cannot be sure of doing it full justice—improved the algebra of Boole by substituting De Morgan's aggregation for Boole's addition. The present writer, not having seen either De Morgan's or Jevons's writings on the subject, again recommended the same change ("On an Improvement in Boole's Calculus of Logic," 1867), and showed the perfect balance existing between the two operations. In another paper, published in 1870, I introduced the sign of inclusion into the algebra.

In 1872, Robert Grassmann, brother of the author of the "Ausdehnungslehre," published a work entitled *Die Formenlehre oder Mathematik,* the second book of which gives an algebra of logic identical with that of Jevons. The very notation is reproduced, except that the universe is denoted by T instead of U, and a term is negatived by drawing a line over it, as by Boole, instead of by taking a type from the other case, as Jevons does. Grassmann also uses a sign equivalent to my $\prec$. In his third book, he has other matter which he might have derived from my paper of 1870. Grassmann's treatment of the subject presents inequalities of strength; and most of his results had been anticipated. Professor Schröder, of Karlsruhe, in the spring of 1877, produced his *Operationskreis des Logikkalkuls.* He had seen the works of Boole and Grassmann, but not those of De Morgan, Jevons, and me. He gives a fine development of the algebra, adopting the addition of Jevons, and he exhibits the balance between $+$ and $\times$ by printing the theorems in parallel columns, thus imitating a practice of the geometricians. Schröder gives an original, interesting, and commodious method of working with the algebra. Later in the same year, Mr. Hugh McColl, apparently having known nothing of logical algebra except from a jejune account of Boole's work in Bain's *Logic,* published several papers on a "Calculus of Equivalent Statements," the basis of which is nothing but the Boolian algebra, with Jevons's addition and a sign of inclusion. Mr. McColl adds an exceedingly ingenious application of this algebra to the transformation of definite integrals.

If $a \prec x$ and $b \prec x$, then $a + b \prec x$;	If $x \prec a$ and $x \prec b$, then $x \prec a \times b$; (2)
and conversely,	and conversely,
if $a + b \prec x$, then $a \prec x$ and $b \prec x$.	if $x \prec a \times b$, then $x \prec a$ and $x \prec b$. (3)

From these definitions we at once deduce the following formulæ:—[18]

A. $a \prec a + b$ $\qquad a \times b \prec a$ (Peirce, 1870)[19]
$b \prec a + b$ $\qquad a \times b \prec b$. (4)

These are proved by substituting $a + b$ and $a \times b$ for x in (3).

B. $x = x + x$ $\qquad x \times x = x$ (Jevons, 1864). (5)

By substituting x for a and b in (2), we get

$$x + x \prec x \qquad x \prec x \times x;$$

and, by (4),

$$x \prec x + x \qquad x \times x \prec x.$$

C. $a + b = b + a$ $\qquad a \times b = b \times a$ (Boole, Jevons). (6)

These formulæ are examples of the *commutative principle.* From (4) and (2),

$$b + a \prec a + b \qquad a \times b \prec b \times a$$

and interchanging a and b we get the reciprocal inclusion implied in (6).

D. $(a + b) + c = a + (b + c)$ $\quad a \times (b \times c) = (a \times b) \times c$ (Boole, Jevons). (7)

These are cases of the *associative principle.* By (4), $c \prec b + c$ and $b \times c \prec c$; also $b + c \prec a + (b + c)$ and $a \times (b \times c) \prec b \times c$; so that $c \prec a + (b + c)$ and $a \times (b \times c) \prec c$. In the same way, $b \prec a + (b + c)$ and $a \times (b \times c) \prec b$, and, by (4), $a \prec a + (b + c)$ and $a \times (b \times c) \prec a$. Hence, by (2), $a + b \prec a + (b + c)$ and $a \times (b \times c) \prec a \times b$. And, again by (2), $(a + b) + c \prec a + (b + c)$ and $a \times (b \times c) \prec (a \times b) \times c$. In a similar way we should prove the converse propositions to these and so establish (7).

18. The proofs of the lettered propositions follow the enunciations.
19. "Logic of Relatives" (§4) gives $a \times b \prec a$. The other formulæ, equally obvious, I do not find anywhere.

E. $(a+b)\times c=(a\times c)+(b\times c) \quad (a\times b)+c=(a+c)\times(b+c)$.[20] (8)

These are cases of the *distributive principle.* They are easily proved by (4) and (2), but the proof is too tedious to give.

F. $(a+b)+c=(a+c)+(b+c) \quad (a\times b)\times c=(a\times c)\times(b\times c)$. (9)

These are other cases of the distributive principle. They are proved by (5), (6), and (7). These formulæ, which have hitherto escaped notice, are not without interest.

G. $a+(a\times b)=a \quad a\times(a+b)=a$ (Grassmann, Schröder). (10)

By (4), $\quad a\prec a+(a\times b) \qquad a\times(a+b)\prec a.$

Again, by (4), $(a\times b)\prec a$ and $a\prec a+b$; hence, by (2).

$$a+(a\times b)\prec a \qquad a\prec a\times(a+b).$$

H. $$(a+b\prec a)=(b\prec a\times b). \tag{11}$$

This proposition is a transformation of Schröder's two propositions 21, (p. 25), one of which was given by Grassmann. By (3)

$$(a+b\prec a)\prec(b\prec a) \qquad (b\prec a\times b)\prec(b\prec a).$$

Hence, since $\quad b\prec b, \qquad a\prec a$

we have, by (2),

$$(a+b\prec a)\prec(b\prec a\times b) \qquad (b\prec a\times b)\prec(a+b\prec a).$$

I. $\left.\begin{array}{l}(a\prec b)\times(x\prec y)\prec(a+x\prec b+y)\\(a\prec b)\times(x\prec y)\prec(a\times x\prec b\times y)\end{array}\right\}$ (Peirce, 1870). (12)

Readily proved from (2) and (4).

J. $$(a\prec b+x)\times(a\times x\prec b)=(a\prec b). \tag{13}$$

This is a generalization of a theorem by Grassmann. In stating it, he erroneously unites the first two propositions by + instead of $\times$. By (12), (5), and (8),

$$(a\prec b+x)\prec\{a\prec(a\times b)+(a\times x)\}$$
$$(a\times x\prec b)\prec\{(a+b)\times(x+b)\prec b\}.$$

But by (4)

$$a\prec a+b \qquad a\times b\prec b.$$

Hence, by (2), it is doubly proved that

$$(a\prec b+x)\times(a\times x\prec b)\prec(a\prec b).$$

The demonstration of the converse is obvious.

20. The first of these given by Boole for his addition, was retained by Jevons in changing the addition. The second was first given by me (1867).

We have immediately, from (2) and (3),

$$\text{K.}\quad (a+b \prec c) = (a \prec c)\times(b \prec c) \quad (c \prec a\times b) = (c \prec a)\times(c \prec b). \quad (14)$$

$$\text{L.}\quad \begin{array}{ll} (c \prec a+b) = \Sigma\{(p \prec a)\times(q \prec b)\} & \text{where } p+q=c \\ (a\times b \prec c) = \Sigma\{(a \prec p)\times(b \prec q)\} & \text{where } c = p\times q. \end{array} \quad (15)$$

The propositions (15) are new. By (12)

$$\begin{array}{ll} \{(p \prec a)\times(q \prec b)\} \prec (c \prec a+b) & \text{where } p+q=c \\ \{(a \prec p)\times(b \prec q)\} \prec (a\times b \prec c) & \text{where } c=p\times q. \end{array}$$

And, since these are true for any set of values of p and q, we have by (2)

$$\begin{array}{l} \Sigma\{(p \prec a)\times(q \prec b)\} \prec (c \prec a+b) \text{ where } p+q=c \\ \Sigma\{(a \prec p)\times(b \prec q)\} \prec (a\times b \prec c) \text{ where } c=p\times q. \end{array}$$

By (4) and (8), we have

$$\begin{array}{l} (c \prec a+b) \prec \{(a\times c)+(b\times c)=c\} \\ (a\times b \prec c) \prec \{(c+a)\times(c+b)=c\}. \end{array}$$

Hence, putting

$$\begin{array}{lll} a\times c=p & b\times c=q & \text{where } p+q=c \\ a+c=p & b+c=q & \text{where } p\times q=c, \end{array}$$

we have

$$\begin{array}{l} (c \prec a+b) \prec (p \prec a)\times(q \prec b) \text{ where } p+q=c \\ (a\times b \prec c) \prec (a \prec p)\times(b \prec q) \text{ where } c=p\times q, \end{array}$$

whence, by (4)

$$\begin{array}{l} (c \prec a+b) \prec \Sigma\{(p \prec a)\times(q \prec b)\} \text{ where } p+q=c \\ (a\times b \prec c) \prec \Sigma\{(a \prec p)\times(b \prec q)\} \text{ where } c=p\times q. \end{array}$$

A formula analogous to (15) will be found below, (35).

From (1) and (2) and (4) we have

$$x+0=x \qquad x=x\times\infty. \quad (16)$$

From (1) and (4),

$$x+\infty=\infty \qquad 0=x\times 0. \quad (17)$$

The definition of the negative has as we have seen three clauses: first, that $\bar{a}$ is of the form $a \prec x$; second, $a \prec \bar{\bar{a}}$; third, $\bar{\bar{a}} \prec a$.

From the first we have that if

$$\begin{array}{ll} c & a \\ \therefore & b \end{array}$$

is valid, then

$$\begin{array}{l} c \quad \bar{b} \\ \therefore \quad \bar{a} \end{array}$$

is valid. Or

$$(c \times a \prec b) \prec (c \times \bar{b} \prec \bar{a}). \tag{18}$$

Also, that if

$$\begin{array}{c} b \\ \therefore \ \text{Either } c \text{ or } a \end{array}$$

is valid, then

$$\begin{array}{c} \bar{a} \\ \therefore \ \text{Either } c \text{ or } \bar{b} \end{array}$$

is valid; or

$$(b \prec c + a) \prec (\bar{a} \prec c + \bar{b}). \tag{19}$$

Combining (18) and (19), we have

$$(a \times b \prec c + d) \prec (a \times \bar{d} \prec c + \bar{b}). \tag{20}$$

By the principles of contradiction and excluded middle, this gives

$$(a \times \bar{d} \prec c + \bar{b}) \prec (a \times b \prec c + d). \tag{21}$$

Thus the formula

$$(a \times b \prec c + d) = (a \times \bar{d} \prec c + \bar{b}) \tag{22}$$

embodies the essence of the negative.

If in (22) we put, first, $a = d \quad b = c = 0$, and then $a = d = \infty \quad b = c$, we have from the formula of identity

$$a \times \bar{a} = 0 \qquad a + \bar{a} = \infty. \tag{23}$$

We have

$$p = (p \times x) + (p \times \bar{x}) \qquad p = (p + x) \times (p + \bar{x}) \tag{24}$$

by the distributive principle and (23). If we write

$$i = p + (a \times \bar{x}) \quad j = p + (b \times x) \quad k = p \times (c + x) \quad l = p \times (d + \bar{x}),$$

we equally have

$$p = (i \times x) + (j \times \bar{x}) \qquad p = (l + x) \times (k + \bar{x}). \tag{25}$$

Now p may be a function of x, and such values may perhaps be assigned to $a, b, c, d,$ that $i, j, k, l,$ shall be free from x. It is obvious that if the function results from any complication of the operations $+$ and $\times$, this is the case. Supposing, then, i, j, k, l, to be constant, we have, putting successively, ∞, and 0, for x,

$$\begin{aligned} \varphi\infty &= i = k \\ \varphi 0 &= j = l \end{aligned}$$

so that

$$\varphi x = (\varphi\infty \times x) + (\varphi 0 \times \bar{x}) \quad \varphi x = (\varphi 0 + x) \times (\varphi\infty + \bar{x}). \tag{26}$$

The first of these formulæ was given by Boole for his addition. I showed (1867) that both hold for the modified addition. These formulæ are real analogues of mathematical developments; but practically they are not convenient. Their connection suggests the general formula

$$(a + x) \times (b + \bar{x}) = (a \times \bar{x}) + (b \times x) \tag{27}$$

a formula of frequent utility.

The distributive principle and (3) applied to (26) give

$$\varphi 0 \times \varphi\infty \prec \varphi x \quad \varphi x \prec \varphi\infty + \varphi 0. \tag{28}$$

Hence

$$(\varphi x = 0) \prec (\varphi 0 \times \varphi\infty = 0) \quad (\varphi x = \infty) \prec (\varphi 0 + \varphi\infty = \infty). \tag{29}$$

Boole gave the former, and I (1867) the latter. These formulæ are not convenient for elimination.

The following formulæ (probably given by De Morgan) are of great importance:—

$$\overline{a \times b} = \bar{a} + \bar{b} \quad \overline{a + b} = \bar{a} \times \bar{b}. \tag{30}$$

By (23)

$$(a \times b) \times (\overline{a \times b}) \prec 0 \quad \infty \prec (a + b) + (\overline{a + b}),$$

whence by (22) and the associative principle

$$\begin{aligned} b \times (\overline{a \times b}) &\prec \bar{a} & \bar{a} &\prec b + (\overline{a + b}) \\ \overline{a \times b} &\prec \bar{a} + \bar{b} & \bar{a} \times \bar{b} &\prec \overline{a + b}. \end{aligned}$$

By (4) and (22)

$$\begin{aligned} \bar{a} &\prec \overline{a \times b} & \overline{a + b} &\prec \bar{a} \\ \bar{b} &\prec \overline{a \times b} & \overline{a + b} &\prec \bar{b}, \end{aligned}$$

whence by (2)

$$\bar{a} + \bar{b} \prec \overline{a \times b} \quad \overline{a + b} \prec \bar{a} \times \bar{b}.$$

The application of (22) gives from (11)

$$(b \,\overline{\prec}\, a \times b) = (a + b \,\overline{\prec}\, a); \tag{31}$$

from (12)

$$\begin{aligned} &(a + x \,\overline{\prec}\, b + y) \prec (a \,\overline{\prec}\, b) + (x \,\overline{\prec}\, y) \\ &(a \times x \,\overline{\prec}\, b \times y) \prec (a \,\overline{\prec}\, b) + (x \,\overline{\prec}\, y); \end{aligned} \tag{32}$$

from (13)

$$(a \,\overline{\prec}\, b) = (a \,\overline{\prec}\, b + x) + (a \times x \,\overline{\prec}\, b); \tag{33}$$

from (14)

$$(a + b \,\overline{\prec}\, c) = (a \,\overline{\prec}\, c) + (b \,\overline{\prec}\, c) \quad (c \,\overline{\prec}\, a \times b) = (c \,\overline{\prec}\, a) + (c \,\overline{\prec}\, b); \tag{34}$$

from (15)

$$\begin{aligned} &(c \,\overline{\prec}\, a + b) = \Pi\{(p \,\overline{\prec}\, a) + (q \,\overline{\prec}\, b)\} \text{ where } p + q = c \\ &(a \times b \,\overline{\prec}\, c) = \Pi\{(a \,\overline{\prec}\, p) + (b \,\overline{\prec}\, q)\} \text{ where } p \times q = c; \end{aligned} \tag{35}$$

from (22)

$$(a \times b \,\overline{\prec}\, c + d) = (a \times \bar{d} \,\overline{\prec}\, c + \bar{b}). \tag{36}$$

§2. *The Resolution of Problems in Non-relative Logic.*

Four different algebraic methods of solving problems in the logic of non-relative terms have already been proposed by Boole, Jevons, Schröder, and McColl. I propose here a fifth method which perhaps is simpler and certainly is more natural than any of the others. It involves the following processes:

First Process. Express all the premises with the copulas $\prec$ and $\overline{\prec}$, remembering that $A = B$ is the same as $A \prec B$ and $B \prec A$.

Second Process. Separate every predicate into as many factors and every subject into as many aggregant terms as is possible without increasing the number of different letters used in any subject or predicate.

An expression might be separated into such factors or aggregants (let us term them *prime* factors and *ultimate* aggregants) by one or other of these formulæ:

$$\begin{aligned} \varphi x &= (\varphi\infty \times x) + (\varphi 0 \times \bar{x}) \\ \varphi x &= (\varphi\infty + \bar{x}) \times (\varphi 0 + x). \end{aligned}$$

But the easiest method is this. To separate an expression into its $\left\{\begin{array}{c}\text{ultimate aggregants}\\ \text{prime factors}\end{array}\right\}$ take any $\left\{\begin{array}{c}\text{product}\\ \text{sum}\end{array}\right\}$ of all the different letters of the expression, each taken either positively or negatively (that is, with a dash over it). By means of the fundamental formulæ

$$\mathrm{X} \times \mathrm{Y} \prec \mathrm{Y} \prec \mathrm{Y} + \mathrm{Z},$$

examine whether the $\left\{\begin{array}{c}\text{product}\\ \text{sum}\end{array}\right\}$ taken is a $\left\{\begin{array}{c}\text{subject}\\ \text{predicate}\end{array}\right\}$ of every $\left\{\begin{array}{c}\text{factor}\\ \text{aggregant}\end{array}\right\}$ of the given expression. If so, it is a $\left\{\begin{array}{c}\text{ultimate aggregant}\\ \text{prime factor}\end{array}\right\}$ of that expression; otherwise not. Proceed in this way until as many $\left\{\begin{array}{c}\text{ultimate aggregants}\\ \text{prime factors}\end{array}\right\}$ have been found as the expression possesses. This number is found in the case of a $\left\{\begin{array}{c}\text{product of sums}\\ \text{sum of products}\end{array}\right\}$ of letters, as follows. Let m be the number of *different* letters in the expression (a letter and its negative not being considered different); let n be the total number of letters whether the same or different, and let p be the number of $\left\{\begin{array}{c}\text{factors}\\ \text{terms}\end{array}\right\}$. Then the number of $\left\{\begin{array}{c}\text{ultimate aggregants}\\ \text{prime factors}\end{array}\right\}$ is

$$2^m + n - mp - p.$$

For example, let it be required to separate $x + (y \times z)$ into its prime factors. Here $m = 3$, $n = 3$, $p = 2$. Hence the number of factors is three. Trying $x + y + z$, we have

$$x \prec x + y + z \qquad y \times z \prec x + y + z,$$

so that this is a factor. Trying $x + y + \bar{z}$, we have

$$x \prec x + y + \bar{z} \qquad y \times z \prec x + y + \bar{z},$$

so that this is also a factor. It is, also, obvious that $x + \bar{y} + z$ is the third factor. Accordingly,

$$x + (y \times z) = (x + y + z) \times (x + y + \bar{z}) \times (x + \bar{y} + z).$$

Again, let us develop the expression

$$(\bar{a} + b + c) \times (a + \bar{b} + \bar{c}) \times (a + b + c).$$

Here $m = 3$, $n = 9$, $p = 3$; so that the number of ultimate aggregants is five. Of the eight possible products of three letters, then, only three are excluded, namely: $(a \times \bar{b} \times \bar{c})$, $(\bar{a} \times b \times c)$, and $(\bar{a} \times \bar{b} \times \bar{c})$. We have, then,

$$(\bar{a}+b+c)\times(a+\bar{b}+\bar{c})\times(a+b+c)=$$
$$(a\times b\times c)+(a\times b\times\bar{c})+(a\times\bar{b}\times c)+(\bar{a}\times b\times\bar{c})+(\bar{a}\times\bar{b}\times c).$$

Third Process. Separate all complex propositions into simple ones by means of the following formulæ from the definitions of + and $\times$:

$$(X+Y\prec Z)=(X\prec Z)\times(Y\prec Z)$$
$$(X\prec Y\times Z)=(X\prec Y)\times(X\prec Z)$$
$$(X+Y\overline{\prec} Z)=(X\overline{\prec} Z)+(Y\overline{\prec} Z)$$
$$(X\overline{\prec} Y\times Z)=(X\overline{\prec} Y)+(X\overline{\prec} Z).$$

In practice, the first three operations will generally be performed off-hand in writing down the premises.

Fourth Process. If we have given two propositions, one of one of the forms

$$a\prec b+x \qquad a\times\bar{x}\prec b,$$

and the other of one of the forms

$$c\prec d+\bar{x} \qquad c\times x\prec d,$$

we may, by the transitiveness of the copula, eliminate x, and so obtain

$$a\times c\prec b+d.$$

Fifth Process. We may transpose any term from subject to predicate or the reverse, by changing it from positive to negative or the reverse, and at the same time its mode of connection from addition to multiplication or the reverse. Thus,

$$(x\times y\prec z)=(x\prec\bar{y}+z).$$

We may, in this way, obtain all the subjects and predicates of any letter; or we may bring all the letters into the subject, leaving the predicate 0, or all into the predicate, leaving the subject ∞.

Sixth Process. Any number of propositions having a common $\left\{\begin{matrix}\text{subject}\\ \text{predicate}\end{matrix}\right\}$ are, taken together, equivalent to their $\left\{\begin{matrix}\text{product}\\ \text{sum}\end{matrix}\right\}$

As an example of this method, we may consider a well-known problem given by Boole. The data are

$$\bar{x}\times\bar{z}\prec v\times(y\times\bar{w}+\bar{y}\times w)$$
$$\bar{v}\times x\times w\prec(y\times z)+(\bar{y}\times\bar{z})$$
$$(x\times y)+(v\times x\times\bar{y})=(z\times\bar{w})+(\bar{z}\times w).$$

The quæsita are: first, to find those predicates of x which involve only y, z, and w; second, to find any relations which may be implied between y, z, w; third, to find the predicates of y; fourth, to find any relation which may be implied between x, z, and w. By the first three processes, mentally performed, we resolve the premises as follows: the first into

$$\bar{x} \times \bar{z} \prec v$$
$$\bar{x} \times \bar{z} \prec y + w$$
$$\bar{x} \times \bar{z} \prec \bar{y} + \bar{w};$$

the second into

$$\bar{v} \times x \times w \prec y + \bar{z}$$
$$\bar{v} \times x \times w \prec \bar{y} + z;$$

the third into

$$x \times y \prec z + w$$
$$x \times y \prec \bar{z} + \bar{w}$$
$$v \times x \times \bar{y} \prec z + w$$
$$v \times x \times \bar{y} \prec \bar{z} + \bar{w}$$
$$z \times \bar{w} \prec x$$
$$z \times \bar{w} \prec v + y$$
$$\bar{z} \times w \prec x$$
$$\bar{z} \times w \prec v + y.$$

We must first eliminate v, about which we want to know nothing. We have, on the one hand, the propositions

$$v \times x \times \bar{y} \prec z + w$$
$$v \times x \times \bar{y} \prec \bar{z} + \bar{w};$$

and, on the other, the propositions

$$\bar{x} \times \bar{z} \prec v$$
$$\bar{v} \times x \times w \prec y + \bar{z}$$
$$\bar{v} \times x \times w \prec \bar{y} + z$$
$$z \times \bar{w} \prec v + y$$
$$\bar{z} \times w \prec v + y.$$

The conclusions from these propositions are obtained by taking one from each set, multiplying their subjects, adding their predicates, and omitting v. The result will be a merely empty proposition if the same letter in the same quality as to being positive or negative be found in the subject and in the predicate, or if it be found twice with opposite qualities either in the subject or in the predicate. Thus, it will be useless to combine the proposition $v \times x \times \bar{y} \prec z + w$ with any which contains $\bar{x}$, y, z, or w in the subject. But all of the second set do this, so that nothing can be concluded from this proposition. So it will be useless to combine $v \times x \times \bar{y} \prec \bar{z} + \bar{w}$ with any which contains $\bar{x}$, y, $\bar{z}$, $\bar{w}$ in the subject, or z in the predicate. This excludes every proposition of the second set except $\bar{v} \times x \times w \prec y + \bar{z}$, which, combined with the proposition under discussion, gives

$$x \times w \prec y + \bar{z} + \bar{w}$$

or

$$x \times w \prec y + \bar{z},$$

which is therefore to be used in place of all the premises containing v.

One of the other propositions, namely, $\bar{x} \times \bar{z} \prec \bar{y} + \bar{w}$ is obviously contained in another, namely: $\bar{z} \times w \prec x$. Rejecting it, our premises are reduced to six, namely:

$$\begin{aligned} &\bar{x} \times \bar{z} \prec y + w \\ &x \times y \prec z + w \\ &x \times y \prec \bar{z} + \bar{w} \\ &z \times \bar{w} \prec x \\ &\bar{z} \times w \prec x \\ &x \times w \prec y + \bar{z}. \end{aligned}$$

The second, third, and sixth of these give the predicates of x. Their product is

$$x \prec (\bar{y} + z + w) \times (\bar{y} + \bar{z} + \bar{w}) \times (y + \bar{z} + \bar{w})$$

or

$$x \prec y \times z \times \bar{w} + y \times \bar{z} \times w + \bar{y} \times z \times \bar{w} + \bar{y} \times \bar{z} \times w + \bar{y} \times \bar{z} \times \bar{w}$$

or

$$x \prec z \times \bar{w} + \bar{z} \times w + \bar{y} \times \bar{z} \times \bar{w}.$$

To find whether any relation between y, z, and w can be obtained by the elimination of x, we find the subjects of x by combining the first, fourth, and fifth premises. Thus we find

$$\bar{y} \times \bar{z} \times \bar{w} + z \times \bar{w} + \bar{z} \times w \prec x.$$

It is obvious that the conclusion from the last two propositions is a merely identical proposition, and therefore no independent relation is implied between y, z, and w.

To find the predicates of y we combine the second and third propositions. This gives

$$y \prec (\bar{x} + z + w) \times (\bar{x} + \bar{z} + \bar{w})$$

or

$$y \prec x \times z \times \bar{w} + x \times \bar{z} \times w + \bar{x}.$$

Two relations between x, z, and w are given in the premises, namely: $z \times \bar{w} \prec x$ and $\bar{z} \times w \prec x$. To find whether any other is implied, we eliminate y between the above proposition and the first and sixth premises. This gives

$$\bar{x} \times \bar{z} \prec x \times z \times \bar{w} + w + \bar{x}$$
$$x \times w \prec x \times z \times \bar{w} + \bar{x} + \bar{z}.$$

The first conclusion is empty. The second is equivalent to $x \times w \prec \bar{z}$, which is a third relation between x, z, and w.

Everything implied in the premises in regard to the relations of x, y, z, w may be summed up in the proposition

$$\infty \prec x + z \times w + y \times \bar{z} \times \bar{w}.$$

CHAPTER III.—THE LOGIC OF RELATIVES.

§1. *Individual and Simple Terms.*

Just as we had to begin the study of Logical Addition and Multiplication by considering ∞ and 0, terms which might have been introduced under the Algebra of the Copula, being defined in terms of the copula only, without the use of + or $\times$, but which had not been there introduced, because they had no application there, so we have to begin the study of relatives by considering the doctrine of individuals and simples,—a doctrine which makes use only of the conceptions of non-relative logic, but which is wholly without use in that part of the subject, while it is the very foundation of the conception of a relative, and the basis of the method of working with the algebra of relatives.

The germ of the correct theory of individuals and simples is to be found in Kant's *Critic of the Pure Reason,* "Appendix to the Transcendental Dialectic," where he lays it down as a regulative principle, that, if

$$a \prec b \qquad b \mathrel{\overline{\prec}} a,$$

then it is always possible to find such a term x, that

$$a \prec x \qquad x \prec b$$
$$x \mathrel{\overline{\prec}} a \qquad b \mathrel{\overline{\prec}} x.$$

Kant's distinction of regulative and constitutive principles is unsound, but this *law of continuity,* as he calls it, must be accepted as a fact. The proof of it, which I have given elsewhere, depends on the continuity of space, time, and the intensities of the qualities which enter into the definition of any term. If, for instance, we say that Europe, Asia, Africa, and North America are continents, but not all the continents, there remains over only South America. But we may distinguish between South America as it now exists and South America in former geological times; we may, therefore, take x as including Europe, Asia, Africa, North America, and South America as it exists now, and every x is a continent, but not every continent is x.

Just as in mathematics we speak of infinitesimals and infinites, which are fictitious limits of continuous quantity, and every statement involving these expressions has its interpretation in the doctrine of limits, so in logic we may define an *individual,* A, as such a term that

$$\mathrm{A} \mathrel{\overline{\prec}} 0,$$

but such that if

$$x < \mathrm{A}$$

then $$x \prec 0.$$

And in the same way, we may define the *simple,* α, as such a term that

$$\infty \mathrel{\overline{\prec}} \alpha,$$

but such that if

$$\alpha < x$$

then $$\infty \prec x.$$

The individual and the simple, as here defined, are ideal limits, and every statement about either is to be interpreted by the doctrine of limits.

Every term may be conceived as a limitless logical sum of individuals, or as a limitless logical product of simples; thus,

$$a = \mathrm{A}_1 + \mathrm{A}_2 + \mathrm{A}_3 + \mathrm{A}_4 + \mathrm{A}_5 + \text{etc.}$$
$$\bar{a} = \bar{\mathrm{A}}_1 \times \bar{\mathrm{A}}_2 \times \bar{\mathrm{A}}_3 \times \bar{\mathrm{A}}_4 \times \bar{\mathrm{A}}_5 \times \text{etc.}$$

It will be seen that a simple is the negative of an individual.

§2. *Relatives.*

A *relative* is a term whose definition describes what sort of a system of objects that is whose first member (which is termed the *relate*) is denoted by the term; and names for the other members of the system (which are termed the *correlates*) are usually appended to limit the denotation still further. In these systems the order of the members is essential; so that (A, B, C) and (A, C, B) are different systems. As an example of a relative, take 'buyer of ——— for ——— from ———'; we may append to this three correlates, thus, 'buyer of every horse of a certain description in the market for a good price from its owner'.

A relative of only one correlate, so that the system it supposes is a pair, may be called a *dual* relative; a relative of more than one correlate may be called *plural.* A non-relative term may be called a term of *singular reference.*

Every relative, like every term of singular reference, is general; its definition describes a system in general terms; and, as general, it may be conceived either as a logical sum of individual relatives, or as a logical product of simple relatives.[21] An individual relative refers to a system all the members of which are individual. The expressions

$$(A:B) \qquad (A:B:C)$$

may denote individual relatives. Taking dual individual relatives, for instance, we may arrange them all in an infinite block, thus,

A : A	A : B	A : C	A : D	A : E etc.
B : A	B : B	B : C	B : D	B : E etc.
C : A	C : B	C : C	C : D	C : E etc.
D : A	D : B	D : C	D : D	D : E etc.
E : A	E : B	E : C	E : D	E : E etc.
etc.	etc.	etc.	etc.	etc.

In the same way, triple individual relatives may be arranged in a cube, and so forth. The logical sum of all the relatives in this infinite block will be the relative universe, ∞, where

$$x \prec \infty,$$

whatever dual relative x may be. It is needless to distinguish the dual universe, the triple universe, etc., because, by adding a perfectly

21. In my "Logic of Relatives," 1870, I have used this expression to designate what I now call *dual relatives.*

indefinite additional member to the system, a dual relative may be converted into a triple relative, etc. Thus, for *lover of a woman,* we may write *lover of a woman coexisting with anything.* In the same way, a term of single reference is equivalent to a relative with an indefinite correlate; thus, *woman* is equivalent to *woman coexisting with anything.* Thus, we shall have

$$\mathrm{A} = \mathrm{A}:\mathrm{A} + \mathrm{A}:\mathrm{B} + \mathrm{A}:\mathrm{C} + \mathrm{A}:\mathrm{D} + \mathrm{A}:\mathrm{E} + \text{etc.}$$
$$\mathrm{A}:\mathrm{B} = \mathrm{A}:\mathrm{B}:\mathrm{A} + \mathrm{A}:\mathrm{B}:\mathrm{B} + \mathrm{A}:\mathrm{B}:\mathrm{C} + \mathrm{A}:\mathrm{B}:\mathrm{D} + \text{etc.}$$

From the definition of a simple term given in the last section, it follows that every simple relative is the negative of an individual term. But while in non-relative logic negation only divides the universe into two parts, in relative logic the same operation divides the universe into 2^n parts, where n is the number of objects in the system which the relative supposes; thus,

$$\begin{aligned}
\infty &= \mathrm{A} + \overline{\mathrm{A}} \\
\infty &= \mathrm{A}:\mathrm{B} + \overline{\mathrm{A}}:\mathrm{B} + \mathrm{A}:\overline{\mathrm{B}} + \overline{\mathrm{A}}:\overline{\mathrm{B}} \\
\infty &= (\mathrm{A}:\mathrm{B}:\mathrm{C}) + (\overline{\mathrm{A}}:\mathrm{B}:\mathrm{C}) + (\mathrm{A}:\overline{\mathrm{B}}:\mathrm{C}) + (\mathrm{A}:\mathrm{B}:\overline{\mathrm{C}}) \\
&\quad + (\overline{\mathrm{A}}:\overline{\mathrm{B}}:\overline{\mathrm{C}}) + (\mathrm{A}:\overline{\mathrm{B}}:\overline{\mathrm{C}}) + (\overline{\mathrm{A}}:\mathrm{B}:\overline{\mathrm{C}}) + (\overline{\mathrm{A}}:\overline{\mathrm{B}}:\mathrm{C}).
\end{aligned}$$

Here, we have

$$\mathrm{A} = \mathrm{A}:\mathrm{B} + \mathrm{A}:\overline{\mathrm{B}};\quad \overline{\mathrm{A}} = \overline{\mathrm{A}}:\mathrm{B} + \overline{\mathrm{A}}:\overline{\mathrm{B}};$$
$$\mathrm{A}:\mathrm{B} = \mathrm{A}:\mathrm{B}:\mathrm{C} + \mathrm{A}:\mathrm{B}:\overline{\mathrm{C}};\quad \mathrm{A}:\overline{\mathrm{B}} = \mathrm{A}:\overline{\mathrm{B}}:\mathrm{C} + \mathrm{A}:\overline{\mathrm{B}}:\overline{\mathrm{C}};$$
$$\overline{\mathrm{A}}:\mathrm{B} = \overline{\mathrm{A}}:\mathrm{B}:\mathrm{C} + \overline{\mathrm{A}}:\mathrm{B}:\overline{\mathrm{C}};\quad \overline{\mathrm{A}}:\overline{\mathrm{B}} = \overline{\mathrm{A}}:\overline{\mathrm{B}}:\mathrm{C} + \overline{\mathrm{A}}:\overline{\mathrm{B}}:\overline{\mathrm{C}}.$$

It will be seen that a term which is individual when considered as n-fold is not so when considered as more than n-fold; but an n-fold term when made $(m + n)$-fold, is individual as to n members of the system, and indefinite as to m members.

Instead of considering the system of a relative as consisting of non-relative individuals, we may conceive of it as consisting of relative individuals. Thus, since

$$\mathrm{A} = \mathrm{A}:\mathrm{A} + \mathrm{A}:\mathrm{B} + \mathrm{A}:\mathrm{C} + \mathrm{A}:\mathrm{D} + \text{etc.},$$

we have

$$\mathrm{A}:\mathrm{B} = (\mathrm{A}:\mathrm{A}):\mathrm{B} + (\mathrm{A}:\mathrm{B}):\mathrm{B} + (\mathrm{A}:\mathrm{C}):\mathrm{B} + (\mathrm{A}:\mathrm{D}):\mathrm{B} + \text{etc.}$$

But

$$\mathrm{B} = \mathrm{B}:\mathrm{A} + \mathrm{B}:\mathrm{B} + \mathrm{B}:\mathrm{C} + \mathrm{B}:\mathrm{D} + \text{etc.};$$

so that

$$A:B = A:(B:A) + A:(B:B) + A:(B:C) + A:(B:D) + \text{etc.}$$

§3. *Relatives connected by Transposition of Relate and Correlate.*

Connected with every dual relative, as

$$l = \Sigma(A:B) = \Pi(\alpha:\beta),$$

is another which is called its *converse,*

$$k\text{-}l = \Sigma(B:A) = \Pi(\beta:\alpha),$$

in which the relate and correlate are transposed. The converse, k, is itself a relative, being

$$k = \Sigma[(A:B):(B:A)];$$

that is, it is the first of any pair which embraces two individual dual relatives, each of which is the converse of the other. The converse of the converse is the relation itself, thus

$$k\text{-}k\text{-}l = l,$$

or say

$$kk = 1.$$

We have also

$$\begin{aligned} k\text{-}\bar{l} &= \overline{k\text{-}l} \\ k\Sigma &= \Sigma k \\ k\Pi &= \Pi k. \end{aligned}$$

In the case of triple relatives there are five transpositions possible. Thus, if

$$b = \Sigma[(A:B):C],$$

we may write

$$\begin{aligned} \mathrm{I}b &= \Sigma[(B:A):C] \\ \mathrm{J}b &= \Sigma[(A:C):B] \\ \mathrm{K}b &= \Sigma[(C:B):A] \\ \mathrm{L}b &= \Sigma[(C:A):B] \\ \mathrm{M}b &= \Sigma[(B:C):A]. \end{aligned}$$

Here we have

$$\begin{aligned} \mathrm{LM} &= \mathrm{ML} = 1 \\ \mathrm{II} &= \mathrm{JJ} = \mathrm{KK} = 1 \\ \mathrm{IJ} &= \mathrm{JK} = \mathrm{KI} = \mathrm{L} \\ \mathrm{JI} &= \mathrm{KJ} = \mathrm{IK} = \mathrm{M} \\ \mathrm{IL} &= \mathrm{MI} = \mathrm{J} = \mathrm{KM} = \mathrm{LK} \\ \mathrm{JL} &= \mathrm{MJ} = \mathrm{K} = \mathrm{IM} = \mathrm{LI} \\ \mathrm{KL} &= \mathrm{MK} = \mathrm{I} = \mathrm{JM} = \mathrm{LJ}. \end{aligned}$$

If we write $a\!:\!b$ to express the operation of putting A in place of B in the original relative

$$b = \Sigma\,[(\mathrm{A}\!:\!\mathrm{B})\!:\!\mathrm{C}],$$

then we have

$$\begin{aligned} \mathrm{I} &= a\!:\!b + b\!:\!a + c\!:\!c \\ \mathrm{J} &= a\!:\!a + b\!:\!c + c\!:\!b \\ \mathrm{K} &= a\!:\!c + b\!:\!b + c\!:\!a \\ \mathrm{L} &= a\!:\!b + b\!:\!c + c\!:\!a \\ \mathrm{M} &= a\!:\!c + b\!:\!a + c\!:\!b \\ 1 &= a\!:\!a + b\!:\!b + c\!:\!c. \end{aligned}$$

Then we have

$$\mathrm{I} + \mathrm{J} + \mathrm{K} = 1 + \mathrm{L} + \mathrm{M},$$

which does not imply

$$(\mathrm{I} + \mathrm{J} + \mathrm{K})l = (1 + \mathrm{L} + \mathrm{M})l.$$

In a similar way the n-fold relative will have $(n! - 1)$ transposition-functions.

§4. *Classification of Relatives.*

Individual relatives are of one or other of the two forms

$$\mathrm{A}\!:\!\mathrm{A} \qquad \mathrm{A}\!:\!\mathrm{B},$$

and simple relatives are negatives of one or other of these two forms.

The forms of general relatives are of infinite variety, but the following may be particularly noticed.

Relatives may be divided into those all whose individual aggregants are of the form A : A and those which contain individuals of the form A : B. The former may be called *concurrents,* the latter *opponents.* Concurrents express a mere agreement among objects. Such, for instance, is the relative *'man that is ———',* and a similar relative may be formed from any term of singular reference. We may denote such a relative by the symbol for the term of singular reference with a comma after it; thus $(m,)$ will denote 'man that is ———' if (m) denotes 'man'. In the same way a comma affixed to an n-fold relative will convert it into an $(n + 1)$-fold relative. Thus, (l) being 'lover of ———', $(l,)$ will be 'lover that is ——— of ———'.

The negative of a concurrent relative will be one each of whose simple components is of the form $\overline{\mathrm{A}\!:\!\mathrm{A}}$, and the negative of an opponent relative will be one which has components of the form $\overline{\mathrm{A}\!:\!\mathrm{B}}$.

We may also divide relatives into those which contain individual aggregants of the form A : A and those which contain only aggregants of the form A : B. The former may be called *self-relatives,* the latter *alio-relatives.* We also have negatives of self-relatives and negatives of alio-relatives.

These different classes have the following relations. Every negative of a concurrent and every alio-relative is both an opponent and the negative of a self-relative. Every concurrent and every negative of an alio-relative is both a self-relative and the negative of an opponent. There is only one relative which is both a concurrent and the negative of an alio-relative; this is 'identical with ——'. There is only one relative which is at once an alio-relative and the negative of a concurrent; this is the negative of the last, namely, 'other than ——'. The following pairs of classes are mutually exclusive, and divide all relatives between them:

Alio-relatives and self-relatives,
Concurrents and opponents,
Negatives of alio-relatives and negatives of self-relatives,
Negatives of concurrents and negatives of opponents.

No relative can be at once either an alio-relative or the negative of a concurrent, and at the same time either a concurrent or the negative of an alio-relative.

We may append to the symbol of any relative a semicolon to convert it into an alio-relative of a higher order. Thus (l;) will denote a 'lover of —— that is not ——'.

This completes the classification of dual relatives founded on the difference of the fundamental forms A : A and A : B. Similar considerations applied to triple relatives would give rise to a highly complicated development, inasmuch as here we have no less than five fundamental forms of individuals, namely,

$$(A:A):A \quad (A:A):B \quad (A:B):A \quad (B:A):A \quad (A:B):C.$$

The number of individual forms for the $(n + 2)$-fold relative is

$$2 + (2^n - 1)\cdot 3 + \frac{1}{2!}\left\{(3^n - 1) - 2(2^n - 1)\right\}\cdot 4 + \frac{1}{3!}$$
$$\left\{(4^n - 1) - 3(3^n - 1) + 3(2^n - 1)\right\}\cdot 5 + \frac{1}{4!}$$
$$\left\{(5^n - 1) - 4(4^n - 1) + 6(3^n - 1) - 4(2^n - 1)\right\}\cdot 6 + \frac{1}{5!}$$
$$\left\{(6^n - 1) - 5(5^n - 1) + 10(4^n - 1) - 10(3^n - 1) + 5(2^n - 1)\right\}\cdot 7 + \text{etc.}$$

If this number be called fn, we have

$$\Delta^n f0 = f(n-1)$$
$$f0 = 1.$$

The form of calculation is

1					
2	1				
5	3	2			
15	10	7	5		
52	37	27	20	15	
203	151	114	87	67	52

where the diagonal line is copied number by number from the vertical line, as fast as the latter is computed.

Relatives may also be classified according to the general amount of filling up of the above-mentioned block, cube, etc. they present. In the first place, we have such relatives in whose block, cube, etc. every line in a certain direction in which there is a single individual is completely filled up. Such relatives may be called *complete in regard to* the relate, or first, second, third, etc. correlate. The dual relatives which are equivalent to terms of singular reference are complete as to their correlate.

A relative may be incomplete with reference to a certain correlate or to its relate, and yet every individual of the universe may in some way enter into that correlate or relate. Such a relative may be called *unlimited* in reference to the correlate or relate in question. Thus, the relative

$$A:A + A:B + C:C + C:D + E:E + E:F + G:G + G:H + \text{etc.}$$

is unlimited as to its correlate. The negative of an unlimited relative will be unlimited unless the relative has as an integrant a relative which is complete with regard to every other relate and correlate than that with reference to which the given relative is unlimited.

A totally unlimited relative is one which is unlimited in reference to the relate and all the correlates. A totally unlimited relative in which each letter enters only once into the relate and once into the correlate is termed a *substitution.*

Certain classes of relatives are characterized by the occurrence or non-occurrence of certain individual aggregants related in a definite way to others which occur. A set of individual dual relatives each of which has for its relate the correlate of the last, the last of all being considered as preceding the first of all, may be called a *cycle.* If there are n individuals in the cycle it may be called a cycle of the n^{th} order.

For instance,

$$\mathrm{A:B} \quad \mathrm{B:C} \quad \mathrm{C:D} \quad \mathrm{D:E} \quad \mathrm{E:A}$$

may be called the cycle of the fifth order. Now, if a certain relative be such that of any cycle of the n^{th} order of which it contains any m terms, it also contains the remaining $(n-m)$ terms, it may be called a cyclic relative of the n^{th} order and m^{th} degree. If, on the other hand, of any cycle of the n^{th} order of which it contains m terms the remaining $(n-m)$ are wanting, the relative may be called an anticyclic relative of the n^{th} order and m^{th} degree.

A cyclic relative of the first order and zero degree contains all individual components of the form A:A. A cyclic relative of the second order and first degree is called an *equiparant* in opposition to a *disquiparant.*

A highly important class of relatives is that of *transitives;* that is to say, those which for every two individual terms of the forms (A:B) and (B:C) also possess a term of the form (A:C).

§5. *The Composition of Relatives.*

Suppose two relatives either individual or simple, and having the relate or correlate of the first identical with the relate or correlate of the second or of its negative. This pair of relatives will then be of one or other of sixteen forms, viz.:

$$\begin{array}{llll}
(\mathrm{A:B})\,(\mathrm{B:C}) & \overline{(\mathrm{A:B})}\,(\mathrm{B:C}) & (\mathrm{A:B})\,\overline{(\mathrm{B:C})} & \overline{(\mathrm{A:B})}\,\overline{(\mathrm{B:C})} \\
(\mathrm{A:B})\,(\mathrm{C:B}) & \overline{(\mathrm{A:B})}\,(\mathrm{C:B}) & (\mathrm{A:B})\,\overline{(\mathrm{C:B})} & \overline{(\mathrm{A:B})}\,\overline{(\mathrm{C:B})} \\
(\mathrm{B:A})\,(\mathrm{B:C}) & \overline{(\mathrm{B:A})}\,(\mathrm{B:C}) & (\mathrm{B:A})\,\overline{(\mathrm{B:C})} & \overline{(\mathrm{B:A})}\,\overline{(\mathrm{B:C})} \\
(\mathrm{B:A})\,(\mathrm{C:B}) & \overline{(\mathrm{B:A})}\,(\mathrm{C:B}) & (\mathrm{B:A})\,\overline{(\mathrm{C:B})} & \overline{(\mathrm{B:A})}\,\overline{(\mathrm{C:B})}.
\end{array}$$

Now we may conceive an operation upon any one of these sixteen pairs of relatives of such a nature that it will produce one or other of the four forms (A:C), $\overline{(\mathrm{A:C})}$, (C:A), $\overline{(\mathrm{C:A})}$. Thus, we shall have sixty-four operations in all.

We may symbolize them as follows: Let

$$\mathrm{A:B}\,(|||)\,\mathrm{B:C} = \mathrm{A:C};$$

that is, let $(|||)$ signify such an operation that this formula necessarily holds. The three lines in the sign of this operation are to refer respectively to the first relative operated upon, the second relative operated upon, and to the result. When either of these lines is replaced by a hyphen (–), let the operation signified be such that the negative of the corresponding relative must be substituted in the above formula. Thus,

$$\overline{\mathrm{A:B}}(-||)\mathrm{B:C}=\mathrm{A:C}.$$

In the same way, let a semicircle ($\smile$) signify that the converse of the corresponding relative is to be taken. The hyphen and the semicircle may be used together. If, then, we write the symbol of a relative with a semicircle or curve over it to denote the converse of that relative, we shall have, for example,

$$\overset{\frown}{\mathrm{A:B}}(\smile||)\mathrm{B:C}=\mathrm{A:C}.$$

Then any combination of the relatives a and e, in this order, is equivalent to others formed from this by making any of the following changes:

First. Putting a straight or curved mark over a and changing the first mark of the sign of operation in the corresponding way; that is,

for $\breve{a}$, from $|$ to $\smile$ or from $-$ to $\overset{-}{\smile}$ or conversely,
for $\bar{a}$, from $|$ to $-$ or from $\smile$ to $\overset{-}{\smile}$ or conversely,
for $\breve{\bar{a}}$, from $|$ to $\overset{-}{\smile}$ or from $-$ to $\smile$ or conversely.

Second. Making similar simultaneous modifications of e and of the second mark.

Third. Changing the third mark from $|$ to $-$ or from $\smile$ to $\overset{-}{\smile}$ or conversely, and simultaneously writing the mark of negation over the whole expression.

Fourth. Changing the third mark from $|$ to $\smile$ or from $-$ to $\overset{-}{\smile}$ or conversely, and interchanging a and e and also the first and second marks.

We have thus far defined the effect of the sixty-four operations when certain members of the individual relatives operated upon are identical. When these members are not identical, we may suppose either that the operation $|||$ produces either the first or second relative or 0. We cannot suppose that it produces ∞ for a reason which will appear further on. Let us elect the formula

$$\mathrm{A:B}(|||)\mathrm{C:D}=0.$$

The other excluded operations will be included in a certain manner, as we shall see below. From this formula, by means of the rules of equivalence, it will follow that all operations in whose symbol there is no hyphen in the third place will also give 0 in like circumstances, while all others will give $\bar{0}$ or ∞.

We have thus far only defined the effect of the sixty-four operations upon individual or simple terms. To extend the definitions to other cases, let us suppose first that the rules of equivalence are generally valid, and second, that

$$\text{If } a -\!\!< b \text{ and } c -\!\!< d \text{ then } a(|||)c -\!\!< b(|||)d$$

or

$$(a -\!\!< b) \times (c -\!\!< d) -\!\!< \{a(|||)c -\!\!< b(|||)d\}.$$

Then, this rule will hold good in all operations in whose symbols the first and second places agree with the third in respect to having or not having hyphens. For operations, in whose symbols the $\left\{\begin{matrix}\text{first}\\ \text{second}\end{matrix}\right\}$ mark disagrees with the third in this respect we must write $\left\{\begin{matrix}b -\!\!< a\\ d -\!\!< c\end{matrix}\right\}$ instead of $\left\{\begin{matrix}a -\!\!< b\\ c -\!\!< d\end{matrix}\right\}$ in this rule. Thus, the sixty-four operations are divisible into four classes according to which one of the four rules so produced they follow.

It now appears that only the hyphens and not the curved marks are of significance in reference to the rule which an operation follows. Let us accordingly reject all operations whose symbols contain curved marks, and there remain only eight. For these eight the following formulæ hold:

$$\begin{array}{ll}
A:B(|||)B:C = A:C & A:B(||-)B:C = \overline{A:C} \\
\overline{A:B}(-||)B:C = A:C & \overline{A:B}(-|-)B:C = \overline{A:C} \\
A:B(|-|)\overline{B:C} = A:C & A:B(|--)\overline{B:C} = \overline{A:C} \\
\overline{A:B}(--|)\overline{B:C} = A:C & \overline{A:B}(---)\overline{B:C} = \overline{A:C} \\
A:B(|||)C:D = 0 & A:B(||-)C:D = \infty \\
\overline{A:B}(-||)C:D = 0 & \overline{A:B}(-|-)C:D = \infty \\
A:B(|-|)\overline{C:D} = 0 & A:B(|--)\overline{C:D} = \infty \\
\overline{A:B}(--|)\overline{C:D} = 0 & \overline{A:B}(---)\overline{C:D} = \infty
\end{array}$$

$$\begin{array}{l}
(a -\!\!< b) \times (c -\!\!< d) -\!\!< \{a(|||)c -\!\!< b(|||)d\} \\
(a -\!\!< b) \times (c -\!\!< d) -\!\!< \{a(---)c -\!\!< b(---)d\} \\[1ex]
(b -\!\!< a) \times (c -\!\!< d) -\!\!< \{a(-||)c -\!\!< b(-||)d\} \\
(b -\!\!< a) \times (c -\!\!< d) -\!\!< \{a(|--)c -\!\!< b(|--)d\} \\[1ex]
(a -\!\!< b) \times (d -\!\!< c) -\!\!< \{a(|-|)c -\!\!< b(|-|)d\} \\
(a -\!\!< b) \times (d -\!\!< c) -\!\!< \{a(-|-)c -\!\!< b(-|-)d\} \\[1ex]
(b -\!\!< a) \times (d -\!\!< c) -\!\!< \{a(--|)c -\!\!< b(--|)d\} \\
(b -\!\!< a) \times (d -\!\!< c) -\!\!< \{a(||-)c -\!\!< b(||-)d\}.
\end{array}$$

As it is inconvenient to consider so many as eight distinct operations, we may reject one-half of these so as to retain one under each of the four rules. We may reject all those whose symbols contain an odd number of hyphens (as being negative). We then retain four, to which we may assign the following names and symbols:

$a(\|\|\|)e = ae$	*Relative or external multiplication.*
$a(\|--)e = {}^a e$	*Regressive involution.*
$a(-\|-)e = a^e$	*Progressive involution.*
$a(--\|)e = a\circ e$	*Transaddition.*[22]

We have then the following table of equivalents, negatives, and converses:[23]

x	$\bar{x}$	$\breve{x}$	$\breve{\bar{x}}$
$ae = \bar{a}\circ\bar{e}$	$\bar{a}^e = {}^a\bar{e}$	$\breve{e}\breve{a} = \breve{\bar{e}}\circ\breve{\bar{a}}$	$\breve{\bar{e}}^{\breve{a}} = {}^{\breve{e}}\breve{\bar{a}}$
$a^e = {}^{\bar{a}}\bar{e}$	$\bar{a}e = a\circ\bar{e}$	${}^{\breve{e}}\breve{a} = \breve{\bar{e}}^{\breve{\bar{a}}}$	$\breve{e}\breve{\bar{a}} = \breve{\bar{e}}\circ\breve{a}$
${}^a e = \bar{a}^{\bar{e}}$	$a\bar{e} = \bar{a}\circ e$	$\breve{e}^{\breve{a}} = {}^{\breve{\bar{e}}}\breve{\bar{a}}$	$\breve{\bar{e}}\breve{a} = \breve{e}\circ\breve{\bar{a}}$
$a\circ e = \bar{a}\,\bar{e}$	$a^{\bar{e}} = {}^{\bar{a}}e$	$\breve{e}\circ\breve{a} = \breve{\bar{e}}\,\breve{\bar{a}}$	$\breve{e}^{\breve{\bar{a}}} = {}^{\breve{\bar{e}}}\breve{a}$

If l denote 'lover' and s 'servant', then

ls denotes whatever is lover of a servant of ———,

l^s whatever is lover of every servant of ———,

${}^l s$ whatever is in every way (in which it loves at all) lover of a servant of ———,

$l\circ s$ whatever is not a non-lover only of a servant of ———
or whatever is not a lover of everything but servants of ———
or whatever is some way a non-lover of some thing besides servants of ———.

§6. *Methods in the Algebra of Relatives.*

The universal method in this algebra is the method of limits. For certain letters are to be substituted an infinite sum of individuals or product of simples; whereupon certain transformations become possible which could not otherwise be effected.

22. The first three of these were studied by De Morgan ("On the Syllogism, No. IV"); the last is new. The above names for the first three (except the adjective *external* suggested by Grassmann's operation) are given in my "Logic of Relatives."

23. A similar table is given by De Morgan. Of course, it lacks the symmetry of this, because he had not the fourth operation.

The following theorems are indispensible for the application of this method:

1st. $$l^{\mathrm{A:B}} = l(\mathrm{A:B}) + k\overline{\mathrm{B}}.$$

Since $\overline{\mathrm{B}}$ is equivalent to the relative term which comprises all individual relatives whose relates are not B, so $k\overline{\mathrm{B}}$ may be conveniently used, as it is here, to express the aggregate of all individual relatives whose correlate is $\overline{\mathrm{B}}$. To prove this proposition, we observe that

$$l^{\mathrm{A:B}} = \overline{\bar{l}(\mathrm{A:B})}.$$

Now $\bar{l}(\mathrm{A:B})$ contains only individual relatives whose correlate is B, and of these it contains those which are not included in $l(\mathrm{A:B})$. Hence the negative of $\bar{l}(\mathrm{A:B})$ contains all individual relatives whose correlates are not B, together with all contained in $l(\mathrm{A:B})$. Q. E. D.

2nd. $${}^{\mathrm{A:B}}l = (\mathrm{A:B})l + \overline{\mathrm{A}}.$$

Here $\overline{\mathrm{A}}$ is used to denote the aggregate of all individual relatives whose relates are not A. This proposition is proved like the last.

3rd. $$\overline{\mathrm{A:B}}^{\,l} = (\mathrm{A:B})\bar{l} + \overline{\mathrm{A}}.$$

This is evident from the second proposition, because

$$\overline{\mathrm{A:B}}^{\,l} = {}^{(\mathrm{A:B})}\bar{l}.$$

4th. $${}^{l}\overline{\mathrm{A:B}} = \bar{l}(\mathrm{A:B}) + k\overline{\mathrm{B}}.$$

Another method of working with the algebra is by means of negations. This becomes quite indispensible when the operations are defined by negations, as in this paper.

To illustrate the use of these methods, let us investigate the relations of ${}^{l}b$ and l^{b} to lb when l and b are totally unlimited relatives.

Write

$$l = \Sigma_i(\mathrm{L}_i : \mathrm{M}_i) \qquad b = \Sigma_j(\mathrm{B}_j : \mathrm{C}_j).$$

Then, by the rules of the last section,

$${}^{l}b \prec {}^{\mathrm{L:M}}b \qquad l^{b} \prec l^{\mathrm{B:C}};$$

whence, by the second and third propositions above,

$${}^{l}b \prec (\mathrm{L}_i : \mathrm{M}_i)b + \overline{\mathrm{L}}_i \qquad l^{b} \prec l(\mathrm{B}_j : \mathrm{C}_j) + k\overline{\mathrm{B}}_j.$$

But by the first rule of the last section

$$(\mathrm{L}_i : \mathrm{M}_i)b \prec lb \quad l(\mathrm{B}_j : \mathrm{C}_j) \prec lb;$$

hence,

$${}^l b \prec lb + \overline{\mathrm{L}}_i \quad l^b \prec lb + k\overline{\mathrm{B}}_j.$$

There will be propositions like these for all the different values of i and j. Multiplying together all those of the several sets, we have

$${}^l b \prec lb + \Pi_i \overline{\mathrm{L}}_i \quad l^b \prec lb + \Pi_j k\overline{\mathrm{B}}_j.$$

But

$$\Pi_i \overline{\mathrm{L}}_i = \overline{\Sigma_i \mathrm{L}_i} \quad \Pi_j k\overline{\mathrm{B}}_j = \overline{\Sigma_j k\mathrm{B}_j},$$

and since the relatives are unlimited,

$$\begin{array}{ll} \Sigma_i \mathrm{L}_i = \infty & \Sigma_j k\mathrm{B}_j = \infty \\ \overline{\Sigma_i \mathrm{L}_i} = 0 & \overline{\Sigma_j k\mathrm{B}_j} = 0; \end{array}$$

hence

$${}^l b \prec lb \quad l^b \prec lb.$$

In the same way it is easy to show that, if the negatives of l and b are totally unlimited,

$$l^b \prec l \circ b \quad {}^l b \prec l \circ b.$$

§7. *The General Formulæ for Relatives.*

The principal formulæ of this algebra may be divided into *distribution formulæ* and *association formulæ*. The distribution formulæ are those which give the equivalent of a relative compounded with a sum or product of two relatives in such terms as to separate the latter two relatives. The association formulæ are those which give the equivalent of a relative A compounded with a compound of B and C in terms of a compound of A and B compounded with C.

I. DISTRIBUTION FORMULÆ.

1. AFFIRMATIVE.

i. *Simple Formulæ.*

$(a + b)c = ac + bc$	$a(b + c) = ab + ac$
$(a \times b)^c = a^c \times b^c$	$a^{b+c} = a^b \times a^c$
${}^{a+b}c = {}^a c \times {}^b c$	${}^a(b \times c) = {}^a b \times {}^a c$
$(a \times b) \circ c = (a \circ c) + (b \circ c)$	$a \circ (b \times c) = (a \circ b) + (a \circ c)$

ii. *Developments.*

$$(a \times b)c = \Pi_p\{a(c \times p) + b(c \times \bar{p})\} \qquad a(b \times c) = \Pi_p\{(a \times p)b + (a \times \bar{p})c\}$$
$$(a + b)^c = \Sigma_p\{a^{c \times p} \times b^{c \times \bar{p}}\} \qquad a^{b \times c} = \Sigma_p\{(a + p)^b \times (a + \bar{p})^c\}$$
$${}^{(a \times b)}c = \Sigma_p\{{}^{a}(c + p) \times {}^{b}(c + \bar{p})\} \qquad {}^{a}(b + c) = \Sigma_p\{{}^{a \times p}b \times {}^{a \times \bar{p}}c\}$$
$$(a + b) \circ c = \Pi_p\{a \circ (c + p) + b \circ (c + \bar{p})\} \qquad a \circ (b + c) = \Pi_p\{(a + p) \circ b + (a + \bar{p}) \circ c\}$$

2. Negative.

i. *Simple Formulæ.*

$$\overline{(a + b)c} = \overline{ac} \times \overline{bc} \qquad \overline{a(b + c)} = \overline{ab} \times \overline{ac}$$
$$\overline{(a \times b)^c} = \overline{a^c} + \overline{b^c} \qquad \overline{a^{b+c}} = \overline{a^b} + \overline{a^c}$$
$$\overline{{}^{a+b}c} = \overline{{}^{a}c} + \overline{{}^{b}c} \qquad \overline{{}^{a}(b \times c)} = \overline{{}^{a}b} + \overline{{}^{a}c}$$
$$\overline{(a \times b) \circ c} = \overline{a \circ c} \times \overline{b \circ c} \qquad \overline{a \circ (b \times c)} = \overline{a \circ b} \times \overline{a \circ c}$$

ii. *Developments.*

$$\overline{(a \times b)c} = \Sigma_p\{\overline{a(c \times p)} \times \overline{b(c \times \bar{p})}\} \qquad \overline{a(b \times c)} = \Sigma_p\{\overline{(a \times p)b} \times \overline{(a \times \bar{p})c}\}$$
$$\overline{(a + b)^c} = \Pi_p\{\overline{a^{c \times p}} + \overline{b^{c \times p}}\} \qquad \overline{\imath^{b \times c}} = \Pi_p\{\overline{(a + p)^b} + \overline{(a + \bar{p})^c}\}$$
$$\overline{{}^{(a \times b)}c} = \Pi_p\{\overline{{}^{a}(c + p)} + \overline{{}^{b}(c + \bar{p})}\} \qquad \overline{{}^{a}(b + c)} = \Pi_p\{\overline{{}^{a \times p}b} + \overline{{}^{a \times \bar{p}}c}\}$$
$$\overline{(a + b) \circ c} = \Sigma_p\{\overline{a \circ (c + p)} \times \overline{b \circ (c + \bar{p})}\} \qquad \overline{a \circ (b + c)} = \Sigma_p\{\overline{(a + p) \circ b} \times \overline{(a + \bar{p}) \circ c}\}$$

II. ASSOCIATION FORMULÆ.

1. Affirmative.

i. *Simple Formulæ.*

$$\overline{{}^{a}(\overline{bc})} = a(bc) = (ab)c = \overline{(\overline{ab})^{c}} \qquad \overline{a(\overline{b^c})} = {}^{a}(b^c) = ({}^{a}b)^c = \overline{(\overline{{}^{a}b})c}$$
$$\overline{a \circ (\overline{b \circ c})} = a^{(b \circ c)} = {}^{(a \circ b)}c = \overline{(\overline{a \circ b}) \circ c} \qquad \overline{a^{(\overline{{}^{b}c})}} = a \circ ({}^{b}c) = (a^b) \circ c = \overline{{}^{(\overline{{}_{a}b})}c}$$
$$\overline{a \circ (\overline{bc})} = a^{(bc)} = (a^b)^c = \overline{(\overline{a^b})c} \qquad \overline{a(\overline{{}^{b}c})} = {}^{a}({}^{b}c) = {}^{(ab)}c = \overline{(\overline{ab}) \circ c}$$
$$\overline{{}^{a}(\overline{b \circ c})} = a(b \circ c) = ({}^{a}b) \circ c = \overline{{}^{(\overline{ab})}c} \qquad \overline{a^{(\overline{bc})}} = a \circ (b^c) = (a \circ b)c = \overline{(\overline{a \circ b})^c}$$

ii. *Developments.*

(A and E are individual aggregants, and α and ϵ simple components of a and e. The summations and products are relative to all such aggregants and components. The formulæ are of four classes; and for any relative c either all formulæ of Class 1 or all of Class 2, and also either all of Class 3 or all of Class 4 hold good.)

Class 1.	Class 2.
$\overline{a(\overline{bc})} = {}^{a}(bc) = \Pi\{({}^{\mathrm{A}}b)c\} = \Pi\{\overline{(\overline{{}^{\mathrm{A}}b})^c}\}$	$\overline{(\overline{c \circ d})e} = (c \circ d)^e = \Pi\{c \circ (d\mathrm{E})\} = \Pi\{\overline{c^{(\overline{d\mathrm{E}})}}\}$
$\overline{a^{(\overline{bc})}} = a \circ (bc) = \Sigma\{(\alpha \circ b)^c\} = \Sigma\{\overline{(\overline{\alpha \circ b})c}\}$	$\overline{{}^{(\overline{c \circ d})}e} = (c \circ d) \circ e = \Sigma\{c^{({}^{d}e)}\} = \Sigma\{\overline{c \circ (\overline{{}^{d}\epsilon})}\}$
$\overline{a \circ (\overline{b^c})} = a^{(b^c)} = \Pi\{(\alpha^b)c\} = \Pi\{\overline{(\overline{\alpha^b})^c}\}$	$\overline{(\overline{c^d}) \circ e} = {}^{(c^d)}e = \Pi\{c \circ (d \circ \epsilon)\} = \Pi\{\overline{c^{(\overline{d \circ \epsilon})}}\}$
$\overline{{}^{a}(\overline{b^c})} = a(b^c) = \Sigma\{(\mathrm{A}b)^c\} = \Sigma\{\overline{(\overline{\mathrm{A}b})c}\}$	$\overline{(\overline{c^d})^e} = (c^d)e = \Sigma\{c^{(d^{\mathrm{E}})}\} = \Sigma\{\overline{c \circ (\overline{d^{\mathrm{E}}})}\}$

CLASS 3.	CLASS 4.
$\overline{a^{\overline{(b\circ c)}}} = a\circ(b\circ c) = \Sigma\{{}^{(\alpha^{b})}c\} = \Sigma\{\overline{(\overline{\alpha^{b}})c}\}$	$\overline{{}^{\overline{(cd)}}e} = (cd)\circ e = \Sigma\{{}^{c}(d\circ\epsilon)\} = \Sigma\{\overline{c(\overline{d\circ\epsilon})}\}$
$\overline{a(\overline{b\circ c\mathrm{a}})} = {}^{\alpha}(b\circ c) = \Pi\{(\mathrm{A}b)\circ c\} = \Pi\{\overline{{}^{\overline{(\mathrm{A}b)}}c}\}$	$\overline{(\overline{cd})e} = (cd)^{e} = \Pi\{c(d^{\mathrm{E}})\} = \Pi\{\overline{{}^{c}(\overline{d^{\mathrm{E}}})}\}$
$\overline{{}^{a}(\overline{{}^{b}c})} = a({}^{b}c) = \Sigma\{{}^{(\mathrm{A}b)}c\} = \Sigma\}\overline{(\overline{{}^{\mathrm{A}}b})\circ c}\}$	$\overline{(\overline{{}^{c}d})^{e}} = ({}^{c}d)e = \Sigma\{{}^{c}(d\mathrm{E})\} = \Sigma\{\overline{c(\overline{d\mathrm{E}})}\}$
$\overline{a\circ(\overline{{}^{b}c})} = a^{({}^{b}c)} = \Pi\{(\alpha\circ b)\circ c\} = \Pi\{\overline{{}^{\overline{(\alpha\circ b)}}c}\}$	$\overline{(\overline{{}^{c}d})\circ e} = {}^{({}^{c}d)}e = \Pi\{c({}^{d}\epsilon)\} = \Pi\{\overline{{}^{c}(\overline{{}^{d}\epsilon})}\}$

The negative formulæ are derived from the affirmative by simply drawing or erasing lines over the whole of each member of every equation.

In order to see the general rules which these formulæ follow, we must imagine the operations symbolized by three marks, as in the commencement of this chapter. We may term the operation uniting the two letters within the parenthesis the *interior* operation, and that which unites the whole parenthesis to the third letter the *exterior* operation. By *junction-marks* will be meant, in case the parenthesis $\left\{\begin{matrix}\text{follows}\\ \text{precedes}\end{matrix}\right\}$ the third letter, the $\left\{\begin{matrix}\text{first}\\ \text{second}\end{matrix}\right\}$ mark of the symbol of the interior operation and the $\left\{\begin{matrix}\text{second}\\ \text{first}\end{matrix}\right\}$ mark of the symbol of the exterior operation. Using these terms, we may say that the exterior junction-mark and the third mark of the interior operation may always be changed together. When they are the same there is a simple association formula. This formula consists in the possibility of simultaneously interchanging the junction-marks, the third marks, and the exteriority or interiority of the two operations. When the exterior junction-mark and the third mark of the interior operation are unlike, there is a developmental association formula. The general term of this formula is obtained by making the same interchanges as in the simple formulæ, and then changing a to A when after these interchanges ab or ${}^{a}b$ occurs in parenthesis, changing a to α when a^{b} or $a\circ b$ occurs in parenthesis, changing e to E when de or d^{e} occurs in parenthesis, and changing e to ϵ when ${}^{d}e$ or $d\circ e$ occurs in parenthesis. When the third mark in the symbol of the exterior operation is affirmative the development is a summation; when this mark is negative there is a continued product.

In the first column of formulæ, the second mark in the sign of the interior operation is a line in Class 1 and a hyphen in Class 3. In the second column, the first mark in the sign of the interior operation is a hyphen in Class 2 and a line in Class 4.

(To be Continued.)

NOTE TO PAGES 198-99.

The relative 0 ought to be considered as at once a concurrent and an alio-relative, and the relative ∞ as at once the negative of a concurrent and the negative of an alio-relative. The statements in the text require to be modified to this extent.

Chapter IV. The Logic of Plural Relatives

MS 371: Fall–Winter 1880

§1. *First Notions.*

In the last chapter we have considered chiefly dual relatives, and what was said of plural relatives was extremely inadequate. I propose now to take up this subject from another point of view, with a new definition of a plural relative and a new notation.

Logicians distinguish between the *distributive* and *collective* acceptations of a class-name. Thus, father, mother, and children are distributively members of a family, but collectively they are the family. The logic of arithmetic and of mathematics in general turns upon the use of collective terms; yet the logic of these terms has never been regularly developed. In a collective term the order of the components may be of no consequence, but in the general theory it must be regarded as material. Not only ordinary non-relative terms but also relative terms may be collective, as 'father and mother of ———'. Such a term is to be considered as a relative with several relates. There may be one correlate or there may be several. We have already seen (page 196) how an ordinary non-relative term may be regarded as the equivalent of a relative one; and in the same way a non-relative collective may be regarded as a collective having one or more correlates. Let us term a relative plural if it has more than one relate or more than one correlate. Let us say that it is of the n^{th} order, if it has n correlates and of the m^{th} degree, if it has m relates. Then we may lay down this definition. A relate of the m^{th} degree and n^{th} order is a term whose definition describes a system of $m + n$ objects (the arrangement being material) the first m of which the term denotes; and the denotation may be limited by specifying what the other n objects are.

Logic may be divided into the logic of non-relatives, the logic of dual relatives or those of the first degree and order, and the logic of plural relatives. Any relative having x as one of its correlates may be combined with a triple relative having x for its relate and having two correlates to produce a relative having one more correlate than the first. And conversely any relative having more than one correlate may be regarded as a compound of two, one of which has all the correlates of the first except two and also x, while the other is a triple relative having x for its relate and for its two correlates those two of the first relative which are absent from the second. If in the last two sentences we everywhere interchange the words relate and correlate, they remain equally true; it follows that every plural relative not triple may be regarded as a compound of triple relatives; so that we may conceive all terms to be single, dual, or triple.[1]

1. This is insisted upon in my "Logic of Relatives" (1870). Also in my "New List of Categories." The principle is essentially involved in the old maxim that all division may be reduced to dichotomy. Professor Sylvester (*Am. Jour. of Math.* vol.1, p. 78) draws attention to the principle under another aspect.

Results of Pendulum Experiments

P 168: **American Journal of Science and Arts,**
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The following are the results obtained from observations made by me, for the U. S. Coast and Geodetic Survey, at four important stations, for the purpose of comparing the lengths of the seconds pendulum, together with reductions to the sea-level and to the equator. In making the last reduction I have assumed the ellipticity to be = 1:293, which is the latest result from measurements of arcs.

	At station.	At sea-level.	At equator.
Hoboken	$0^{m}.9932052$	$0^{m}.9932074$	$0^{m}.9910003$
Paris	0.9939337	0.9939500	0.9910132
Berlin	0.9942399	0.9942482	0.9909865
Kew	0.9941776	0.9941790	0.9910083

The differences of the figures in the last column from $0^{m}.991$, a value conveniently near their mean, when reduced to oscillations per diem are: Hoboken $+0^{s}.01$; Paris $+0^{s}.58$; Berlin $-0^{s}.59$; Kew $+0^{s}.36$. The following are the residuals of former observations according to Clarke (*Geodesy,* p. 349).

New York $+0^{s}.20$; Paris $-3^{s}.29$; Kew $+2^{s}.89$.

Colonel Clarke has used a value of the ellipticity = 1:292.2 derived from pendulum experiments. This slight difference, however, is not important.

It should be explained that the result for Hoboken is derived from [T^2 Inv.] "Regular Set," given on page 318, and also on page 416 of the Report of the Superintendent of the U. S. Coast and Geodetic Survey for 1876. This number is treated as explained on page 319, where in the second line from the bottom for [T^2 Rev.] read

[T^2 Inv.]. The altitude of the Hoboken station is stated on page 204. The numbers for the European stations are copied from page 320.

The length which I have taken as the metre has been derived from the German Eichungsamt, as fully explained in my report. This is about 19.2 microns shorter than the quantity which is considered to be a metre in our own office of weights and measures, and is admitted in Berlin to be doubtful. It is impossible to fix the true metre at present; but I have but little doubt the above values will ultimately have to be diminished by about twenty microns on account of the error in the standard used.

[The Logic Notebook]

MS 375: 6 November 1880

1880 Nov. 6

Being unable at present to write a second paper on the Algebra of Logic I make notes for one.

Notes on Previous Paper

On Chapter I §4. I have represented that the properties of the negative were

$$
\begin{array}{ll}
1^{st} & \overline{A} = (A \prec x) \\
2^{nd} & A \prec \overline{\overline{A}} \\
3^{rd} & \overline{\overline{A}} \prec A
\end{array}
$$

In my circular I have stated correctly that 2^{nd} follows from first & *incorrectly* that 3^{rd} follows from 1^{st}, which it dont. Very subtle fallacy drawn from long reduction of mood.

Observe 1^{st} is not balanced

Give table of moods etc.

On Chapter II

Usually in algebra $u = 0$ or $u = C$. $\varphi x = C$ represents the truth of a certain proposition the falsity of which is represented by φx having any other value.

So we may conceive that $\varphi(x,y)$ is the number x's that are y's. $\varphi(x,y) = 0$ represents No x is y. Or we may write

$\varphi(x,y)$	number	x's	that are	y's
$\psi(x,y)$	"	"	"	" not y's
$\chi(x,y)$	"	non x's	"	" y's
$\omega(x,y)$	"	non x's	"	" not y's

Equating these to zero gives the four universal propositions, writing

$$\varphi(x,y) \neq 0$$

(with Cayley) give particular propositions.

The above expressions are not really functions of x and y; yet we have the equations

$$x = \varphi + \psi \qquad y = \varphi + \chi$$
$$U - x = \chi + \omega$$

so that we do have 3 equations among the 4 unknowns. If then we assume one more say

$$\varphi + \omega - \psi - \chi = V$$

then we have

$$\varphi = \frac{x+y}{2} - \frac{U-V}{4} = \frac{x+y}{2} + \frac{V-U}{4}$$
$$\psi = \frac{x-y}{2} + \frac{U+V}{4}$$
$$\chi = \frac{y-x}{2} + \frac{U+V}{4}$$
$$\omega = -\frac{x+y}{2} + \frac{3}{4}(U-V)$$

or we might write

$$U - W = \frac{x+y}{2} - \varphi \qquad \text{Put } W = \frac{1}{2}Y + \frac{1}{2}U$$
$$\varphi = \frac{x+y}{2} + W - U \qquad \varphi = \frac{x+y}{2} + \frac{Y-U}{2}$$
$$\psi = \frac{x-y}{2} - W + U \qquad \psi = \frac{x-y-Y+U}{2}$$
$$\chi = \frac{-x+y}{2} - W + U \qquad \chi = \frac{-x+y-Y+U}{2}$$
$$\omega = \frac{-x-y}{2} + W \qquad \omega = \frac{-x-y+Y+U}{2}$$

Here W and V would be quantities logical not mathematical functions of x and y

Logical sum $(x+y) = \dfrac{x+y-Y+U}{2}$ Log Prod $(x \times y) = \dfrac{x+y+Y-U}{2}$

$$(x+\bar{y}) = \frac{x-y+Y+U}{2} \qquad x \times \bar{y} = \frac{x-y-Y+U}{2}$$
$$\bar{x}+y = \frac{-x+y+Y+U}{2} \qquad \bar{x} \times y = \frac{-x+y-Y+U}{2}$$
$$\bar{x}+\bar{y} = \frac{-x-y-Y-3U}{2} \qquad \bar{x} \times \bar{y} = \frac{-x-y+Y+U}{2}$$

In short we need but *one* logical function to express everything We may write this function (x,y) and let it mean

$$\begin{aligned}(x,y) &= i\varphi + j\psi + k\chi + l\omega\\ \mathrm{U} &= \varphi + \psi + \chi + \omega\\ x &= \varphi + \psi\\ y &= \varphi \qquad + \chi\end{aligned}$$

Solving

$$\varphi = \frac{lu - (x,y) + (i-l)x + (k-l)y}{-i+j+k-l}$$

$$x + y - \varphi = \frac{-lu + (x,y) + (k-i)x + (j-i)y}{-i+j+k-l}$$

Nov 7

The same object may be accomplished *without* any logical function (x,y). Namely let us take arbitrarily any two numbers v f and let

$$x = \mathrm{f}$$

signify that whatever object can be chosen is not x

$$x = \mathrm{v}$$

that whatever object can be chosen is x. Then we cannot have at once

$$x = \mathrm{f} \qquad x = \mathrm{v}$$

because f and v are different numbers. The equation

$$(x - \mathrm{f})(x - \mathrm{v}) = 0$$

will denote that $x = \mathrm{f}$ or $x = \mathrm{v}$ that is that every object chosen is either x or not-x. The idea is that for each object chosen this holds so that for *each*, $x = \mathrm{f}$ that is that object is not x or $x = \mathrm{v}$ that is that object is v.

Then

$$(x - \mathrm{f})(y - \mathrm{v}) = 0$$

means each object chosen is either not-x or is y.

Cayley proposes to write the negative of this thus

$$(x - \mathrm{f})(y - \mathrm{v}) \neq 0$$

but this would be: the object chosen must be x and cant be y. This states too much. The true denial of the first equation would be $(x - \mathrm{f})(y - \mathrm{v})$ is not always 0, not is *never* zero.

I have myself proposed (Logic of Rel p 7) to write $x > y$ to mean x's comprise some objects besides y's but properly this can only mean $x = \mathrm{v} \quad y = \mathrm{f}$ (if $\mathrm{v} > \mathrm{f}$) and so is equivalent to Cayley's proposal. But if x_i denotes what x becomes for the i^{th} object, since

$$(x - \mathrm{f})(y - \mathrm{v})$$

if not zero $= -(\mathrm{v} - \mathrm{f})^2$ and therefore essentially negative we may write

$$\Sigma_i (x_i - \mathrm{f})(y_i - \mathrm{v}) < 0$$

to express negative of

$$(x - \mathrm{f})(y - \mathrm{v}) = 0$$

which is the same as

$$\Sigma_i (x_i - \mathrm{f})(y_i - \mathrm{v}) = 0$$

we may write the denial

$$\Sigma_i (x - \mathrm{f})(y - \mathrm{v}) < 0$$

We have

$\Sigma_i xy = 0$ same as $xy = 0$

$\Pi_i (x - y) = 0$ same as there is an object i for which $x = y$

So we may also write the denial

$$\Pi_i (xy - \mathrm{v}^2) = 0$$

provided f is not-v.

Composition of Relatives

It is better to define the 8 modes of compounding dual relatives by De Morgans 8 forms of proposition

ls denotes everything of which it can be said that what is $\breve{l}$ by it is partient of servant of $\breve{l}(ls) \overline{\prec} \bar{s}$

$$l \circ s \qquad \bar{\breve{l}}(l \circ s) \overline{\prec} s$$

$$l^s \qquad \bar{\breve{l}}(l^s) \prec \bar{s}$$

$$^{l}s \qquad l({}^{l}s) \prec s$$

Reciprocally we may consider the eight propositions as differing by the different modes of combining relatives

$l \prec s$	$\infty \prec {}^{\breve{l}}s$	$lx \prec sx$	$x \prec {}^{\breve{l}}(sx)$
$\bar{l} \prec \bar{s}$	$\infty \prec \breve{l}^{s}$	$\bar{l}x \prec \bar{s}x$	$x \prec \breve{l}^{sx}$
$l \overline{\prec} \bar{s}$	$\infty \prec \breve{l}s$	$lx \overline{\prec} \bar{s}x$	

[A Boolian Algebra with One Constant]

MS 378: Winter 1880–81

Every logical notation hitherto proposed has an unnecessary number of signs. It is by means of this excess that the calculus is rendered easy to use and that a symmetrical development of the subject is rendered possible; at the same time, the number of primary formulae is thus greatly multiplied, those signifying facts of logic being very few in comparison with those which merely define the notation. I have thought that it might be curious to see the notation in which the number of signs should be reduced to a minimum; and with this view I have constructed the following. The apparatus of the Boolian calculus consists of the signs, $=$, $>$ (not used by Boole, but necessary to express particular propositions), $+$, $-$, $\times$, 1, 0; in place of these seven signs, I propose to use a single one.

I begin with the description of the notation for conditional or 'secondary' propositions. The different letters signify propositions. Any one proposition written down by itself is considered to be asserted. Thus,

A

means that the proposition A is true. Two propositions written in a pair are considered to be both denied. Thus,

AB

means that the propositions A and B are both false; and

AA

means that A is false. We may have pairs of pairs of propositions and higher complications. In this case we shall make use of commas, semicolons, colons, periods, and parentheses, just as in chemical notation to separate pairs which are themselves paired. These punctuation marks can no more count for distinct signs of the algebra, than the parentheses of the ordinary notation.

To express the proposition that 'If S then P'. First, write

A

for this proposition. But the proposition is that a certain conceivable state of things is absent from the universe of possibility. Hence instead of A we write

BB.

Then B expresses the possibility of S being true and P false. Since, therefore, SS denies S, it follows that (SS, P) expresses B. Hence we write

SS, P; SS, P.

Required to express the two premises 'If S then M' and 'If M, then P'. Let

A

be the two premises. Let B be the denial of the first and C that of the second; then in place of A we write

BC

But we have just seen that B is (SS, M) and that C is (MM, P); accordingly we write

SS, M; MM, P

All the formulae of the calculus may be obtained by development or elimination. The development or elimination having reference say to the letter X, two processes are required, which may be called the erasure of the X's and the erasure of the double X's. The erasure of the X's is performed as follows:

Erase all the X's and fill up each blank with whatever it is paired with. But where there is a double X this cannot be done; in this case erase the whole pair of which the double X forms a part, and fill up the space with whatever it is paired with. Go on following these rules. A pair of which both members are erased is to be considered as doubly erased. A pair of which either member is doubly erased is to be considered as only singly erased, without regard to the condition of the other member. Whatever is singly erased is to be replaced by the repetition of what it is paired with.

To erase the double X's, repeat every X and then erase the X's.

If φ be any expression, $\frac{\varphi}{x}$ what it becomes after erasure of the x's and $\frac{\varphi}{xx}$ what it becomes after erasure of the double x's, then

$$\varphi = \frac{\varphi}{x}x; \frac{\varphi}{xx}, xx.$$

If φ be asserted, then

$$\frac{\varphi}{x}\frac{\varphi}{xx}, \frac{\varphi}{x}\frac{\varphi}{xx}$$

may be asserted.

The following are examples. Required to develop x in terms of x. Erasing the x's the whole becomes erased, and

$$\frac{\varphi}{x}x = xx$$

Erasing the double x's, the whole becomes doubly erased and $\frac{\varphi}{xx}xx$ is erased and

$$\varphi = \frac{\varphi}{x}x, \frac{\varphi}{xx}xx = xx, xx$$

So that

$$x = xx, xx.$$

Required to eliminate x from $(xx, x; a)$.

$$\frac{\varphi}{x} = 00, 0; a = aa$$

$$\frac{\varphi}{xx} = 00, 00; 00: a = aa$$

$$\therefore \quad \varphi = \frac{\varphi}{x}\frac{\varphi}{xx}, \frac{\varphi}{x}\frac{\varphi}{xx} = aa, aa; aa, aa = aa$$

Required to eliminate x from (xa, a).

$$\frac{\varphi}{x} = 0a, a = aa, a$$

$$\frac{\varphi}{xx} = 00, a; a = aa$$

$$\therefore \quad \varphi = aa, a; aa: aa, a; aa = aa, aa = a$$

Required to develop $(ax; b, xx: ab)$ according to x.

$$\frac{\varphi}{x} = a0; b,\ 00: ab = aa, aa; ab = a, ab = a$$

$$\frac{\varphi}{xx} = a00; b,\ 0: ab = bb, bb; ab = b, ab = b$$

$$\therefore\ \varphi = \frac{\varphi}{x}x,\ \frac{\varphi}{xx}xx = ax; b, xx$$

Required to eliminate M from (SS, M; MM, P).

$$\frac{\varphi}{\mathrm{M}} = \mathrm{SS}, 0; 00, \mathrm{P} = \mathrm{SS}, \mathrm{SS}; \mathrm{SS}, \mathrm{SS} = \mathrm{SS}$$

$$\frac{\varphi}{\mathrm{MM}} = \mathrm{SS}, 00; 0, \mathrm{P} = \mathrm{P}, \mathrm{P}; \mathrm{P}, \mathrm{P} = \mathrm{P}$$

$$\therefore\ \mathrm{SS}, \mathrm{M}; \mathrm{MM}, \mathrm{P} = \mathrm{SS}, \mathrm{P}; \mathrm{SS}, \mathrm{P}$$

which is the syllogistic conclusion.

We may now take an example in categoricals. Given the premises 'There is something beside S's and M's', and 'There is nothing beside M's and P's'; to find the conclusion. As the combined premises state the existence of a non-S non-M and the non-existence of an MP, they are expressed by

$$\mathrm{SM}, \mathrm{SM}; \mathrm{MP}$$

To eliminate M, we have

$$\frac{\varphi}{\mathrm{M}} = \mathrm{S0}, \mathrm{S0}; \mathrm{0P} = \mathrm{SS}, \mathrm{SS}; \mathrm{PP} = \mathrm{S}, \mathrm{PP}$$

$$\frac{\varphi}{\mathrm{MM}} = \mathrm{S}, 00; \mathrm{S}, 00: 00, \mathrm{P} = \text{Erased}$$

$$\therefore\ \frac{\varphi}{\mathrm{M}}\,\frac{\varphi}{\mathrm{MM}}, \frac{\varphi}{\mathrm{M}}\,\frac{\varphi}{\mathrm{MM}} = \mathrm{S}, \mathrm{PP}; 0: \mathrm{S}, \mathrm{PP}; 0 = \mathrm{S}, \mathrm{PP}; \mathrm{S}, \mathrm{PP}: \mathrm{S}, \mathrm{PP}; \mathrm{S}, \mathrm{PP} = \mathrm{S}, \mathrm{PP}$$

The conclusion therefore is that there is something which is not an S but is a P.

Of course, it is not maintained that this notation is convenient; but only that it shows for the first time the possibility of writing both universal and particular propositions with but one copula which serves at the same time as the only sign for compounding terms and which renders special signs for negation, for 'what is' and for 'nothing' unnecessary. It is true, that a 0 has been used, but it has only been used as the sign of an erasure.

The Axioms of Number

MS 380: Winter 1880–81

The following is a complete list of the assumptions of arithmetic. They may be considered as constituting a definition of positive discrete number. From them, every proposition of the theory of numbers may be deduced by formal logic.

I.

Whatever is greater than, is other than.

II.

"Greater than" is a transitive relative; that is, whatever is greater than something greater than, is greater than.

III.

Whatever is greater than a number is a number.

IV.

Number is a system of simple quantity; that is to say, every number is related to every other either as greater or as less.

V.

There is no maximum number: every number is less than a number.

VI.

Unity is the minimum number.

VII.

Number increases by discrete steps. Whatever is greater than a number is greater than some number, without being greater than an intermediate number greater than that.

VIII.

Number is singly infinite: any number can be reached by successive minimum steps. More precisely, if every number smaller than but not smaller than a number smaller than another is in any transitive relation to that other, then every number smaller than another is in the same transitive relation to that other.

IX.

In any counting, every object of the lot counted is counted off by a number.

X.

No number in any counting counts off anything counted off by any other number in the same counting.

XI.

No object in any counting is counted off by any number that counts off any other object in the same counting.

XII.

In any counting, every number counting off an object is less than every number that does not count off an object.

XIII.

The lot counted being finite, there is a final number in every counting of it.

XIV.

The final number of a count counts off an object.

XV.

In any counting, the final number of the count is greater than any other number that counts off an objec*[*t.*]*

Definitions of Addition and Multiplication.

The sum of two numbers, *m* and *n*, whether these be the same or different, is the final number of a count of objects, consisting of two mutually exclusive lots, of countings of which *m* and *n* are the final numbers respectively.

The product of a number n by a number m is the final number of a count of a lot of objects that consists of mutually exclusive lots, the final number of a count of each of which being n, while the final number of the count of these final numbers, considered as a lot of objects, is m.

[On Associative Algebras]

MS 381: Winter 1880–81

To ascertain of what associative algebras having a limited number of linearly independent units it is true that if $pq = p'q$ then $p = p'$ unless $q = 0$ and if $pq = pq'$ then $q = q'$ unless $p = 0$.

I regard $\sqrt{-}$ as being a unit linearly independent of 1. My father in his work on linear associative algebra takes the opposite view but I shall be able to avail myself of certain propositions proved by him; and I commence by restating these propositions with the proofs.

The first is his §40 that in every linear associative algebra there is at least one idempotent or one nilpotent expression. If A be any expression in such an algebra there must be some equation

$$\Sigma_m(a_m \mathrm{A}^m) = 0$$

where the a_m's are scalars. Collect all the terms except the last into one expression BA. Then the equation becomes

$$\mathrm{BA} + a_1\mathrm{A} = (\mathrm{B} + a_1)\mathrm{A} = 0$$

Hence

$$(\mathrm{B} + a_1)\mathrm{A}^m = 0$$

$$(\mathrm{B} + a_1)\mathrm{B} = 0$$

$$\left(-\frac{\mathrm{B}}{a_1}\right)^2 = -\frac{\mathrm{B}}{a_1}$$

so that $-\dfrac{\mathrm{B}}{a_1}$ is idempotent unless $\mathrm{B}^2 = 0$.

The next proposition I propose to borrow from my father is contained in his §41. If $i^2 = i$ and $i\mathrm{A} = \mathrm{B}$ then $i\mathrm{B} = \mathrm{B}$ and $i(\mathrm{A} - \mathrm{B}) = 0$. So that every expression A may be separated into two parts B and $\mathrm{A} - \mathrm{B}$ such that one of these multiplied by i gives itself while the other gives zero. But either of these parts may vanish. So with multiplication into i.

Finally from my father's §53 I adapt the following. Given an expression $i^2 = i$ such that multiplied by or into any other A it gives A. Then

$$\Sigma_m (a_m \mathrm{A}^m) + bi = 0.$$

I now pass to the new propositions. First the algebras we seek are precisely those for which $pq \neq 0$ so long as $p \neq 0$ or $q \neq 0$. For if this be true if $pq = p'q$ then $(p - p')q = 0$. Hence $p - p' = 0$ or $p = p'$ unless $q = 0$. Also if from $pq = p'q$ it follows that $p = p'$ if $q \neq 0$ then from $pq = 0 = 0q$ it follows that $p = 0$ unless $q = 0$.

Hence from the first proposition just proved it follows that in every algebra where this is true there is an idempotent expression i. And since we never have

$$i\mathrm{A} = \mathrm{B}$$
$$i(\mathrm{A} - \mathrm{B}) = 0$$

unless $\mathrm{A} - \mathrm{B} = 0$ it follows that

$$i\mathrm{A} = \mathrm{A}$$

invariably. The unit i is therefore arithmetical unity.

Let A be any other expression. We must have the equation

$$\Sigma_m (a_m \mathrm{A}^m) + b = 0$$

This equation may by the ordinary theory of equations be resolved in the form

$$(\mathrm{A} - x_1)(\mathrm{A} - x_2)(\mathrm{A} - x_3) \ldots \text{etc.} = 0$$

where $x_1\ x_2\ x_3$ etc. are ordinary real or imaginary quantities. If any one of these roots say x_1 be real, the equation is divisible by $\mathrm{A} - x_1$ and denoting the quotient by B we have

$$(\mathrm{A} - x_1)\mathrm{B} = 0$$

so that in that case the algebra would not belong to the class we seek.

If all the roots be imaginary the whole equation may be resolved into a continued product of quadratics, and some one of these must be equal to zero. Hence there must be some quadratic equation of the form

$$A^2 + aA + b = 0$$

Hence

$$\sqrt{b^2 - \frac{1}{4}a^2}\left(A + \frac{1}{2}a\right)$$

is an expression whose square is negative unity.

Notes on Associative Multiple Algebra

MS 382: Winter 1880–81

I.

A multiple algebra is one in which there are units of different qualities capable of being added and multiplied together. Thus, ordinary algebra is a double algebra the units being *unity* and a square root of negative unity. An associative algebra is one in which the multiplication of the units, though not generally commutative, is always associative. A linear algebra is one in which the number of linearly independent expressions is finite; but this restriction seems to be of little moment.

The theory of associative multiple algebra includes that of groups, a group being a special kind of associative algebra. My father in his work entitled *Linear Associative Algebra* has given a method for the discovery of all such systems of algebra, and has given

All	the	single algebras,	2	in	number	
"	"	double "	3	"	"	
"	"	triple "	5	"	"	
"	"	quadruple "	11	"	"	
"	"	quintuple "	70	"	"	

" " sextuple algebras containing any expression which is its own square, 65 in number. But he has reckoned the ordinary square root of negative unity as a scalar, so that each of his algebras will generally include several if the ordinary imaginary be reckoned as a separate unit. The tables given in this work may be applied to finding the groups of the first five orders, and show at once that the list given by Professor Cayley (*Am. Jour. of Math.* Vol. 1, p.

51) is extremely defective. Thus, the latter omits the following derived from Professor Peirce's b_2.

	1	i	j
1	1	i	j
i	i	i	j
j	j	i	j

II.[1]

Multiplication expresses relation. Thus, $i\mathrm{A}$ expresses the quantity which is related to A in the manner signified by i. We may then for any algebra imagine another non-relative algebra, the expressions of which can only be multiplicands or products, never multipliers. Let i be any unit of a multiplying algebra, and let the general expression of the non-relative algebra be $a\mathrm{I} + b\mathrm{J} + c\mathrm{K} + d\mathrm{L} + \text{etc.}$ where a, b, c, d, etc. are scalars. Let the product of this by i be written thus,

$$\begin{aligned}&(\mathrm{A}_1 a + \mathrm{A}_2 b + \mathrm{A}_3 c + \text{etc.})\mathrm{I}\\ &+ (\mathrm{B}_1 a + \mathrm{B}_2 b + \mathrm{B}_3 c + \text{etc.})\mathrm{J}\\ &+ (\mathrm{C}_1 a + \mathrm{C}_2 b + \mathrm{C}_3 c + \text{etc.})\mathrm{K}\\ &+ \text{etc.}\end{aligned}$$

Then, by an enlargement of the multiplying algebra, we may suppose i separated into a sum as follows

$$\begin{aligned}i = {}&\mathrm{A}_1(\mathrm{I}:\mathrm{I}) + \mathrm{A}_2(\mathrm{I}:\mathrm{J}) + \mathrm{A}_3(\mathrm{I}:\mathrm{K}) + \text{etc.}\\ &+ \mathrm{B}_1(\mathrm{J}:\mathrm{I}) + \mathrm{B}_2(\mathrm{J}:\mathrm{J}) + \mathrm{B}_3(\mathrm{J}:\mathrm{K}) + \text{etc.}\\ &+ \mathrm{C}_1(\mathrm{K}:\mathrm{I}) + \mathrm{C}_2(\mathrm{K}:\mathrm{J}) + \mathrm{C}_3(\mathrm{K}:\mathrm{K}) + \text{etc.}\\ &+ \text{etc.}\end{aligned}$$

where $(\mathrm{I}:\mathrm{I})$, $(\mathrm{I}:\mathrm{J})$, etc. are new units such that in general

$$(\mathrm{I}:\mathrm{J})\mathrm{J} = \mathrm{I} \qquad (\mathrm{I}:\mathrm{J})(\mathrm{J}:\mathrm{K}) = (\mathrm{I}:\mathrm{K})$$

and

$$(\mathrm{I}:\mathrm{J})\mathrm{K} = 0 \qquad (\mathrm{I}:\mathrm{J})(\mathrm{K}:\mathrm{L}) = 0$$

In this way it appears that every unit of every algebra may be regarded in at least one way as a linear polynomial in units following this law.

This proposition is equally true whether the ordinary imaginary be or be not regarded as a scalar.

1. The main proposition of this note was presented to the American Academy of Arts and Sciences, May 11, 1875; and is published in the *Proceedings* of that body, p. 392.

In order to reduce ordinary double algebra to this form (not regarding the imaginary as a scalar) we put

$$\begin{aligned} 1 &= X{:}X + Y{:}Y \\ \sqrt{-1} &= X{:}Y - Y{:}X \end{aligned}$$

This representation is closely allied to the geometric representation of imaginaries, for it shows that the multiplication by $\sqrt{-1}$ converts the ordinate into an abscissa of equal value, and the abscissa into an ordinate of the negative of its value; in other words it produces a quadrantal rotation in a positive direction. It may be remarked, that strictness of logic in the development of the calculus, requires that $\sqrt{-1}$ should be regarded as a unit in a continuum limited to two dimensions. This is the way in which Cauchy treats the subject; and this is the only way in which his principles of integration can be established. Thus if we take the variable $z = e^{\theta\sqrt{-1}}$ and make it vary by a continuous change of its argument θ, it is essential to Cauchy's theory that while z changes from unity round to unity again $\sqrt{z}$ changes from unity to negative unity. Representing z by a line OP turning in a plane about O, and $\sqrt{z}$ by another line OQ turning about the same point in the same fixed plane, we easily see that this is so. But if P is free to move over the surface of a sphere, the position of Q which bisects the arc between the variable and the original positions of P, becomes indeterminate when OP is exactly in the inverse of its original position, that is when $z = 1$. If, then, in order to resolve the indeterminacy we let P make an infinitesimal detour around the "dead-point," we see that Q will make a detour infinitely near a great circle so that when P returns to its original position, that of Q is not reversed at all, but is also restored. Thus, Cauchy's whole theory of integration would fall to the ground if $\sqrt{-1}$ represented merely *a* square root of unity, or if the manifoldness of the variability exceeded 2.

The algebra of quaternions will appear in the new form as follows:—

$$\begin{aligned} 1 &= X{:}X + Y{:}Y + Z{:}Z + W{:}W \\ i &= X{:}Y - Y{:}X + Z{:}W - W{:}Z \\ j &= W{:}Y - Y{:}W + X{:}Z - Z{:}X \\ k &= Z{:}Y - Y{:}W + W{:}X - X{:}W \end{aligned}$$

In this point of view, quaternions appears as an algebra of the space of four dimensions, where the motion is so restricted that any rotation in any plane, say of XY, is accompanied by an equal rotation in the conjugate plane of ZW. As this restriction is no greater than requiring every particle to move in an elliptical or hyperbolic space of three dimensions, it would seem to follow that quaternions must be adequate to the representation of the non-Euclidean geometry. Inasmuch, indeed, as it has several times been shown that this geometry may be represented in our ordinary geometry, there is no reason to doubt the proposition.

The system of biquaternions of section IV of Professor Clifford's paper is represented as follows:—[2]

$$
\begin{aligned}
1 &= X:X + Y:Y + Z:Z + W:W + \Xi:\Xi + H:H + S:S + \Omega:\Omega \\
\omega &= X:X + Y:Y + Z:Z + W:W - \Xi:\Xi - H:H - S:S - \Omega:\Omega \\
i &= X:Y - Y:X + Z:W - W:Z + \Xi:H - H:\Xi + S:\Omega - \Omega:S \\
j &= W:Y - Y:W + X:Z - Z:X + \Omega:H - H:\Omega + \Xi:S - S:\Xi \\
k &= Z:Y - Y:W + W:X - X:W + S:H - H:S + \Omega:\Xi - \Xi:\Omega
\end{aligned}
$$

From Grassmann's calculus of extension we may deduce the following algebra[3]:—

Three rectangular vectors

$$
\begin{aligned}
i &= M:A - B:Z + C:Y + X:N \\
j &= M:B - C:X + A:Z + Y:N \\
k &= M:C - A:Y + B:X + Z:N
\end{aligned}
$$

Three rectangular aspects

$$
\begin{aligned}
I &= M:X + A:N \\
J &= M:Y + B:N \\
K &= M:Z + C:N
\end{aligned}
$$

one solid

$$V = M:N$$

unity

$$1 = M:M + A:A + B:B + C:C + X:X + Y:Y + Z:Z + N:N$$

III.

Proof that there are but three associative algebras in which division by finites always yields an unambiguous quotient.

2. See Professor B. Peirce's remarks, *Proceedings of Am. Acad. Arts & Sci.* May 1875.

3. Given by me, *Proceedings of Am. Acad. of Arts and Sciences,* Oct. 10, 1877.

1. If in any algebra division is unambiguous the product of two finites can never be zero. For if $pq = 0$, then $pr = p(r + q)$, and division by p may give r or $r + q$.

2. It is shown in the *Linear Associative Algebra,* §40, that every algebra that contains no expression whose square vanishes contains an idempotent expression, i. It is further shown that every other expression is resolvable into the sum of two such that

$$i\mathrm{A} = \mathrm{A}, \qquad i\mathrm{B} = 0.$$

But in the case supposed, B must vanish, and every expression multiplied by or into i must give itself.

3. Although in the *Linear Associative Algebra,* the scalar coëfficients are permitted to be imaginary, while here they are restricted to being real, yet the reasoning holds by which it is shown, in §53, that A being any expression of the algebra, there is an equation

$$\Sigma_m (a_m \mathrm{A}^m) + b = 0.$$

If this equation has a real root, then, by the reasoning in the text, in the absence of expressions whose squares vanish there must be a second expression, besides i, which is idempotent, and further, §54, in that case there is a product of finites which vanishes. It follows that the above equation, in the class of algebras under consideration, can have only imaginary roots; and consequently two real scalars s and t may be found, such that

$$(\mathrm{A} - s)^2 + t^2 = 0$$

or

$$\left(\frac{\mathrm{A} - s}{t}\right)^2 = -1.$$

In other words, every expression in the algebra can, by a real linear transformation, be converted into a quantity whose square is negative unity, or say into a *versor vector.*

[Unequivocal Division of Finites]

MS 383: Winter 1880–81

We have begun the inquiry into algebras in which the division of finites is unequivocal that is in which

$$\text{If } p \mp p' \qquad q \mp q' \qquad \text{then} \qquad pq \mp p'q \qquad pq \mp pq'$$

We have found that every expression in such an algebra is resolvable in one way and one only into a sum of two parts, the first of which is an ordinary number and the second such that its square is an ordinary negative number. Of course either of these parts may disappear.

An ordinary number we call a scalar
A quantity whose square is negative we call a vector
A quantity whose square is -1 we call a unit vector

That scalar which subtracted from the quantity q yields a vector remainder we call the scalar of q. The vector remainder we call the vector of q. That positive quantity the negative of whose square is the square of the vector v we call the tensor or modulus of v.

Our next step is to prove that the vector part of the product of two vectors is linearly independent of these vectors and of unity. That is, i and j being any two vectors, if

$$ij = s + v$$

s being a scalar and v a vector, we cannot so determine three scalars a, b, c so that

$$v = a + bi + cj$$

This is proved if we prove that ij is not of the form $a + bi + cj$, that is that there is no scalar which subtracted from ij leaves a remainder $bi + cj$. If this be true when i and j are any unit vectors whatever, it is true when these are multiplied by ordinary numbers, and so is true of every pair of vectors. We will, therefore, suppose i and j to be unit vectors.

Now $ij^2 = -i$. If therefore we had

$$\begin{aligned} ij &= a + bi + cj \\ -i = i(j^2) = (ij)j &= aj + bij - c \\ &= -c \qquad + aj \\ &\quad + ba + b^2 i + bcj \end{aligned}$$

Then we should have

$$-i = b^2 i$$

or

$$b^2 = -1$$

which is absurd b being an ordinary number.

The next step is to prove that if the product of any two vectors i and j is $ij = s + v$, s being the scalar and v the vector then $ji = r(s - v)$ where r is an ordinary number or scalar, positive or negative. If this is true when i and j are unit vectors it is true whatever vectors they are. We will therefore suppose them unit vectors.

Let us write

$$\begin{aligned} ji &= s' + v' \\ vv' &= s'' + v'' \end{aligned}$$

Then

$$\begin{aligned} ij \cdot ji = (s + v)(s' + v') = {} & ss' + sv' + s'v + v'' \\ & + s'' \end{aligned}$$

But

$$ij \cdot ji = ij^2 i = -i^2 = 1$$

Hence

$$v'' = 1 - ss' - s'' - sv' - s'v$$

But since v'' is the vector of vv' this cannot be unless v'' vanishes. Hence

$$0 = 1 - ss' - s'' - sv' - s'v$$

Hence

$$sv' + s'v = 0 \qquad \text{or} \qquad v' = -\frac{s'}{s}v$$

That is

$$ji = s' - \frac{s'}{s}v$$

or writing r for $\frac{s'}{s}$

$$ji = r(s - v)$$

Q.E.D.

The next step is to prove that this r is equal to unity, so that if $ij = s + v$ then $ji = s - v$. It is obviously sufficient to prove this when i and j are unit vectors. Now any quantity whatever upon having its scalar subtracted from it becomes a vector, unless it were a scalar at first. What is true then is that from any quantity such a scalar may be subtracted as to leave it either a scalar or a vector, that is to make the square of the remainder a scalar. We do not yet know whether the sum of two vectors is a vector or not. Let us, then, take any such sum as $ai + bj$ and suppose $-x$ to be the scalar which subtracted from it makes the square of the remainder a scalar. We have then $(x + ai + bj)^2$ a scalar.

But

$$\begin{aligned}(x + ai + bj)(x + ai + bj) = {} & x^2 + 2axi + 2bxj + abv \\ & -a^2 \qquad\qquad\qquad\quad - abrv \\ & -b^2 \\ & +abs \\ & +abrs\end{aligned}$$

Let c be the scalar which this equals. Then

$$ab(r - l)v = 2axi + 2bxj + x^2 - a^2 - b^2 + ab(r + l)s - c$$

But this is impossible unless the equation vanishes, because v is the vector of ij. Hence either

$$r - 1 = 0 \qquad \text{or} \qquad v = 0$$

But if $v = 0$, $ij = s$ and multiplying into j we should get

$$-i = sj$$

which is absurd. Hence $r = 1$ and $ji = s - v$. Q.E.D.

Next prove sum of two vectors is a vector, because it cannot be a scalar and $(ai + bj)^2 = -a^2 - b^2 + 2abs$.

I now propose to prove that the number of independent vectors in any algebra fulfilling the required condition is either 0, 1, or 3. I have therefore to prove 1st that the number cannot be 2 and 2nd that it cannot be as great as 4.

That it cannot be 2 is obvious for the vector of ij is independent of i and j. The whole remaining difficulty is to show that it cannot be as great as 4.

Let us suppose, then, that we have two independent vectors i and j whose product is

$$ij = s_1 + v_1$$

Let us immediately substitute for j.

$$j' = s_1 i + j$$

Then we have

$$ij' = v_1 \qquad j'i = -v_1$$

This is a third vector linearly independent of i and j'. Let us suppose that we have a fourth vector k, linearly independent of all the others. Suppose we have

$$\begin{aligned} j'k &= s_2 + v_2 \\ ki &= s_3 + v_3 \end{aligned}$$

Let us then substitute for k

$$k' = s_3 i + s_2 j' + k$$

and we have

$$\begin{aligned} j'k' &= -s_3 v_1 + v_2 \qquad & k'j' &= s_3 v_1 - v_2 \\ k'i &= -s_2 v_1 + v_3 \qquad & ik' &= s_2 v_1 - v_3 \end{aligned}$$

Let us further suppose

$$(ij')k' = s_4 + v_4$$

Then because ij' is a vector

$$k'(ij') = s_4 - v_4$$

But

$$k'j' = -j'k' \qquad k'i = -ik'$$

because both products are vectors. Hence

$$i \cdot j'k' = -i \cdot k'j' = -ik' \cdot j' = +k'i \cdot j' = +k' \cdot ij'$$

Hence

$$s_4 + v_4 = s_4 - v_4$$

or

$$v_4 = 0$$

and the product of two vectors is a scalar. These vectors cannot then be independent or k cannot be independent of i and j. So that a fourth independent vector is impossible and the theorem is proved.

[*Jevons's* Studies in Deductive Logic]

P 198: Nation *32 (31 March 1881): 227*

Studies in Deductive Logic. By W. Stanley Jevons, LL.D. (London and New York: Macmillan & Co. 1880.)

Some forty years ago the two mathematicians, De Morgan and Boole, commenced a reform of formal logic. Their researches were continued by a number of other excellent thinkers (Mr. Jevons among them) in different countries, and the work is now so far advanced that the new logic is beginning to take its place in the curriculum of the universities, while many persons have imagined that some almost magical power of drawing conclusions from premises was to be looked for, and that logic would prove as fertile in new discoveries as mathematics. Concerning such hopes Professor Sylvester says: "It seems to me absurd to suppose that there exists in the science of pure logic anything which bears a resemblance to the infinitely developable and interminable heuristic processes of mathematical science." "To such a remark," replies the author of the book under notice, in his preface, "this volume is perhaps the best possible answer." A more exaggerated pretension never was made. The book is a convenient manual of exercises in elementary logic, tinctured with the author's peculiar views, of which there will be different opinions, but, at any rate, sufficiently sound to be useful in the class-room. But if Professor Jevons were to penetrate only a little ways into the heuretic world of the mathematicians—an excursion quite worth the while of a logician—were to learn what discoveries are there made every month, and what sort of a stamp a proposition must bear to be considered, in that field, as really new, it is to be hoped that he would feel something different from self-satisfaction at recollecting that he had set up anything in this little volume as worthy to be compared with the triumphs of a Sylvester. Logic, inductive and deductive, is an important discipline, probably more important than the higher mathematics, just as the multiplication-table is more important than the calculus; but very, very few are the new problems which have ever been solved by the regular application of any system of logic. That part of logic which can best compete with mathematics in the discovery of new truths is the complicated theory of relative terms. But even there the comparison would be very unequal between what is only a branch of mathematics and the whole body of mathematics together. The solution of problems used to be considered as the glory and touchstone of the mathematician; in our time, the aim is rather at the discovery of methods, and we might perhaps look to the logician to produce a *method* of discovering methods. But the main advantages which we have to expect from logical studies are rather, first, clear disentanglements of reasoning which is felt to be cogent without our precisely knowing wherein the elenchus lies—such, for instance, as the reasoning of elementary geometry; and, second, broad and philosophical *aperçus* covering several sciences, by which we are made to see how the methods used in one science may be made to apply to another. Such are really the chief advantages of the new systems of formal logic, much more than any facilities they afford for drawing difficult conclusions; and it is evident that if logic is to make any useful progress in the future, we must set out with some more or less accurate notion of what sort of advantages we are to seek for.

Width of Mr. Rutherfurd's Rulings

P 204: Nature *24 (21 July 1881): 262*

By the direction of the superintendent of the U. S. Coast and Geodetic Survey, I have long been engaged in the precise measurement of a wave-length of light, in order to obtain a check upon the secular molecular changes of metallic bars used as standards of length. In advance of the publication of my work, it may be useful to say that I have found that the closest-ruled diffraction-plates by Mr. Rutherfurd have a mean width of ruling which varies in different specimens from 68078 to 68082 lines to the decimetre, at 70° F. There is a solar spectral line, well suited for precise observation, whose minimum deviation with one of Mr. Rutherfurd's plates,—in the spectrum of the 2^{nd} order with the closest-ruled plates,—is 45° 01′ 56″ at 70° F. I would propose that this line be adopted as a standard of reference, by such observers of wave-lengths as desire to escape the arduous operation of measuring the mean width of their rulings; for by means of the measures which I shall shortly publish, it will be possible to deduce, from the minimum deviation of this line produced by any given gitter, the mean width of that gitter and consequently the wave-length of any other line whose deviation has been observed with it. The accuracy of this method will greatly exceed that of assuming Ångström's measures to be correct. The wave-length of the line in question (still subject to some corrections which may be considerable) is 5624825. Ångström gives 562336.

Logic; and the Methods of Science

MS 396: Fall–Winter 1881

BOOK I. FORMAL LOGIC.

CHAPTER I. The modus ponens.

The relation between truth and falsity, as we must begin by conceiving them, is thus defined. 1st, Nothing is both true and false: 2nd, Every proposition is either true or false. The former clause of the definition is called the principle of contradiction, the latter the principle of excluded middle.

A somewhat different view is familiar to physicists. Dealing as they do with matters of measurement, they hardly conceive it possible that the absolute truth should ever be reached, and therefore instead of asking whether a proposition is true or false, they ask how great its error is. Just as geometry has its descriptive and its metrical portions, the former considering whether points coincide or not, the latter measuring how far distant from one another they are, just as chemical analysis has its qualitative and quantitative divisions, so logic has first to decide whether a proposition or reasoning be true or false, and, secondly in the latter case, to measure the amount of its falsity.

The two principles of contradiction and excluded middle do not stand at all upon the same plane. All motive for the criticism of our own thoughts or those of others springs from the conviction of a distinction between truth and falsity; and no philosopher has yet been found to maintain that any proposition is in precisely the same sense absolutely true and false at once. Hegel, it is true, professes to do this; but that was because he mistook the relation actually existing between his own thought and that of ordinary men. On the other hand, it has often been held, even when not professed, that concerning some matters there is no truth nor falsity. All philosophical sceptics and all those who believe in limits to human cognition, since they hold that certain propositions are never to be either accepted nor rejected, even if they say those propositions are either true or false can really mean nothing by it. But putting aside, for the present, these topics of higher logic, what concerns us now is that certain rudimentary forms of reasoning, embracing all those that the traditional logic has handed down to us, depend only upon the impossibility of a fact's being both true and false, and remain equally sound arguments, if we suppose that some things are neither true nor false.

It will be advantageous, from the very outset, to introduce certain arithmetical conceptions into logic. These notions are really somewhat extraneous to the subject; but nevertheless they will be found very valuable in giving our thoughts a definite and concrete form. Let us call the state of a proposition as being true or false (or whatever else it may be) its value. We may choose any two numbers at pleasure, to represent the values of truth and falsity, and denote these by the letters v and f respectively, so that the value of every true proposition is said to be v, and that of every false proposition is said to be f, while propositions neither true nor false, if there be any, take other values. Thus two propositions will have the same value if they are either both true or both false, notwithstanding the difference in their meaning, just as two triangles may have the same area and yet be very differently shaped. Accordingly, using the letters of the alphabet to designate different propositions, if we write $x = y$ it will not at all signify that the propositions x and y are equivalent in meaning, but only that both are either true or false at once. The equation $x = \mathrm{v}$ or $x - \mathrm{v} = 0$ will mean that the proposition x is true,—precisely what x written alone would mean. The equation $x = \mathrm{f}$ or $x - \mathrm{f} = 0$ will signify that the proposition x is false: and the same thing would be signified by writing any algebraical expression which becomes equal to v when $x = \mathrm{f}$ and equal to f when $x = \mathrm{v}$. The simplest such expression is $\mathrm{v} + \mathrm{f} - x$. This then represents the negative of the proposition x.

The simplest of all possible inferences is what is commonly called the modus ponens of hypothetical syllogism, though some logicians very properly object to its being called a syllogism at all. It is this:

If x is true, y is true,
But x is true;
Hence, y is true.

Suppose, for example, that being at the theatre, I were to hear a rumour of a house being on fire near where I lived. I should say "If this be true, I had better be going." Suppose then, that on making inquiries, I were to find that the rumour was true: I should hurry home, in accordance with the inference.

Let us represent this in algebraical notation. The hypothetical proposition "If x is true, y is true," is such that if it be true and if x be true, then y is true, but if x be not true or if both x and y be true, then the truth of the hypothetical proposition depends on other circumstances. All this is perfectly represented by the equation,

$$(x - \mathrm{v})\,\mathrm{A} = (y - \mathrm{v}) : \mathrm{B},$$

where A and B are quantities whose values are not determined by those of x and y alone, and which may vanish but cannot become infinite. For if $x = \mathrm{v}$ the equation cannot hold unless $y = \mathrm{v}$. It may not hold even then, if $\mathrm{B} = 0$. If x does not equal v, the equation may or may not be true, according to the values of y, A, and B. From this equation together with $x = \mathrm{v}$ we at once deduce $y = \mathrm{v}$, thus performing the inference of the modus ponens by an arithmetical process. This may be surprising at first sight, but it ceases to be so when we reflect that all that we have done is to find among the innumerable relations of which numbers are susceptible, one which is the analogue of that very simple relation subsisting between x and y by virtue of the hypothetical proposition.

CHAPTER 2. Barbara.

If to the equation

$$(x - \mathrm{v})\,\mathrm{A} = (y - \mathrm{v}) : \mathrm{B}$$

meaning that "If x is true, y is true," we join the similar equation

$$(y - \mathrm{v})\,\mathrm{A}' = (z - \mathrm{v}) : \mathrm{B}'$$

meaning that "If y is true, z is true," we can eliminate y and so get

$$(x - \mathrm{v})\,\mathrm{AA}' = (z - \mathrm{v}) : \mathrm{BB}'$$

which signifies that "If x is true, z is true." This form of inference is essentially that which the logicians term Barbara.

When we say that a proposition cannot be both true and false, we mean not in any one case, for in one case it may, of course, be true and in another false. By one case, here, we mean at any one time, of any one thing, in any one sense, etc. Let a designate any case of a given definite series of cases. It makes no difference whether this series is a class of things of which the proposition might be true, or a range of possibilities or what. We may be considering the class of man with reference to their possessing some character, or the cholera as being prevalent at some or all of certain times, or a social phenomenon as occurring in a certain range of possible social constitutions, or anything of the sort. Whatever be the nature of the series of cases, we may write x_a to mean that in the case a, x is true.

Methods of Reasoning

MS 397: Fall–Winter 1881

FIRST METHOD. The simple consequence.

By a p r o p o s i t i o n, is meant, in logic, anything which can be held for true, or which can be supposed to be so held. Thus, "all men are free and equal," is a proposition, and it is at the same time composed of two propositions, that all men are free and that all men are equal. It is plain that any compound of propositions forms a proposition.

R e a s o n i n g is accepting a proposition as true, while recognizing some other proposition as the reason for it. By recognizing a proposition as the reason for another, is meant recognizing that the belief in the former causes the belief in the latter in a way in which true propositions will not, (at least usually,) produce belief in such as are false.

A proposition accepted on account of a reason is called a c o n c l u s i o n, and is said to be i n f e r r e d from that reason. The reason, itself a proposition, is generally conceived as composed of several propositions, and these are termed the p r e m i s e s of the inference.

The reasoner always conceives, however vaguely, that there is some general rule by which he passes from premises to conclusion; otherwise he would say that the premises were followed by the conclusion, but not that they caused or determined the latter. If this rule be conceived to be one which will never lead from true premises to a false conclusion, the reasoning is said to be n e c e s s a r y; but if the rule is conceived to be one which will only lead to the truth on the whole, or in the long run, while it may occasionally lead to error, the reasoning is said to be p r o b a b l e. For the present, we shall confine our attention to necessary inferences.

The rule connecting the premises with the conclusion of a necessary inference may always be stated in this form: that if propositions of a certain description are true, then a proposition related to them in a certain way will also always be true. This rule, so stated, is called the leading principle of the inference.

An inference whose leading principle is true is said to be a valid inference; its reason is called sound, and its conclusion is said to really follow from the premises.

All truth is ascertained by observation: but a proposition the truth of which can easily be ascertained by observing the parts of a diagram, or something of the sort, which we can construct at pleasure, is said to be *evident.* Thus, it is evident that a triangular pyramid of cannon-balls having two balls on a side consists of four balls in all. A leading principle that is evident is called a logical principle, the inference to which it belongs is termed complete, and its conclusion is said to follow logically from the premises. For example, suppose a silver dollar fails to ring when thrown upon a table: the necessary conclusion is that it is counterfeit. This is a valid, but not a complete inference, for its leading principle is that every dollar, not counterfeit, rings when thrown upon the table; which is true, but not evident. To complete the reasoning, we add this leading principle as a premise, whereupon we obtain the following inference: "Every good dollar rings, etc.: this dollar does not ring, etc.: hence, this dollar is not good." The leading principle of this is that nothing possesses any character which is never found in conjunction with another character which it is known to possess: and this is evidently true.

The rudest kind of reasoning consists in drawing a conclusion from a single premise. The premise, in this case, is called the antecedent, the conclusion the consequent, while the inference itself is called a *consequence,* which name is likewise given to the leading principle. I reason in this way when I hear a person make a statement, and jump at once to the conclusion that what he says is true, without stopping to consider whether he is a veracious witness or not; or when I see a magician put a pocket-handkerchief in a box, and am unreflectingly led to believe it remains in the box five minutes later, without asking myself whether this is what generally happens in magicians' tricks or not. Such reasoning, however sound, is plainly incomplete, and represents an entirely unreflecting and uncritical state of mind. There are, however, simple consequences that are logically complete: these are usually termed immediate inferences. Such, for example, is the inference from "There is a number smaller than any other," to "There is but one number than which no other is smaller." The principle that having once accepted a proposition we are right in adhering to it, until the matter is reexamined, may be represented by the formula, "A, therefore A." This is called the *identical inference.*

It may here be remarked that it will be found particularly convenient, in discussing methods of reasoning, to denote propositions by letters of the alphabet; so that "If A, then B," will be a compendious way of saying that if a certain proposition, A, is true, then a certain proposition, B, is true.

SECOND METHOD. The *modus ponens.*

The moment that a person who has made the incomplete inference, "A, therefore B," is led to reflect upon or criticise his procedure in the slightest degree, he will recognize the leading principle "If A, then B," as a premise and thus reform his inference, as follows:

If A, then B,
But A:
Hence, B.

This form of inference is called the modus ponens.

A proposition consisting of two clauses connected by an "if," so that one proposition is said to follow from the other, called in grammar as it formerly was in logic, a conditional sentence, has in our times been more commonly termed hypothetical by the logicians. That which, in grammar, is called a simple sentence, is in logic termed a categorical proposition. Such a proposition is put by logicians into the standard form, "S is P," where S and P represent two names. Thus, the dog runs is stated in logic in the form, the dog is running. This often violates the usage of language, but logic has its own forms of expression, and the important point in this case is to show that the simple sentence is equivalent to saying that objects to which one name applies another name applies. Propositions asserting that if A is B is, whenever A is B is, wherever A is B is, and whatever A is B is, are all of the same general nature, and any fact which can be stated in the form of a hypothetical proposition can also be stated in the form of a categorical proposition. Hypothetical propositions usually assert nothing with regard to the actual state of things and relate only to what is possible. This range of possibility sometimes includes all that could without self-contradiction be supposed to be true, sometimes all that is in accord with physical laws, and generally all that in some supposable state of knowledge would not be known to be false. Take, for example, the proposition "If this patient has yellow fever his temperature will shortly rise." Here, we are supposed not to know whether the patient has yellow fever or not, but we are supposed to know the different courses that the disease takes in patients like the one in hand, and the hypothetical proposition is equivalent to this categorical one: Every yellow fever patient like the one in hand shortly experiences a rise in temperature. To take another example, suppose If I am well tomorrow, I shall go fishing. We do not know whether I shall be well or not; we further do not know but I shall be well and in good humor, we do not know but I shall be well and in bad humor, etc. But the statement is that every such case is a case in which I should go fishing. In many cases the state of ignorance supposed is fictitious, which is indicated in grammar by the use of past tenses. "If the witness were telling the truth, he would not blush." We here go back to an imaginary state of knowledge in which we do not know whether the witness blushes or tells the truth but among all the possibilities that open, all those in which he tells the truth are cases in which he does not blush. In like manner, the meaning of every hypothetical proposition can be precisely expressed in categorical form, the only difficulty being to settle the exact meaning which the hypothetical proposition, usually a vague mode of speech, bears. The following are offered as exercises.

If the population of the United States increases, from 1880 to 1890, as fast as that of the state of Ohio did from 1870 to 1880, and if the population of France increases at the same rate as it did from 1872 to 1881, then at the end of the century, the population of the United States will be triple that of France.

At the end of a century, the population of the United States will be as dense as that of Europe is now, unless some catastrophe should prevent.

If an inhabitant of the planet Jupiter can climb a mountain on that planet as easily as a man can climb a proportionally high mountain on the earth, the former must have 30 times the strength of the latter as compared with his mass: and if, further, the inhabitant of Jupiter is as much larger than a man as Jupiter is than the earth, and is no more likely to break his bones by muscular contraction than a man is, the strength of the material of his bones must be 300 times as great as that of ours per unit of mass.

If Juan Perez de Manchena had not been the confessor of Queen Isabella, Columbus would never have discovered America.

If Newton had not discovered the law of gravitation, some other man would.

If Sir Philip Francis wrote the letters of Junius, he was a singular instance of a vain and ambitious man never showing his real ability.

Every hypothetical proposition describes a state of things in its antecedent, and then in its consequent assigns another description thereto; and almost every categorical proposition, though it be not equivalent to a hypothetical one, amounts to the assertion that something it describes may be otherwise described. These two descriptions may be expressible in general terms, and the latter, at least, usually is so; but it is an important theorem of logic that no proposition whatever can be completely and fully expressed in general terms alone. For let us consider what is meant, precisely, by a general term or general description. Such a description is a single word or phrase or series of phrases, or something equivalent to that,—say, a signal from a code. A description is thus a conventional sign of some kind. It may have some resemblance to the thing signified, like an onomatopoeic word, and that resemblance may have had something to do with its selection as a sign. Still, once selected, it has become conventional. By this, I do not mean that it was established by any treaty, but only that it is significant of its object by virtue of a mental habit associating together the word and thing. A habit is a general rule operative within the organism, and hence a conventional sign is naturally general. Besides, an idea has no individual identity,—two ideas exactly alike are the same idea. Accordingly, if a conventional sign is not general, it is not purely conventional. Now an object can always be imagined, or at least supposed, which shall reunite any two descriptions that are not absolutely contradictory, so that a proposition that merely says that among supposable objects there is one of a given description to which another given description is applicable might as well be left unsaid. But though descriptions apply to any supposable objects of the sort described, propositions are usually restricted to objects now existing, or to those which have existed or will exist, or to such as can exist in conformity with physical laws and other conditions, or to those that are met with in some realm of fiction, etc. This restriction, an essential part of the meaning of the proposition, cannot, for the reason just given, be expressed by any general description. It is usually indicated by a reference to an avenue of sense or source of information by which we are placed in real relation with the world of objects to which the proposition refers. Very often, it is only the tone of the discourse which gives us to understand whether what is said is to be taken as history, physical possibility, or fiction. In other cases, phrases such as "the fact is," "according to the nature of things," and the like are employed, and these no doubt partake of the nature of conventional signs. Yet so far as they refer us to some living experience or to something with which we have been made familiar by its action on us and ours on it, they signify their objects, not by virtue of habitual association merely, but by the force of a real causal connection. In short, though all words are to some extent conventional, yet some of them do not possess that generality which is the distinguishing mark of purely conventional signs. "Here," "now," "this," are rather like finger-pointings which forcibly direct the mind to the object denoted. They resemble the letters on a geometrical diagram in merely serving to bring the mind back to the same identical object which has previously come before it; and differ from conventional signs in being made to signify their objects by being actually attached to them. When I say I mean my discourse to refer to the real world, the word "real" does not describe what kind of a world it is: it only serves to bring the mind of my hearer back to that world which he knows so well by sight, hearing, and touch, and of which these sensations are themselves indices of the same kind. Such a purely demonstrative sign is a necessary appendage to a proposition, to show what world of objects, or as the logicians say, what "universe of discourse" it has in view.

Inferences like the following are to be referred to the modus ponens:

Whenever A is B is,	Wherever A is B is,	Whatever A is B is,
Now A is,	Here is A,	This is A,
Hence, now B is.	Hence, here is B.	Hence, this is B.

Forms like these are the simplest examples of logical inferential formulae. They represent, in a diagrammatical way, the relations of the parts of an inference. The premises of every logical inference state that certain relations subsist between certain objects, and to draw the conclusion, we have to contemplate these relations and to see that where these relations subsist something else is true. It is indispensible that an act of observation should be performed. For instance, in reasoning by the *modus ponens,* it is necessary actually to notice that the proposition stated in the second premise is the same as the antecedent of the first premise. It will not, therefore, be sufficient to state the relations; it is necessary actually to exhibit them, or to represent them by signs the parts of which shall have analogous relationships. This principle, of fundamental importance in logic, will be made more clear when we come to more difficult forms of reasoning. It explains why diagrammatic representations of inferences are to be preferred to general descriptions.

It will be seen that we have to do in logic with three kinds of signs, 1^{st}, diagrams, which stand for their objects by virtue of being like them, 2^{nd}, indices, which stand for their objects by virtue of being connected with them, and 3^{rd}, descriptions, which stand for their objects in virtue of being mentally associated with them.

THIRD METHOD. Barbara.

A slightly more advanced method of reasoning is shown in the following schema:

If A, then B;
If B, then C:
Hence, if A, then C.

This mode of reasoning (stated in categorical form) is known to logicians as the syllogism in Barbara. Several of the rules current in logic are only forms of the statement that Barbara is valid reasoning. Such is the maxim "Nota notae est nota rei ipsius," the mark of a mark is a mark of the thing itself, where the mark of a mark must be interpreted as a description applicable to everything to which another description applies. Such too is the dictum de omni, "whatever is asserted of the whole of a class is asserted of every part thereof." Another way is to say with De Morgan that the relation of antecedent to consequent is a transitive one, that is, if A is in this relation to B, and B to C, then A is in this relation to C.

A description which applies to everything to which another description applies is said to be wider than the latter and to contain the latter under it. The validity of Barbara may therefore be stated in the form, Whatever contains something that contains a third itself contains that third. This conception is used in the technical names of the parts of the syllogism. Stating Barbara in the categorical form, we have

Every M is P,
Every S is M,
Hence, every S is P.

The description M, which occurs in both premises but is eliminated from the conclusion, is called the middle term, S and P the extremes. The description S, which is contained under the middle term, is called the minor extreme; the description P, under which the middle term is contained, is called the major extreme; and the same adjectives are applied to the premises in which S and P are respectively found.

The major premise of Barbara is a Rule, the minor premise is the statement that a certain Case comes under that rule, while the conclusion gives the Result of the rule in that case. Barbara, thus consists in the direct application of a rule; and every proceeding of that sort is of the same nature as the inference in Barbara. When, in consequence of receiving a certain sensation, we act in a certain way by force of a habit, which is a rule operative within the organism, our action cannot be said to be an inference, but it conforms to the formula of Barbara. So when an effect follows upon its cause by virtue of a law of nature, the operation of causation takes place in Barbara. These instances give us an inkling that logic is far more than an art of reasoning: its forms have psychological and metaphysical importance.

FOURTH METHOD. Indirect inference.

To say that from two premises, P and Q, a certain conclusion R follows, is the same as to say that from either of the premises, say P, we may conclude "If Q then R," that is to say, P being once granted, R follows from Q. Applying this transformation to the syllogism in Barbara,

S is M, M is P: therefore, S is P,

we obtain two forms of immediate inference, viz.:—

M is P:
Hence, If S is M, S is P,

and

S is M:
Hence, If M is P, S is P.

Now the following is a regular inference in Barbara:

If S is M, S is P;
If S is P, then X.
Hence, If S is M, then X.

But the minor premise here (the first one) is the conclusion of the first of our two immediate inferences; consequently, the following inference is valid:

M is P,
If S is P, then X;
Hence, If S is M, then X.

This form of inference is called the minor indirect syllogism. The following is a concrete example:

All men are mortal,
If Enoch and Elijah were mortal, the Bible errs;
Hence, if Enoch and Elijah were men the Bible errs.

Again, we may start with this syllogism in Barbara:

If M is P, then S is P;
If S is P, then X;
Hence, If M is P, then X.

The first premise of this is the conclusion of the second immediate inference given above. Substituting for it the premise of that inference, we have the following form, which is called the major indirect syllogism:

S is M,
If S is P, then X:
Hence, If M is P, then X.

Example:

All patriarchs are men,
If all patriarchs are mortal, the Bible errs;
Hence, if all men are mortal the Bible errs.

We have already seen that the assertion that a thing exists is not of the nature of a general description. It may now be added that the assertion that any sort of thing does not exist is of that nature. In point of fact, every general description amounts to the statement that some sort of thing does not exist. To say that all crows are black is the same as to say that non-black crows do not exist. Propositions are thus of two essentially different kinds; those which assert that one thing follows from another, being thus equivalent to hypotheticals, and these amount to denying the existence of something; and secondly those which affirm the existence of something and thus amount to denying that one thing follows from another. "The cockatrice has a serpentine tail," means that if anything is a cockatrice it has a serpentine tail, and amounts to denying the existence of a cockatrice without such a tail. It may be noticed by the way that though the categorical is thus put into hypothetical form, yet the relation of subject to predicate is not shown to be nothing more than what is involved in the relation of antecedent to consequent, but that the former relation is a special form of the latter. Propositions of the first class are termed u n i v e r s a l, those of the second p a r t i c u l a r. A compound proposition may be partly of one kind and partly of the other. Thus, if I say "all the mammals of Australia carry their young in pouches," I affirm that there are mammals in Australia, and at the same time state that if there are any they carry their young in pouches. There are other propositions, which in their main import belong distinctly to one of the two classes, and yet imply the truth of a proposition of the other class. Take this example: "Some men would not be terrified by any ghost." This is distinctly the affirmation of the existence of something; yet it implies that if there be a ghost, there will be men whom it cannot terrify. Again, "If all men are sinners, some men are fools," is distinctly hypothetical, yet it also implies the existence of some men.

To deny a proposition, then, is to apply to it a general description, that of being false. If now in the second of the two forms of immediate inference given above we replace the P by the special description "false," and put the premise into hypothetical form, we get

If S, then M.
Hence, if M is false, S is false.

This is the principle of the mode of inference called the reductio ad absurdum, by which we show that if a proposition, S, were true, another proposition, M, would necessarily be true, but that M is not true, so that S cannot be true. This inference takes the name of the modus tollens when put in the form

If S then M,
M is false,
Hence, S is false.

If in the formula given for the minor indirect syllogism, we put "false" for X, we obtain the following:

M is P,
That S is P is false,
Hence, that S is M is false.

This mood is called Baroko. Making the same substitution in the major indirect syllogism, we get

S is M,
That S is P is false,
Hence, that M is P is false.

This mood is called Bokardo. Baroko and Bokardo are simple cases of the reductio ad absurdum. Baroko is useful for establishing distinctions between things, as when we reason,

All fishes are propagated from eggs,
But it is not true that whales are propagated from eggs,
Therefore, it is not true that whales are fishes.

Bokardo is useful for establishing exceptions to rules, as thus:

The dingo is a mammal of Australia,
But it is not true that dingos carry their young in pouches,
Therefore, it is not true that all the mammals of Australia carry their young in pouches.

There are many other modes of syllogism derivable in similar ways, but they are of no great importance, and will be passed by for the moment.

FIFTH METHOD. The principle of excluded middle.

We have thus far considered but a single property of negation, namely that denying a proposition is applying a general to it. The formula of the modus ponens gives us by transformation this form of immediate inference:

S is true,
Hence, if from S P follows, then P is true.

Substitute for P the special description "false" and we have the immediate inference

S is true,
Hence, the denial of S is false.

The statement of the validity of this general inference is termed the principle of contradiction. The converse of this principle, namely that from the denial of the denial of a proposition the truth of that proposition follows is termed the principle of excluded middle. This principle constitutes a distinct axiom concerning negation. It is not involved in any of the traditional forms of inference, but it is involved in certain modes introduced into logic by De Morgan. I do not mean that reasoning involving this principle had never been used before, but only that such forms had escaped recognition as distinct from others, and indeed De Morgan himself does not see that they are essentially different from the rest. The inference from "If A is true, B is false," to "If B is true, A is false," depends merely on the principle of contradiction, that is on "false" being a general predicate. For "false" put any predicate, C, and it will be equally true that if it be admitted that A being granted, C follows from B, we must also admit that B being granted, C follows from A. On the other hand, the inference from "If A is false, B is true," to "If B is false, A is true," is of an entirely different nature, and depends on the principle of excluded middle.

Note on the Mouse-Trap Problem

MS 398: Fall 1881–Spring 1882

In the first number of the *Quarterly Journal* (April 1857), Professor Cayley proposes a problem which intersects with the following. Take a pack of n cards numbered in order from ace at the top to n at the bottom. Count the cards, taking them singly from the top and putting them at the bottom as you count them. Put the a^{th} on the table. Begin counting again and put the $(a+b)^{\text{th}}$ on the table; then the $(a+2b)^{\text{th}}$ and so on in arithmetical progression. In what order will the cards emerge?

The answer is this. Let $a + bn = r$. Then they will come out in the r^{th} arrangement of the first n numbers. The r^{th} arrangement of the first n numbers is this. The first number is the residue of r to the modulus n (n is considered as a residue and *zero* not) the other numbers are the residues to the same modulus of the first $n - 1$ numbers in their r^{th} arrangement each increased by r. For example, we wish to know in what order 6 cards will come out if $a = 2$, $b = 3$. In this case $r = 20$. Then we make the following calculation (calculating in vertical lines)

$20 \equiv 1 \pmod{1}$						1
$20 \equiv 2 \pmod{2}$					2	1
$20 \equiv 2 \pmod{3}$				2	1	3
$20 \equiv 4 \pmod{4}$			4	2	1	3
$20 \equiv 5 \pmod{5}$		5	4	2	1	3
$20 \equiv 2 \pmod{6}$	2	1	6	4	3	5

The number of arrangements is evidently equal to the least common multiple of the first n numbers.

Note on 0^0

MS 399: Fall 1881–Spring 1882

Dr. Franklin has already pointed out that 0^0 is not indeterminate, except in special cases. But the matter may be considered from a somewhat different point of view from his. We know that unless x is of an infinite order, $\log x$ is of the zero order: hence, if $x = 0$ and $y = 0$, we have $\log x^y = y \log x = 0$ unless either x be of an infinite order or y be of a zero order. In the former case, the form is not properly 0^0, for $x^y = 0^{\infty 0} = 0^n$, the value of which depends upon the relation of the order of y to the order of the order of x. In the latter case, where y is of a zero order, the form is again not properly 0^0, but is $x^y = 0^{0^0}$. Properly speaking, then, $0^0 = 1$, $0^{0^0} = 0$, $0^{0^{0^0}} = 1$, etc. From this point of view, it appears that the only quantity that is truly of the zero order, is unity. But what shall we say to such quantities as $\log 0$, which are undoubtedly of a zero order without being unity? The answer to this paradox will be found in asking what is the order of the order of $\log 0$? Put $\log i = i^j$. Then, $\log \log i = j \log i$. We see that since $\log i$ is of the zero order, the order of j (i.e. the order of the order of i) is the same as the order of $\log \log i$. Now, $\log \log i$, being of a 0 order, $\log \log i$ is of a 0×0 order. Hence, $\log i$ is of the 0 order, and the order of that 0 is itself 0, and the order of the last 0 is again 0, and so on ad infinitum. Thus,

$$\log 0 = 0^{0^{0^{0^{0^{\text{ad inf.}}}}}}$$

This leads us to consider the strange function

$$x' = x^{x^{x^{x \text{ ad inf.}}}}$$

We have, in the first place, $x^{x'} = x'$, so that the function is the inverse of the function $x'^{-x'} = x$. Writing E for the Neperian base, C for the ratio of the circumference of a circle to its diameter, ɨ for the square root of minus 1, r and t, r' and t', for the moduli and arguments of x and x' respectively, the ordinary formulae show that

$$r = r'^{\,-r'\cdot\cos t'}\,.\,\mathrm{E}^{r't'\cdot\sin t'}$$

$$t = -r't'\cos t' - r'\log r'\sin t'.$$

When $r' = 0$, then unless t' is infinite, $r = 1$ and $t = 0$ (not $= 2\mathrm{EC}$). Hence, for $x = 1$ one value of x' is 0,—a singular result. If $r' = 0$ and $t' = \infty$ both r and t become indeterminate, yet are connected together. In fact, since when $t' = \infty$, $\sin t'$ and $\cos t'$ have all values from 0 to 1, it follows that in the case considered, of the two numbers t and r, one has all values from 1 to an indeterminate number while the other has all values from 1 to the reciprocal of that indeterminate number. It follows that if x is of the form $y \cdot \cos(1:y) + y \cdot \sin(1:y) \cdot i$ (the locus of a hyperbolic spiral in the plane of imaginary quantity) then one value of x' is zero. As the different zeros in the neighborhood of any one point cannot belong to the same sheet of the Riemann's surface, it follows that the sheets being infinitely numerous pack together to form a solid, and that a line of zeros runs through this in a helix whose projection is a hyperbolic spiral. Next, except for special values of t' when $r' = \infty$, r is of an infinite order, and t is infinite. When however $\cos t'$ is a zero of an order equal to that of r', r is generally zero while t is infinite. Thus, when $x = 0$ one value of x' is infinity.

For x' real there is but a single value of x for every value of x'. Starting with $x' = -\infty$ we have $x = \infty$. As x' increases x diminishes, until $x' = -1:\mathrm{E}$ when $x = 0.6922$. From this point, x increases with x' until $x' = +1:\mathrm{E}$ when $x = 1.4444$. Finally, from this point, x decreases as x' increases and vanishes as x' becomes infinite. We thus see that when x is a positive quantity less than 0.6922, x' has but one real value, which is positive. When the value of x lies between 0.6922 and 1, x' has one positive and two negative values. When x lies between 1 and 1.4444, x' has two positive and one negative values. And when x exceeds 1.4444, x' has but one real value, which is negative.

[On Propositions and Syllogisms of Differing Order]

MS 400: Fall 1881–Spring 1882

Propositions of the 0 order.

(The term 0 order explained below.)

Extensive	$1 -\!\!< \breve{A} \dagger B$	$1 -\!\!< \breve{A} B$
Intensive	$1 -\!\!< \breve{\alpha} \dagger \beta$	$1 -\!\!< \breve{\alpha} \beta$

Syllogisms 0-0 order.

$$\left.\begin{array}{l} 1 -\!\!< \breve{A} \dagger B \\ 1 -\!\!< \breve{\bar{\bar{B}}} \dagger C \end{array}\right\} \text{Conclusion } 1 -\!\!< \breve{A} \dagger C$$

$$\left.\begin{array}{l} 1 -\!\!< \breve{A} \dagger B \\ 1 -\!\!< \breve{\bar{\bar{B}}} C \end{array}\right\}$$

First conclusion $1 -\!\!< \breve{A} C$. Second conclusion $1 -\!\!< \breve{A} \dagger n C$ Both conclusions follow from premises and neither of them implies the other. The second conclusion is not of the system of propositions considered

$$\left.\begin{array}{l} 1 -\!\!< \breve{A} B \\ 1 -\!\!< \breve{\bar{\bar{B}}} C \end{array}\right\} \text{Conclusion } 1 -\!\!< \breve{A} n C.$$

In words the second syllogism is

Every $\bar{B}$ is A
Some $\bar{B}$ is C

∴ Some C is A and whatever is identical with every C is A

The first of these propositions might (were it not for the premises) be false while the second was true. Namely in case no A existed and in case there were more than one C.

By the order of a proposition I mean the number of times it contains the relative p or 'possessor of' (in the sense of an object possessing a quality). Propositions of even orders are either Extensive or Intensive. Those of odd orders have not this distinction.

Propositions of 1^{st} Order.

$$1 \prec \breve{A} \dagger p \dagger \beta \qquad\qquad 1 \prec \breve{A} \dagger \bar{p} \dagger \beta$$
$$1 \prec \breve{A}(p \dagger \beta) \quad 1 \prec (\breve{A} \dagger p)\beta \qquad\qquad 1 \prec \breve{A}(\bar{p} \dagger \beta) \quad 1 \prec (\breve{A} \dagger \bar{p})\beta$$
$$1 \prec \breve{A}p \dagger \beta \quad 1 \prec \breve{A} \dagger p\beta \qquad\qquad 1 \prec \breve{A}\bar{p} \dagger \beta \quad 1 \prec \breve{A} \dagger \bar{p}\beta$$
$$1 \prec \breve{A}p\beta \qquad\qquad 1 \prec \breve{A}\bar{p}\beta$$

Syllogisms of the 0-1 Order
i.e. those having premises of the 0 and the 1^{st} Order.

Suppose one of the premises is $\beta \prec \gamma$ or $1 \prec \breve{\bar{\beta}} \dagger \gamma$. Then γ may be substituted for β in the other premise provided β occurs without a dash over it. If in the premise of the first order β is connected by relative addition; that is if that premise is one of the following

$$1 \prec \breve{A} \dagger p \dagger \beta \qquad\qquad 1 \prec \breve{A} \dagger \bar{p} \dagger \beta$$
$$1 \prec \breve{A}(p \dagger \beta) \qquad\qquad 1 \prec \breve{A}(\bar{p} \dagger \beta)$$
$$1 \prec \breve{A}p \dagger \beta \qquad\qquad 1 \prec \breve{A}\bar{p} \dagger \beta$$

there is no second conclusion. But when β is connected by multiplication there is a second conclusion. Thus consider the premises $1 \prec \breve{\bar{\beta}} \dagger \gamma$ and $1 \prec \breve{A} \dagger p\beta$. We may write

$$1 \prec (\breve{A} \dagger p\beta)(\breve{\bar{\beta}} \dagger \gamma) \prec \breve{A} \dagger p\beta(\breve{\bar{\beta}} \dagger \gamma) \prec \breve{A} \dagger p(\beta\breve{\bar{\beta}} \dagger \gamma)$$
$$\prec \breve{A} \dagger p(n \dagger \gamma) \prec \breve{A} \dagger p\gamma$$

which gives the first conclusion; or we may write

$$1 \prec (\breve{A} \dagger p\beta)(\breve{\bar{\beta}} \dagger \gamma) \prec \breve{A} \dagger p\beta(\breve{\bar{\beta}} \dagger \gamma) \prec \breve{A} \dagger p\beta\breve{\bar{\beta}} \dagger \gamma$$
$$\prec \breve{A} \dagger pn \dagger \gamma$$

which is the second conclusion. The second conclusion can always be found by substituting $pn \dagger \gamma$ for $p\gamma$ in the first (or $\overline{pn} \dagger \gamma$ for $\bar{p}\gamma$).

If the premise of the zero order is of the form $1 \prec \breve{\bar{\beta}}\gamma$ and in that of the 1^{st} order β is connected by addition there are two conclusions. Thus

$$1 \prec (\breve{A} \dagger p \dagger \beta)(\breve{\bar{\beta}}\gamma) \prec \breve{A} \dagger p \dagger \beta\breve{\bar{\beta}}\gamma \prec \breve{A} \dagger p \dagger n\gamma$$

and

$$1 \prec (\breve{\mathrm{A}} \dagger p \dagger \beta)\breve{\bar{\beta}}\gamma \prec (\breve{\mathrm{A}} \dagger p \dagger \beta\breve{\bar{\beta}})\gamma \prec (\breve{\mathrm{A}} \dagger p)\gamma$$
$$1 \prec \breve{\mathrm{A}}(p \dagger \beta)\breve{\bar{\beta}}\gamma \prec \breve{\mathrm{A}}(p \dagger n\gamma)$$

and

$$\prec \breve{\mathrm{A}}p\gamma$$
$$1 \prec (\breve{\mathrm{A}}p \dagger \beta)\breve{\bar{\beta}}\gamma \prec \breve{\mathrm{A}}p \dagger n\gamma$$

and

$$\breve{\mathrm{A}}p\gamma$$

Observe in reference to the conclusions of the last sets of premises that

$$\breve{\mathrm{A}}(p \dagger n\gamma) \prec \breve{\mathrm{A}}p \dagger n\gamma$$

but the other conclusion is the same in the two cases.

But when, the premise of the zero order being $1 \prec \breve{\bar{\beta}}\gamma$, the premise of the first order has β connected by multiplication, there is only one conclusion which involves n. In these cases $n\gamma$ is substituted for β in the latter premise to form the conclusion.

Syllogisms of the 1-1 order.

Let the first premise be in $\breve{\mathrm{A}}$ and β, the second in $\breve{\mathrm{C}}$ and $\bar{\beta}$. Then if in both premises β is connected by addition, there will always be a single conclusion which will be of the 2nd order. Thus

$$1 \prec \breve{\mathrm{A}} \dagger p \dagger \beta \quad 1 \prec \breve{\mathrm{C}} \dagger p \dagger \bar{\beta}$$
$$1 \prec (\breve{\mathrm{A}} \dagger p \dagger \beta)(\breve{\bar{\beta}} \dagger \breve{p} \dagger \mathrm{C}) \prec \breve{\mathrm{A}} \dagger p \dagger \breve{p} \dagger \mathrm{C}$$
$$1 \prec \breve{\mathrm{A}} \dagger p \dagger \beta \quad 1 \prec \breve{\mathrm{C}}(p \dagger \bar{\beta})$$
$$1 \prec (\breve{\mathrm{A}} \dagger p \dagger \beta)(\breve{\bar{\beta}} \dagger \breve{p})\mathrm{C} \prec (\breve{\mathrm{A}} \dagger p \dagger \breve{p})\mathrm{C}$$

etc.

If in one premise β is connected by addition and in the other by multiplication, there will always be two conclusions of the second order if p occurs in both premises either with or without a dash, or two conclusions of the zero order if p occurs in one premise with and in the other without a dash. Thus

$$1 \prec \breve{\mathrm{A}}(p \dagger \beta) \qquad 1 \prec (\breve{\mathrm{C}} \dagger \bar{p})\bar{\beta}$$

$$1 \prec \breve{\mathrm{A}}(p \dagger \beta)\breve{\bar{\beta}}(\breve{\bar{p}} \dagger \mathrm{C}) \prec \breve{\mathrm{A}}(p \dagger \beta\breve{\bar{\beta}})(\breve{\bar{p}} \dagger \mathrm{C}) \prec \breve{\mathrm{A}}p(\breve{\bar{p}} \dagger \mathrm{C})$$

$$-\!\!< \breve{A}(p\breve{\bar{p}} \dagger C) -\!\!< \breve{A}C$$

and

$$-\!\!< \breve{A}n \dagger C$$

We have also

$$-\!\!< \breve{A}\{p \dagger n(\breve{\bar{p}} \dagger C)\}$$

which appears not to imply the others. It appears therefore that in the last case there may be conclusions of the 2^{nd} order also. But the theory of such cases is not very easily made out.

When β is connected by multiplication in the two premises there is only one conclusion, which is of the 2^{nd} order and contains n.

In every case in which the middle term enters without a dash in one premise and with a dash in the other, it can be eliminated. The order of the conclusion is usually the sum of the orders of the premises but in some cases is less by some even number.

Particular forms of syllogism arise when the middle term is such that

$$\beta\breve{\beta} -\!\!< 1$$

or when there is some other third premise.

Note on the Boolian Algebra

MS 401: Fall 1881–Spring 1882

Let the variables, x, y, z, etc. denote propositions. Let two constant numbers v and f be chosen arbitrarily, and let the equality of any variable to v signify that the proposition it denotes is true, while its equality to f signifies falsity.

Then since every proposition is either true or false, every variable must satisfy the equation

$$(x - \mathrm{f})(x - \mathrm{v}) = 0.$$

In some states of things one factor, in others the other may vanish. If the proposition y is true in every state of things in which the proposition x is true, the equation is satisfied, that

$$(x - \mathrm{f})(y - \mathrm{v}) = 0$$

for otherwise x might be true and y false in the same state of things.

The equations of the two syllogistic premises 'If X, then Y' and 'If Y, then Z', are

$$(x - \mathrm{f})(y - \mathrm{v}) = 0$$
$$(y - \mathrm{f})(z - \mathrm{v}) = 0$$

Multiplying the first by $(z - \mathrm{v})$ and the second by $(x - \mathrm{f})$ and subtracting one from the other, and dividing by $(\mathrm{v} - \mathrm{f})$ we get

$$(x - \mathrm{f})(z - \mathrm{v}) = 0$$

which signifies 'If X, then Z', which is the syllogistic conclusion.

Any expression which becomes equal to v when any one or more of the variables x, y, z, etc. is equal to v and which becomes equal to f when x, y, z, etc. are all equal to f, represents the logical *aggregate* of the propositions x, y, z, etc. The simplest expression of this sort is

$$v + \frac{(x - v)(y - v)(z - v)}{(f - v)(f - v)}.$$

Any expression which becomes equal to f when either x, y, z, etc. is equal to f and which becomes equal to v when x, y, z, etc. are all equal to v, represents the logical compound of x, y, z, etc. The simplest such expression is

$$f + \frac{(x - f)(y - f)(z - f)}{(v - f)(v - f)}.$$

Any expression which becomes equal to f when x is equal to v, and becomes equal to v when x is equal to f, represents the negative of x. The simplest such expression is

$$f + v - x.$$

The variables, instead of denoting propositions, may denote terms. In that case v will denote what is present, f what is absent, and the algebra will remain unaltered.

The particular numbers chosen for f and v are perfectly arbitrary. Boole takes $f = 0$, $v = 1$. This makes the expression for the logical aggregate of x, y, z, etc.,

$$1 - (1 - x)(1 - y)(1 - z) \text{ etc.}$$

and the expression for the logical compound of the same letters

$$xyz \text{ etc.}$$

I have preferred to take $f = 0$, $v = \log \infty$. It must be remembered that $\log \infty$ is, although infinite, of the zero order of infinity. Consequently

$$\log \infty + \log \infty = \log \infty$$
$$\log \infty - \log \infty = 0$$

although the values of these expressions would be indeterminate with ordinary algebraical infinity, $\frac{1}{0}$. Moreover

$$\infty^0 \times \infty^0 = \infty^0.$$

But

$$0 \times \log \infty = 0.$$

Writing, now, for short, ∞ instead of $\log \infty$, we have

$$\infty x = x,$$

for this is true whether x be zero or infinity. Hence also

$$x = \frac{x}{\infty}.$$

Bearing these propositions in mind, it is easily seen that the expression for the logical aggregate of x, y, z, etc. becomes

$$x + y + z$$

while the expression for the logical compound remains

$$x \times y \times z.$$

But the latter might, perhaps more philosophically, be expressed in the form

$$-\log(\mathfrak{G}^{-x} + \mathfrak{G}^{-y} - \mathfrak{G}^{-x-y})$$

Proof of the Fundamental Proposition of Arithmetic

MS 402: Fall 1881–Spring 1882

The proposition is that the order of sequence in which the things of any collection are counted makes no difference in the result, provided there be any order of counting in which the count can be completed.

I wish to use this language. Suppose there is a class of ordered pairs such that PQ is one of them (QP may, or may not, belong to the class). Then, supposing λ signifies this class of pairs, I say that P is λ of Q and Q is λ'd by P.

Suppose a collection of things, say the A's is such that whatever class of ordered pairs λ may signify, the following conclusion shall hold. Namely, if every A is λ of an A, and if no A is λ'd by more than one A, then every A is λ'd by an A. If that necessarily follows, I term the collection of A's *finite.* That is the sense in which I use the word *finite.*

I begin with the following lemma. Every collection of things the count of which can be completed by counting them in a suitable order of succession is finite. For suppose there be a collection of which this is not true, and call it the A's. Then there is some relative, λ, such that while every A is λ of an A, and no two A's λ of the same A, there is some A not λ'd by any A. Remove this A which is not λ'd by any A. Then, the same thing will be true. Namely, 1^{st}, every A is still λ of an A, for no A λ'd by an A has been removed; 2^{nd}, no two A's are λ of the same A; and third there is an A not λ'd by any A, namely, that A which was λ'd by the removed A, and by no other A. Now if we consider the terminated counting of the collection, and lower by one every cardinal number higher than that which in the counting was called against the removed A, we see that after this A has been removed, the counting of the collection can still be terminated; only it is terminated by a number less by one than before. It follows by a Fermatian inference that if there be a collection not finite the count of which can by a suitable arrangement be terminated by any number n, then the same is true of some collection the count of which can be terminated by any lower number. Then there must be some collection whose count can be terminated by 1 which is not finite. But if this unit, say A, is λ to an A, which can only be itself, it is λ'd by an A; and so it is finite; and thus the original supposition is reduced to absurdity, and the lemma is proved.

The whole difficulty of the main proposition will be found to be contained in this lemma (which another proposed proof virtually takes for granted). For let the A's, which have been counted in two ways, be ranged in a row, with the number which was called against each in the first count written above it, and that which was called against it in the second count written below it, and let the terminating number of the second count (if either) be the greatest. Let the cardinal numbers from 1 up to the highest number of the second count be called the α's. Then, as they stand written above and below the A's, every α is under an α, but no two α's are under the same α (for no number occurs twice in the upper line). Consequently, the number of α's being finite (since a count of them is terminable), every α is above an α, or in other words every α, including the greatest, is found in the upper line and was used in the first count.

I may mention that I have written off this proof without running over it in my mind; for the principles of logic showed me that a "syllogism of transposed quantity" must be used, and that for that purpose, the lemma was required; and further that this lemma could only be proved by Fermatian inference. Of course, such a proposition has only a logical interest.

Comparison of the Metre with a Wave-Length of Light

MS 403: Fall 1881–Summer 1882

Part I.

Metallic bars used as standards of length having more than once been found to have changed their lengths in the course of years, three different means of measuring such changes have been suggested. First, the standard length may be marked upon the surface of a metallic tube or bottle, and the amount of water at 4°C which this bottle holds may be determined by weighing. Supposing, then, that the metal undergoes a permanent expansion or contraction equally in its three dimensions, the mass of water it contains will be altered three times as much as its length. Second, the length of the standard bar may be compared with that of the seconds pendulum at a fixed station. Third, the standard length may be compared with that of a wave of light identified by a line in the solar spectrum. The present work is an attempt to apply this third method.

The whole operation has two parts; the first consisting in the measurement of the angular deviation of a ray of light in traversing under fixed conditions a given diffraction-grating, the second in the comparison of the width of the grating with the length of a metre.

The gratings used have been ruled upon glass, expressly for this work, upon the machine of Mr. Rutherfurd,[1] in whose laboratory the work has been mainly performed, and to whose generous and invaluable counsels such measure of success as has been attained is due. The spacing of the gratings ruled by Mr. Rutherfurd's machine is determined by the motion of a micrometer-screw whose head consists of

1. For a description of this machine, written by Professor A. M. Mayer, see Appleton's *New American Cyclopaedia,* Article "Spectrum."

a wheel with 360 teeth upon its circumference. If a certain pallet falls at the passage of every tooth, about 6808 lines are ruled per centimetre; but the pallet may be held up so as to fall only at every other tooth when only 3404 lines per centimetre are ruled, or it may be held up over several teeth as to produce a blank space.

The first series of measurements of deviation were made with a grating ruled for the purpose and marked No. 1. This grating has about 340 lines per millimetre, and after it had been ruled, the screw was turned back to the starting point and the plate was passed through the machine a second time when the pallet was allowed to fall at every 681st tooth only. Thus every 340th line is of double thickness and the plate is thus approximately divided into millimetres. This spectra given by this plate are such as were considered fine a few years ago but would now only be rated as moderately good. The overlapping of the spectra gave the part observed very different colors on the two sides. After the measures were completed, the lines were filled with plumbago; and since that the colors are nearly alike on the two sides.

At a later date, a dozen new plates were ruled; but only three of these were good enough for the purpose, and only one, marked H, was very fine. These gratings have all 681 lines per millimetre. They are terminated at both ends by three lines distant from each other and from the grating by 7 teeth of the wheel. The distance from the mean of the three lines at one end to the mean of the three at the other end, is in two of the gitters, H and I, 13620 teeth or about 2 centimetres, and in the third, F, 6810 teeth or 1 centimetre. It is hardly necessary to say that every plate has a line perpendicular to the ruling, which marks the point at which the measurement of the width of ruling is to be made.

The following is a detailed description of the four diffraction-plates used. *[. . .]*

The rulings upon these plates have three kinds of inequalities, the progressive, the periodic, and the accidental.

Owing probably to a gradual squeezing out of the oil from between the screw and its nut, it is found that the lines at one end of a plate are frequently nearer together than at the other. This is shown by alternately covering one and the other half of a large diffraction-plate while in the spectrometer; when the two spectra will be found not to coincide. In order further to test this, a scale of 5 centimetres was ruled upon the machine, the divisions being each marked by three lines distant 7 teeth from one another. The screw was turned rapidly in passing over each centimetre, so that the circumstances were not precisely those under which a grating is ruled. The different centimetres of this scale were then compared upon the comparing instrument described below. After the first comparison the scale was taken out of the comparing instrument and put back after being turned end for end. The details of these comparisons are given in the record of comparisons, at the end of this paper, under date of 1879 March 14–15 and the results were as follows:—

HALF DECIMETRE SCALE OF CENTIMETRES, No. 4.

	Excess of each centimetre over the mean.				
	1st cm.	2nd cm.	3rd cm.	4th cm.	5th cm.
First comp.	$+0\overset{\mu}{.}74$	$+0\overset{\mu}{.}29$	$-0\overset{\mu}{.}08$	$-0\overset{\mu}{.}41$	$-0\overset{\mu}{.}55$
Second comp.	+0.66	+0.26	−0.05	−0.31	−0.53
Mean	+0.70	+0.27	−0.06	−0.36	−0.54
Excess over mean of three middle cm.	+0.75	+0.32	−0.01	−0.31	−0.49

It thus appeared that there is a change of pitch or scale of ruling at the rate of about $\frac{1}{30000}$; so that if light were to fall only on a part of a grating having a similar inequality of ruling, a shifting of the light along the grating by only one millimetre would produce an inadmissible difference in the position of the spectrum. This experiment convinced me of the necessity of using the utmost care in having the whole grating uniformly illuminated during the measures of deviation; and I may add that the smallest negligence in this respect always gave rise to large discrepancies in the deviations.

In regard to the periodical inequality of rulings made upon Mr. Rutherfurd's machine, I have published some studies, mathematical and experimental, in a separate paper.[2] This inequality gives rise to subsidiary images, or "ghosts," of each spectrum. The distance of these from one another shows the period of the inequality, which is found to be 357 teeth of the wheel forming the screw-head. Their relative brilliancy shows the amplitude of the inequality, which is different in different plates. If N be the order of that spectrum in which the first ghost is of equal brightness with the principal spectrum then $\frac{1}{2\pi N}$ is the ratio of the greatest displacement of a line from

2. "On the Ghosts in Rutherfurd's Diffraction-Spectra." *American Journal of Mathematics,* Vol. 2, p. 330.

the mean position to the mean width of the ruling. Since for the plates used we may take N = 8, the maximum displacement from this cause is about $0^{\mu}.03$. Now 38 complete periods of the inequality are equal to 13566 ± 10 teeth. Consequently the distance between the extreme lines of a two centimetre grating will exceed a whole number of periods by from $\frac{44}{360}$ to $\frac{63}{360}$ of a period. It follows that the relative displacement of the extremes of the double centimetre may amount to $0^{\mu}.02$ or $0^{\mu}.03$, but is probably less than one-millionth of the distance. It must be admitted, however, that the value assumed for N is a mere guess, inasmuch as the higher orders of spectra are too faint for observation with a transmission-grating. Gratings of the same width of ruling have been examined (and they were by no means bad ones) in which N had a value between 3 and 4; and it must be admitted that this is one of the weak points of the work.

In order to determine the mean magnitude of the accidental inequalities of the ruling, two scales were ruled of 12 divisions of 7 teeth, or about 10 microns, each. These divisions were then compared together. The details are given at the end of the report. The results are as follows.

Observed excess of each $\frac{1}{100}$ mm. *over mean.*

No. of division.	First scale.	Second scale.
1	$-0^{\mu}.02$	$-0^{\mu}.06$
2	−0.02	−0.01
3	−0.01	+0.01
4	+0.00	+0.01
5	−0.00	−0.01
6	+0.02	−0.01
7	−0.03	+0.01
8	+0.01	+0.01
9	−0.03	−0.01
10	+0.01	+0.01
11	+0.02	+0.01
12	+0.01	−0.01

The probable error of a single division calculated from these numbers is $\pm 0^{\mu}.0145$. But this involves the error of observation. Now each of the above numbers is the mean of 6 pointings, and from the discrepancies of the single pointings it appears that the probable error of the mean of 6 is $\pm 0^{\mu}.0122$. Hence, the probable error of the division is $\pm \sqrt{(0^{\mu}.0145)^2 - (0^{\mu}.0122)^2} = 0^{\mu}.01$. Thus, the purely accidental errors of ruling amount to only one two-millionth of the breadth of a two centimetre grating. This supposes the extremities of a division to be marked by single lines. Since they are actually marked by three lines the error to be feared from this cause is only one three-millionth of the breadth of the grating.

The preliminary series of measures of deviation were made upon a spectrometer with a glass circle belonging to Mr. Rutherfurd. All the other measures were made upon a spectrometer constructed by Messrs. Stackpole & Brothers, Fulton Street, New York, after a design by Mr. Rutherfurd and me. This spectrometer is shown in the plate. The circle of glass is divided into spaces of 10′. It is read by two microscopes which are provided with low power and high power oculars. The lenses are by Byrne of New York. The slit is moved by a screw with a graduated head and opened and closed by another. Each division of the screw-heads of these screws is equivalent to $5\frac{1}{2}''$. A filar micrometer is attached to the telescope, the revolution of the screw being equivalent to 197″. The diffraction-plate sets upon a little turn-table unprovided with graduation. The whole spectrometer stands upon an accurately made brass turn-table with a graduated circle, and this turn-table is set into the top of a solid three-legged mahogany table, which is leveled up by putting pieces of metal under the feet.

I now proceed to describe the manner in which the measures of deviation were made. The light first fell upon a heliostat, usually of the Fahrenheit pattern, carrying in the later measures an accurately plane mirror, from which the beam was reflected to a *porte-lumière* having a plane and parallel glass mirror. From thence it was reflected horizontally and approximately in a northerly direction. In the preliminary series of experiments, an integrating condenser was used, consisting of a telescope in sidereal focus having such a magnifying power that the spot of light well covered the grating. In the later experiments, this was replaced by a differentiating condenser, consisting of a well-corrected achromatic lens by Alvan Clark and Sons, the mate of the lens of the collimator. By this means, the image of the sun was brought accurately into focus upon the slit-plate, and the slit passing through the middle of the image. The focus was determined, first, by examining the image through colored glass by means of a high-power loupe, and, second, by observing that the boundaries of the spectrum, above and below, were sharply defined in the telescope, after the collimator and telescope were focussed. By turning the turn-table carrying the spectrometer, the observer was able to bring the centre of the sun upon the slit. Great care was used to have the collimator accurately in sidereal focus. After the preliminary series of experiments, this was accomplished in the following manner. Attached to the focussing tube of the collimator was the index of a scale of hundredths of inches attached to the fixed tube. There was also a subsidiary telescope similar to that of the spectrometer, provided with a high-power eye-piece, and carrying a scale attached to its focussing part similar to that on the collimator. Then the collimator being brought nearly to sidereal focus, the spectrometer telescope was focussed on the slit. Next, the subsidiary telescope was brought into collimation with the collimator and was focussed on the slit, and its focus was read on the scale; afterwards the same telescope was collimated with the spectrometer telescope and focussed on its web and the focus was again read. If the two foci were the same, the collimator was in sidereal focus. If not, a suitable change was made in its focus and the experiment was repeated. During the observations, the spectrometer telescope never was precisely in sidereal focus, owing to the wedge shape of the glass diffraction-plate and, also, with some plates, owing to the progressive inequality of the ruling.

The axis of the spectrometer was made vertical by means of a spirit level placed on the telescope. When this adjustment was made, the collimator and telescope were horizontal. The beam of light was made horizontal by observing the sun with a Casella travelling theodolite. The slit was made vertical and the filar micrometer (when used) horizontal by means of a spirit level.

The adjustment of a diffraction-plate consists in bringing its ruled surface to the axis of the spectrometer, in making the ruled surface vertical, and in making the lines vertical. For the first adjustment a subsidiary lens was slipped over the objective of the spectrometer telescope so as to bring the ruling into focus. The plate was then moved forward or back until the line in the centre of the field appeared stationary when the plate was partly revolved. The optical axis of the telescope was found by revolving the sliding tube of the collimator carrying the slit and determining with the filar micrometer the mean position of the latter. The slit was then set so that when horizontal it passed through the optical axis; its middle point was next found and marked by a piece of paper pasted to the slit-plate. The plane of the ruling was then made vertical by moving it until the reflection from the ruled side of the plate, of the middle point of the slit, always fell on the same point. Finally, the lines were made vertical by making the middle point of the slit, as seen in the spectrum, fall as much below the optical axis on one side as above on the other.

The deviation was always observed at its minimum. In the preliminary series of measurements this was found by turning the plate one way and the other and setting it midway between the two positions in which the spectrum began to stir. But afterwards, since it seemed that the deviation observed in this way must always be too great, a very small telescope was mounted on the counterpoise of the spectrometer telescope and brought into accurate collimation with the latter. Then the reflection of the slit from the ruled surface of the grating being brought upon the cross-wire of the little telescope the part of the spectrum on the cross-wire of the main telescope was at its minimum deviation. Still later, the little telescope was replaced by a plane mirror which was adjusted so as to bring the reflection of the slit into coincidence with the line of the spectrum at its minimum angle on the cross-wire of the main telescope.

In making the measures, the sun was only allowed to fall on the slit just long enough to make a pointing, when a cap was placed over the condenser until the time came to reëxamine the pointing. The greatest precautions were observed in reading the circle. Sky-light was used for the microscopes. A subsidiary lens was first placed over the eye-piece of each microscope so as to bring the window or other aperture from which the light came into focus. The image of the light thus observed was brought into the centre of the field of view and the reading of the circle carrying the spectrometer accurately noted. At each reading the instrument was brought exactly into this position. After the microscopes were read the instrument was turned back, the light thrown on the slit, the pointing was reëxamined, and a criticism of it was regularly entered upon the record. The setting was said to be too much to the right or left by an amount indicated by one of the following carefully used adverbs.

Perhaps	$0''.0$	Very slightly	0.2
A trifle	0.1	Rather	0.3

Slightly	0″.4	Altogether	1.2
Perceptibly	0.5	Considerably	1.3
A little	0.6	Much	1.4
Somewhat	0.7	Grossly	1.5
Quite	0.8	Excessively	1.6
Rather decidedly	0.9	Inordinately	1.7
Decidedly	1.0	Extravagantly	1.8
Markedly *or*		Abominably	1.9
Very decidedly	1.1	Outrageously	2.0
		Intolerably	2.1

The order, as more or less strong, of these expressions being fixed by my own feeling, it was found that comparison with the residuals showed that these latter could be somewhat reduced by applying in each case a correction determined by neglecting "perhaps" and considering each interval between two successive adverbs to represent 0″.1. The correction derived from this table is referred to in this paper as the "correction for judgment."

The following is an example of the record of a single measurement of deviation.

Schneider's shop, Baltimore. 1881, April 16. Time of beginning: ———. Observer: C. S. P. Recorder: E. D. P. Gitter F. Usual line. Barom. Green No. 1936, 29^{in}.654, 51°.8. Dry bulb 54.5. Wet bulb, 47°.0.

Telescope to Right; Face of gitter, away.

Microscope		A		B		Mean		Corr. for judgment.
1st Pointing	360°02′	54″.8	50″.6	60″.2	57″.6	55″.8	Inordinately too red.	57″.5
Ther. Geissler No. 103, 13°.14; Baudin, No. 7317, 12°.00.								
2nd Pointing		54.6	51.9	61.2	58.5	56.5	Somewhat too red.	57.2
3rd Pointing		55.5	51.7	61.4	58.6	56.8	Rather too red.	57.1
Mean, (not corr. for runs)	360°02′					56.4		57.3

Telescope to Left; Face of gitter, to.

1st pointing	90°07′	15″.7	15″.7	48″.1	45″.4	31″.2	Altogether too green.	32″.4
Ther. Geissler, 13°.50; Baudin, 12°.33								
2nd pointing		17.5	18.1	50.4	47.3	33.3	A bit too green.	33.7½
3rd pointing		17.5	17.1	50.2	47.6	33.1	V. slightly too green.	33.3
Mean, (not corr. for runs)				90°07′		32.5		33.1

Telescope to Left; Face of gitter, away.

Microscope		A		B		Mean		Corr. for judgment.
1st pointing	90°06′	46″.7	46″.7	79″.4	77″.1	62″.5	Extravagantly too green.	64″.3
		Ther. Geissler, 13°.90; Baudin, 12°.62						
2nd pointing		50.4	51.1	83.7	81.6	66.7	Quite too red.	65.9
3rd pointing		50.6	48.4	83.2	79.6	65.5	A trifle too green.	65.6
Mean (not corr. for runs)				90 06′		64.9		65.2

Telescope to Right; Face of gitter, to.

1st pointing	360°03′	37″.6	33″.7	43″.6	40″.6	38″.9	A little too red.	39″.5
		Ther. Geissler, 13°.70; Baudin, 12°.68.						
2nd pointing		37.4	34.0	42.7	39.6	38.4	Outrageously too red.	40.4
3rd pointing		38.3	35.2	44.7	41.7	40.0	Markedly too red.	41.1
Mean (not corr. for runs)						39.1		40.3

Collimator shifted. Noticed that microscope A bisects the same part of the line that microscope B bisected before the shifting.

Telescope to Right; Face of gitter, to.

1st pointing	270°01′	46″.4	44″.4	66″.1	67″.2	56″.0	Altogether too red.	57″.2
		Ther. Geissler, 14°.03; Baudin, 12°.86						
2nd pointing		47.3	44.8	68.4	68.4	57.2	A trifle too red.	57.3
3rd pointing		47.0	46.2	67.9	69.3	57.6	Rather decidedly.	58.5
Mean (not corr. for runs)	270°01′					56.9		57.6

Telescope to Left; Face of gitter, away.[3]

1st pointing	360°05′	16″.1	17″.6	28″.4	28″.4	22″.6	Decidedly too green.	23″.6
		Ther. Geissler, 14°.33; Baudin, 13°.09						
2nd pointing		22.8	19.2	29.2	26.6	24.4	Good.	24.4
3rd pointing		18.8	16.2	25.4	21.6	20.5	Rather decidedly too green.	21.4
Mean (not corr. for runs)	360°05′					22.5		23.1

Telescope to Left; Face of gitter, to.

1st pointing	360°05′	55″.3	51″.5	60″.6	58″.7	56″.5	A little too green.	57″.1
		Ther. Geissler, 14°.55; Baudin, 13°.34						
2nd pointing		55.3	51.5	61.9	58.6	56.8	Quite too green.	57.6
3rd pointing		57.7	55.1	64.4	61.4	59.6	Decidedly too red.	58.6
Mean (not corr. for runs)	360°05′					57.6		57.8

3. *Remark.* Second line of 1st Microscope too near fixed wire to be well read.

Telescope to Right; Face of gitter, away.

Microscope		A		B		Mean		Corr. for judgment.
1st pointing	270°01′	07″.4	06″.4	27″.7	29″.3	17″.7	Decidedly too red.	18″.7
Ther. Geissler No. 103, 14°.44; Baudin, 13°.31								
2nd pointing		08.4	05.1	29.1	29.4	18.0	Quite too red.	18.8
3rd pointing		08.6	07.0	28.5	29.5	18.4	A trifle too red.	18.5
Mean (not corr. for runs)	270°01′					18.0		18.7

Time of ending 1^h15^m. Bar. Green 1936, $29^{in}.682$, 55°.0.

It has been assumed that the runs of the microscopes would remain unaltered for a month at a time. In the principal series of measures a double deviation of 90° was measured upon every quadrant of the circle, beginning with the space between the line marked by a multiple of 15° and the ten minute mark next following it. Two microscopes having been read, there were only twelve of these different parts of the circle. On each day, the double deviation was measured upon two of the parts, so as to cover the whole circle. In this way, every one of the 24 ten minute spaces beginning with a multiple of 15° was measured either twice with one microscope or once with each microscope. The results are shown in the following table. *[. . .]*

Principal Line. Gitter H
Principal Series.

1879	Temp. F	Double deviation corr. for barom.	Corr. for runs.	Corr. for judgement.	Red. to 70°	
May 8	70°.7	90°03′52″.5	52″.4	52″.5	53″.3	
" 9	71.7	51.1	51.0	51.2	53.1	
" 10	66.1	57.9	57.8	58.0	53.5	
" 15	74.3	46.3	46.2	46.6	51.5	Definition bad
" 21	74.4	47.9	47.8	47.9	52.9	
" 22	68.2	54.5	54.4	54.3	52.2	

Result 90°03′52″.75 ± 0″.25

Principal Line. Gitter H
Subsidiary measures.

1879	Temp. F	Double deviation corr. for barom.	Corr. for runs.	Corr. for judgement.	Red. to 70°
1880					
Feb. 19	52°5	90 03 72.9	73.1		53.0
" 20	43.7	83.0	83.2		53.1
1881					
Feb. 24	41°7	90 03 86.8	86.5	86.6	54.3
Mar. 2	43.9	81.3	81.0	81.3	51.5
" 6	49.0	76.3	76.0	75.7	51.7
" 11					
" 15					
" 16					
" 25					

Gitter I produces a spectrum inferior in definition to H or even F. For this reason it was hardly used before 1881, when three measures of the principal line were made with it. On 1879, May 6, however, it was used to measure a line near the principal line, which had then not been chosen. These measures can be reduced to the principal line, by means of the following micrometrical measures.

1879, May 26. Barometer, 9^h42^m. $750^{mm}_{.}5$ 17°6 [A correction of $+9^{mm}_{.}8$ to barom. and also of $-0^{mm}_{.}1$ for difference of elevation.] Gitter F. Five lines

E
A = Kirchhoff 1193
B = " 1200.6 = Principal Line.
C = " 1207 = V. der W. No. 16.
D = " 1218 = r. fine double of May 6.

Gitter face away. Telescope

Circle readings.	Before micrometer readings	300°	02	30.3
		120	02	30.5
		300	02	30.4
	After micrometer readings	300	02	29.6
		120	02	29.5
		300	02	29.6
	Mean	300	02	30.0

Micrometer.	E	A	B	C	D
	3ʳ776	7ʳ588	9ʳ044	10ʳ411	12ʳ369

Thers. a.c.a. above 61°.6. c.s.p. below 61°.5

Circle moved. Now reads

Before micrometer readings	300	07	31.0
	120	07	31.9
	300	07	31.5
After micrometer readings	300	07	29.5
	120	07	31.9
	300	07	30.7
Mean	300	07	31.1

Micrometer.	E	A	B	C	D
	$5^{\mathrm{r}}.300$	$9^{\mathrm{r}}.117$	$10^{\mathrm{r}}.553$	$11^{\mathrm{r}}.933$	$13^{\mathrm{r}}.885$

Ther. above 61°.3; below 61°.5

Gitter face to. Telescope

Circle reading.

Before micrometer readings	210°	02	31.3
	30	02	26.7
	210	02	29.0
After micrometer readings	210	02	30.4
	30	02	26.0
	210	02	28.2
Mean	210	02	28.6

Micrometer.	D	C	B	A	E
	$6^{\mathrm{r}}.695$	$8^{\mathrm{r}}.638$	$10^{\mathrm{r}}.026$	$11^{\mathrm{r}}.463$	$15^{\mathrm{r}}.272$

Ther. above 61°.1; below 61°.2

Circle moved. Now reads

Before micrometer readings	210	07	31.2
	30	07	29.7
	210	07	30.4
After micrometer readings	210	07	31.8
	30	07	30.5
	210	07	31.2
Mean	210	07	30.8

Micrometer.	D	C	B	A	E
	$8^{\mathrm{r}}.230$	$10^{\mathrm{r}}.172$	$11^{\mathrm{r}}.554$	$12^{\mathrm{r}}.999$	$16^{\mathrm{r}}.806$

Ther. above 61°.6; below 61°.2

Gitter face away. Telescope

Circle unmoved. Reading.

After micrometer readings	210°	07′	31″.8
	30	07	30.4
	210	07	31.1
Mean	210	07	31.1

Micrometer.	D	C	B	A	E
	8ʳ.405	10ʳ.367	11ʳ.745	13ʳ.189	17ʳ.014

Ther. above 61°.3; below 61°.3

Circle moved. Now reads

Before micrometer readings	210	02	30.9
	30	02	27.5
	210	02	29.2
After micrometer readings	210	02	31.7
	30	02	27.7
	210	02	29.7
Mean	210	02	29.4

Micrometer.	D	C	B	A	E
	6ʳ.883	8ʳ.845	10ʳ.219	11ʳ.667	15ʳ.478

Ther. above 61°.7; below 61°.8

Gitter face to. Telescope

Circle readings.

Before micrometer readings	300	02	29.1
	120	02	28.7
	300	02	28.9
After micrometer readings	300	02	27.6
	120	02	27.0
	300	02	27.3
Mean	300	02	28.1

Micrometer.	E	A	B	C	D
	3ʳ.571	7ʳ.383	8ʳ.836	10ʳ.211	12ʳ.178

Ther. above 63°.4; below 63°.3

Circle moved. Now reads

Before micrometer readings	300	07	28.7
	120	07	31.1
	300	07	29.9
After micrometer readings	300	07	27.4
	120	07	30.4
	300	07	28.9
Mean	300	07	29.4

Micrometer.	E	A	B	C	D
	5ʳ.094	8ʳ.921	10ʳ.371	11ʳ.740	13ʳ.701

Ther. above, 62°.2; below, 61°.8

The four movements of the circle give us for the value of the micrometer revolution (after correcting for runs)

	300″.9 =	302″.0 =	301″.5 =	301″.1 =	
E	1ʳ.524	1ʳ.535	1ʳ.536	1ʳ.523	
A	1.529	1.534	1.522	1.538	
B	1.509	1.528	1.526	1.534	
C	1.522	1.536	1.522	1.529	
D	1.516	1.534	1.522	1.523	
Means	1.520	1.533	1.526	1.529	
1 rev. =	198″.0	197″.0	197″.6	196″.3	Mean 197″.2

The eight sets of micrometer readings give for the distances of the spectral lines

	EA	AB	BC	CD
	3ʳ.812	1ʳ.456	1ʳ.367	1ʳ.958
	3.817	1.436	1.360	1.952
	3.809	1.437	1.388	1.943
	3.807	1.445	1.382	1.942
	3.825	1.444	1.378	1.962
	3.811	1.448	1.374	1.962
	3.812	1.453	1.375	1.967
	3.827	1.449	1.370	1.961
Means	3.815	1.446	1.374	1.956
In seconds	752″.3	285″.2	271″.0	385″.7

The following is the *résumé* of the measures made with Gitter F. The measures show just as those with Gitter H did, that when such a coëfficient of expansion is used as to bring the measures of the spring of 1879 into accord with those of the following winter, then the measures of 1881 give a deviation somewhat smaller.

Principal line. Gitter F

	Reduced to 70°	Temp.
1879, May 27	90° 03′ 44″.8	62°
Dec. 18	44.3	50
1881, April 10	90 03 39.0	53
April 11	39.4	56
April 16	43.1	55

Gitter No. 1 was used in the preliminary measures with Mr. Rutherfurd's spectrometer upon the line No. 16 of Van der Willigen. The fact that this line could be chosen as suitable for observation shows in a strong light the unsatisfactory character of the spectra produced by this gitter. After the preliminary measures, the lines of this gitter were filled with black lead. This certainly improved the definition considerably while still leaving it inferior to that of I; but what is very surprizing is that since this operation the deviation of lines with this gitter seems to have entirely changed. The following is a *résumé* of the measures.

Principal Line. Gitter No. 1.
Reduced to 70°

1879, Dec. 13.	90° 03′ 44″.5
1881, April 3	44.3
" " 7	44.0

Measures of Length.

The comparisons of the width of the gitters with centimetres and double centimetres, of the different centimetres of scales of decimetres together, of different decimetre scales, and of the different decimetres of a metre scale of decimetres have all been made upon a comparing instrument invented by me and made in Mr. Rutherfurd's laboratory. The instrument has proved a perfect success for all these purposes except the last, and these last measures require to be repeated upon a new instrument.

A pair of iron ways were straightened until a high power telescope running over them upon a carriage remained sensibly in collimation with a collimator. Upon these ways were then placed two carriages. The first only carried the scale to be compared from one division to another and needs no particular attention. The other carried three microscopes 10 inches long, provided each with a filar micrometer, having objectives of $\frac{1}{5}$ inch specially made for the purpose by W. Wales, and magnifying some 1500 diameters. These microscopes were very solidly mounted so as to be all vertical, to have coincident focal planes, and to have the middle points of their fields in a straight line and equidistant. The two outside microscopes numbered No. 1 and No. 2 looked at two fixed scales called "donkeys"; while the middle one, numbered 3, looked at the scale to be compared. The whole instrument was cemented onto a block of marble and covered with a glass case, itself enclosed in a case made of two thicknesses of tinned iron enclosing a thickness of 5 mm of raw cotton. The filar micrometers projected from the case, but the slots in which they moved were closed by pieces of thick pasteboard attached to the microscopes. Between the glass case and the outer covering was a metallic thermometer which on the least change of temperature automatically opened and closed the register of the hot-air apparatus with which the room was heated. A thermometer was kept between the ways, close to the scale to be compared.

The micrometers having only been used to measure very minute differences, it is not worth while to consider any temperature correction. Moreover, the objectives having been constructed of very short focus, the values of the screw-revolutions may be assumed as constant throughout the work. In determining this value, we may also take the rulings made on Mr. Rutherfurd's machine as constant. It is found that in the mean of a large number of measures

14 teeth of Rutherfurd's machine

On	Micrometer	1	measure	$3\overset{r}{.}1352$
"	"	2	"	3.1764
"	"	3	"	3.1550

We may therefore take the mean of Micrometers 1 and 2 as having the same value as Micrometer 3.[4] Since there are about 681 teeth to the millimetre, it follows that the value of the revolution may be taken at $\frac{14}{681}\cdot\frac{1}{3.1550}$ mm or $6\overset{\mu}{.}517$.

The following is a list of the comparisons made upon this machine. Those which are essential to the operation are marked ☞. Every comparison consists of four pointings on each line, unless otherwise stated.

Comparisons of Centimetres.

All these comparisons form one series, the donkeys having been undisturbed.

1879, Jan 14–18. Centimetres of decimetre scale No 1. Temp 62°.

1879, Jan 20–24. Centimetres of decimetre scale, No 2. Temp 65°.

1879, Jan 27–29. The same after reversal. Temp 71°. 2 pointings on each line.

1879, Jan 30–Feb 1. Centimetre No 3. Temp 69°, 65°, 75° Three comparisons at different temperatures.

☞ 1879, Feb 8–10. Centimetres of Decimetre scale No 4. Temp 71°.

☞ 1879, Feb 12–14. The same after reversal. Temp 72°.

1879, Feb 15. Old Centimetre, No 1. Dated 1878 Jan 18. Temp 67° Three complete comparisons.

4. Although this mode of reduction is sufficiently accurate, a more rigorous process has in point of fact been pursued.

☞ 1879, Feb 18–19. Centimetres of decimetre scale, No 4. Temp 71°, 67°.

1879, Feb 20. Old centimetre, No 1. Temp 66°. Two comparisons.

☞ 1879, Feb 22–24. Centimetres of decimetre scale, No 4. Temp 65°.

1879, Feb 25. Old centimetre, No 1. Temp 66°. Two comparisons.

☞ 1879, Feb 27. Centimetres of decimetre scale No 4. Temp 72° and 67°.

1879, March 1. Old centimetre No 1. Temp 64°.

☞ 1879, March 10–12. Gitter, No 1. Ten comparisons. After every second comparison the gitter was removed replaced and readjusted. Temp 64° to 69°.

1879, March 14–15. Centimetres of half-decimetre scale No 4. Temp 69°.

1879, March 17. The same after reversal. Temp 69°.

☞ 1879, April 4 to 11. Gitter, No 1, after the filling of its lines with plumbago. Sixteen comparisons. The gitter was removed after every comparison. Temp 56° to 75°.

Comparisons of Double Centimetres.
First Connected Series.

☞ 1879, June 19–20. Pairs of Centimetres of Decimetre scale, No 4. Temp 68°.

☞ 1879, June 23. Pairs of Centimetres of Decimetre scale, No 3. Temp 70°.

☞ 1879, June 24. The same, after reversal. Temp 73°.

☞ 1879, June 25. Gitter I. Two comparisons, between which the gitter was reversed. Temp 75°.

☞ 1879, June 26–27. Gitter H. Four comparisons. After the second the gitter was reversed. Temp 76°.

☞ 1879, June 28. Pairs of centimetres of decimetre scale, No 3. Temp 78°.

☞ 1879, June 30. The same after reversal. Temp 77°.

☞ 1879, July 1. Gitter H. Four comparisons. After the second, the gitter was reversed. Temp 77°.

☞ 1879, July 2. Pairs of centimetres of decimetre scale No 3. Temp 73°.

☞ 1879, July 3. The same after reversal. Temp 76°.

☞ 1879, July 4. Gitter H. Four comparisons. After the second, the gitter was reversed. Temp 79°.

☞ 1879, July 5. Pairs of centimetres of decimetre scale No 3. Temp 79°.

☞ 1879, July 7. The same, after reversal. Temp 74°.

☞ 1879, July 8. Gitter H. Four comparisons. After the second, the gitter was reversed. Temp 79°, 74°.

☞ 1879, July 9. Pairs of centimetres of decimetre scale No 3. Temp 75°.

☞ 1879, July 10. The same, after reversal.

☞ 1879, July 12. Gitter I. Two comparisons, between which the gitter was reversed. Temp 76°.

Second Connected Series.

☞ 1880, Jan 7. Gitter I. Temp 69°.
☞ 1880, Jan 8. Gitter F, compared first with the right hand centimetre and then with the left hand centimetre of the two centimetre donkeys. Temp 70°.
☞ 1880, Jan 8. Gitter H. Temp 71°.
☞ 1880, Jan 9–10. Pairs of centimetres of decimetre scale No 3. Temp 69° and 72°.
☞ 1880, Jan 10–13. The same after reversal. Temp 72° and 69°.
☞ 1880, Jan 16. Gitter I. Two comparisons. Temp 41°.
☞ 1880, Jan 17. Gitter H. Two comparisons. Temp 42°.
☞ 1880, Jan 20–21. Pairs of centimetres of decimetre scale No 3. Temp 45°.
☞ 1880, Jan 21. The same after reversal. Temp 44°.
☞ 1880, Jan 26–27. The same. Temp 45°.

Comparisons of Decimetres.
First Connected Series.
(Low power objectives.)

☞ 1879, May 23–26. Decimetres of Glass metre, No 1. Temp 63°.
☞ 1879, May 27–30. The same, after reversal. Temp 65°.
☞ 1879, June 4–5. After an alteration in the instrument, first six decimetres of Glass metre No 1. Temp 73°.
☞ 1879, June 6–7. The same after reversal. Temp 74°.
☞ 1879, June 8–9. Decimetre scale No 4. Three comparisons. After the second, the scale was reversed. Temp 67°.
☞ 1879, June 9–10. Last four decimetres of Glass metre No 1. Temp 69°.
☞ 1879, June 10. The same, after reversal. Temp 70°.
☞ 1879, June 11–12. Fifth decimetre of Glass metre, No 1. Two comparisons. Temp 75°.
☞ 1879, June 12. Eighth and Ninth decimetres of Glass metre, No 1.
☞ 1879, June 13–14. Decimetre scale No 4. Four comparisons. After the second the scale was reversed. Temp 73°.

Second Connected Series
(Usual $\frac{1}{5}$ in. objectives.)

☞ 1879, July 14–31. Alternate comparisons of decimetre scales Nos. 3 and 4 as follows:—

No 3	left	position.	Temp	76°
" "	right	"	"	77
" 4	left	"	"	77
" "	right	"	"	80

No	3 left	position.	Temp	82°	
"	" right	"	"	84	
"	4 left	"	"	81	
"	" right	"	"	82	two comparisons.
"	3 left	"	"	75	
"	" right	"	"	70	two comparisons.
"	" left	"	"	73	
"	" right	"	"	72	
"	" left	"	"	73	
"	4 left	"	"	72	
"	" right	"	"	73	
"	" left	"	"	72	
"	" right	"	"	76	
"	3 left	"	"	75	
"	" right	"	"	76	
"	4 left	"	"	76	
"	" right	"	"	77	
"	3 left	"	"	76	
"	" right	"	"	77	
"	4 left	"	"	77	
"	" right	"	"	78	
"	3 left	"	"	71	
"	" right	"	"	71	
"	4 left	"	"	75	
"	" right	"	"	76	
"	3 left	"	"	77	
"	" right	"	"	77	
"	4 left	"	"	76	
"	4 right	"	"	78	
"	3 left	"	"	78	
"	" right	"	"	79	
"	4 left	"	"	77	
"	" right	"	"	78	

Third Connected Series.

☞ 1880, Jan 30–Feb 9. Alternate comparisons of decimetre scales Nos. 3 and 4 as follows:—

No	3 left	position.	Temp	40°	
"	4 "	"	"	44	
"	" "	"	"	49	Two comparisons.
"	" right	"	"	44	
"	" "	"	"	41	
"	3 right	"	"	44	Two comparisons.
"	4 left	"	"	41	
"	" "	"	"	44	

No 4 right position.	Temp	40°	Two comparisons.
" 3 left "	"	39	
" " " "	"	44	
" " " "	"	42	Two comparisons.
" " " "	"	46	
" " right "	"	46	Two comparisons.

The following are the results of the measures.

CENTIMETRES.
Gitter No. 1, before filling lines,
longer than Donkeys.

Note. The part compared begins at $4\frac{4}{360}$ revolutions from the side opposite No. 1 and goes $19\frac{30}{360}$ or 6810 teeth, ending $3\frac{34}{360}$ revolutions from the other side.

Temp.	Diff. in rev.	Diff. in microns	Reduced to 70°F
63.4	−.028	−0µ.18	−0µ.20
63.8	−.024	−0.16	−0.18
65.6	+.003	+0.02	+0.01
66.1	+.003	+0.02	+0.01
67.4	−.031	−0.20	−0.21
67.6	−.036	−0.23	−0.24
69.1	−.054	−0.35	−0.35
69.5	−.048	−0.31	−0.31
68.1	−.008	−0.05	−0.06
68.0	−.009	−0.06	−0.07
Mean			−0.16

Result doubtful owing to lines not being filled.

Gitter No 1, after filling lines,
longer than donkeys.

Temp.	Excess In rev.	Excess In microns	To 70°F
62°.3	−0r.015	−0µ.10	−0µ.13
62.7	−0.011	−0.07	−0.10
59.4	−0.009	−0.06	−0.10
60.8	−0.015	−0.10	−0.13
69.2	−0.045	−0.29	Focus wrong.
70.1	−0.047	−0.31	" "

Temp.	Excess In rev.	Excess In microns	To 70°F
56.0	−0.033	−0.22	−0.27
66.3	−0.027	−0.18	−0.19
75.0	−0.028	−0.18	−0.16
74.7	−0.027	−0.18	−0.16
74.3	−0.029	−0.19	−0.18
67.9	−0.021	−0.14	−0.15
67.8	−0.023	−0.15	−0.16
67.8	−0.023	−0.15	−0.16
66.1	−0.028	−0.18	−0.19
66.8	−0.018	−0.12	−0.13
Mean			−0.158

These measures are also somewhat discordant. Rejecting those in which the focus was wrong and dividing the others according to temperatures into two sets of ten we get—

$61^{\circ}.9 \quad -0^{\mu}.13$
$70.6 \quad -0.16$

Making a relative coëfficient of expansion of $-0^{\mu}.0034$ per degree F. With this, the last column above has been calculated.

Centimetres of Decimetre Scale No 4.

Temp	1st Centimetre Difference		To 70° F	Temp	2nd Centimetre Difference		To 70° F
$70^{\circ}.3$	$-0^{r}.115$	$-0^{\mu}.75$	$-0^{\mu}.75$	$70^{\circ}.4$	$-0^{r}.072$	$-0^{\mu}.47$	$-0^{\mu}.47$
72.6	−0.115	−0.75	−0.73	72.8	−0.070	−0.46	−0.43
71.6	−0.120	−0.78	−0.76	71.5	−0.062	−0.40	−0.38
64.3	−0.122	−0.80	−0.86	64.4	−0.076	−0.50	−0.56
71.6	−0.116	−0.76	−0.74	72.5	−0.073	−0.48	−0.45
Means			−0.77				−0.46
	3rd Centimetre				4th Centimetre		
$70^{\circ}.3$	$-0^{r}.141$	$-0^{\mu}.92$	$-0^{\mu}.92$	$70^{\circ}.1$	$-0^{r}.102$	$-0^{\mu}.67$	$-0^{\mu}.67$
72.7	−0.133	−0.87	−0.84	72.6	−0.110	−0.72	−0.69
71.6	−0.129	−0.84	−0.82	71.6	−0.102	−0.67	−0.65
64.5	−0.134	−0.87	−0.93	64.7	−0.107	−0.70	−0.76
72.5	−0.138	−0.90	−0.87	72.6	−0.103	−0.67	−0.64
			−0.88				−0.68

5th Centimetre

Temp	Difference		To 70° F
$70^{\circ}2$	$-0^{r}117$	$-0^{\mu}76$	$-0^{\mu}76$
72.6	−0.125	−0.82	−0.79
71.5	−0.125	−0.82	−0.80
64.8	−0.117	−0.76	−0.81
72.6	−0.121	−0.79	−0.76
			−0.78

6th Centimetre

Temp	Difference		To 70° F
$70^{\circ}1$	$-0^{r}052$	$-0^{\mu}33$	$-0^{\mu}33$
70.6	−0.054	−0.35	−0.34
66.6	−0.057	−0.37	−0.41
64.8	−0.034	−0.22	−0.27
66.1	−0.050	−0.33	−0.37
			−0.34

7th Centimetre

$71^{\circ}7$	$-0^{r}006$	$-0^{\mu}04$	$-0^{\mu}02$
70.6	−0.013	−0.08	−0.07
66.9	−0.017	−0.11	−0.14
65.2	−0.020	−0.13	−0.18
66.6	−0.018	−0.12	−0.16
			−0.11

8th Centimetre

$71^{\circ}5$	$-0^{r}146$	$-0^{\mu}95$	$-0^{\mu}93$
70.6	−0.129	−0.84	−0.83
66.9	−0.117	−0.76	−0.79
65.4	−0.121	−0.79	−0.84
67.0	−0.121	−0.79	−0.82
			−0.84

9th Centimetre

$70^{\circ}9$	$-0^{r}038$	$-0^{\mu}25$	$-0^{\mu}24$
71.0	−0.019	−0.12	−0.11
66.9	−0.006	−0.04	−0.07
65.4	−0.014	−0.09	−0.14
67.0	−0.021	−0.14	−0.17
			−0.15

10th Centimetre

$70^{\circ}0$	$-0^{r}072$	$-0^{\mu}47$	$-0^{\mu}47$
70.8	−0.069	−0.45	−0.44
66.8	−0.049	−0.32	−0.35
65.0	−0.065	−0.42	−0.47
67.0	−0.065	−0.42	−0.45
			−0.44

Excess of Double Centimetres over Donkey
Combination of the First Series.
Decimetre scale No 4.

	Temp			Reduced to 70°
1st and 2nd Cm.	$69^{\circ}1$	$-0^{r}008$	$-0^{\mu}05$	$-0^{\mu}04$
3rd and 4th Cm.	67.2	−0.057	−0.37	−0.34
5th and 6th Cm.	67.8	+0.005	+0.03	+0.05
7th and 8th Cm.	68.1	+0.033	+0.22	+0.24
9th and 10th Cm.	69.4	+0.090	+0.59	+0.61

Pairs of Centimetres of Decimetre Scale No 3.

1st and 2nd Centimetres

Temp			
68°.1	$+0^{\mathrm{r}}.191$	$+1^{\mu}.25$	+1.25
75.0	+0.195	+1.27	+1.26
77.3	+0.188	+1.23	+1.22
76.9	+0.179	+1.17	+1.16
71.5	+0.182	+1.19	+1.19
77.7	+0.184	+1.20	+1.19
78.3	+0.184	+1.20	+1.18
74.0	+0.189	+1.23	+1.22
73.5	+0.176	+1.15	+1.14
78.8	+0.188	+1.23	+1.21
			+1.20

3rd and 4th Centimetres

Temp			
69°.0	$+0^{\mathrm{r}}.092$	$+0^{\mu}.60$	$+0^{\mu}.60$
74.5	+0.100	+0.65	+0.64
77.9	+0.099	+0.65	+0.63
76.7	+0.100	+0.65	+0.64
72.0	+0.098	+0.64	+0.64
76.5	+0.106	+0.69	+0.68
78.6	+0.101	+0.66	+0.64
73.7	+0.103	+0.67	+0.66
74.0	+0.104	+0.68	+0.67
76.8	+0.111	+0.72	+0.71
			+0.65

5th and 6th Centimetres

Temp			
69°.5	$+0^{\mathrm{r}}.019$	$+0^{\mu}.12$	$+0^{\mu}.12$
72.7	+0.018	+0.12	+0.11
78.3	+0.024	+0.16	+0.14
76.4	+0.015	+0.10	+0.09
72.5	+0.021	+0.14	+0.13
76.2	+0.017	+0.11	+0.10
78.6	+0.021	+0.14	+0.12
73.5	+0.020	+0.13	+0.12
74.4	+0.026	+0.17	+0.16
76.5	+0.016	+0.10	+0.09
			+0.12

7th and 8th Centimetres

Temp			
69°.9	$-0^{\mathrm{r}}.095$	$-0^{\mu}.62$	$-0^{\mu}.62$
71.7	−0.102	−0.67	−0.67
78.6	−0.102	−0.67	−0.68
76.0	−0.104	−0.68	−0.69
73.1	−0.099	−0.65	−0.66
75.9	−0.098	−0.64	−0.65
78.6	−0.089	−0.58	−0.60
73.2	−0.097	−0.63	−0.64
76.0	−0.090	−0.59	−0.60
76.1	−0.094	−0.61	−0.62
			−0.64

9th and 10th Centimetres

Temp			
70°.9	$-0^{\mathrm{r}}.010$	$-0^{\mu}.06$	$-0^{\mu}.06$
72.2	−0.012	−0.08	−0.08
78.9	−0.013	−0.08	−0.10
79.9	−0.015	−0.10	−0.12
73.6	−0.009	−0.06	−0.07
75.3	−0.019	−0.12	−0.13
78.9	−0.013	−0.08	−0.10
72.8	−0.012	−0.08	−0.09
76.2	−0.008	−0.05	−0.06
75.5	−0.010	−0.07	−0.08
			−0.09

Gitter H.

Temp.			Reduced to 70°
$73^{\circ}.9$	$+0^{\mathrm{r}}.074$	$+0^{\mu}.48$	$+0^{\mu}.44$
74.4	+0.078	+0.51	+0.46
76.9	+0.083	+0.54	+0.47
77.4	+0.089	+0.58	+0.50
77.0	+0.076	+0.50	+0.43
77.1	+0.071	+0.46	+0.38
77.6	+0.082	+0.53	+0.45
77.6	+0.075	+0.49	+0.41
77.9	+0.080	+0.52	+0.44
78.4	+0.069	+0.45	+0.36
79.4	+0.074	+0.48	+0.38
79.9	+0.080	+0.52	+0.42
78.6	+0.081	+0.53	+0.44
79.7	+0.082	+0.53	+0.43
73.6	+0.080	+0.52	+0.48
74.1	+0.077	+0.50	+0.45
Mean			+0.434

Gitter I.

75.1	$+0^{\mathrm{r}}.421$	$+2^{\mu}.67$	$+2^{\mu}.62$
75.1	+0.425	+2.75	+2.70
75.2	+0.402	+2.60	+2.55
76.2	+0.417	+2.70	+2.64
Mean			+2.63

Excess of Double Centimetres over Donkey Combination of the Second Series.

Gitter H.

Temp.	Excess		Reduced to 70°
$71^{\circ}.3$	$+0^{\mathrm{r}}.104$	$+0^{\mu}.68$	$+0^{\mu}.67$
41.3	+0.052	+0.34	+0.64
42.0	+0.053	+0.35	+0.65
			+0.65

Gitter I.

Temp.	Excess		Reduced to 70°
69°.0	+0^r.439	+2$^\mu$.86	+2$^\mu$.87
41.9	+0.391	+2.55	+2.80
41.2	+0.395	+2.58	+2.84
			+2.84

Gitter F.

Temp.	Excess over 1st cm of Donkeys.		Reduced to 70°F
70°.2	+0^r.061	+0$^\mu$.40	+0$^\mu$.40
	Excess over 2nd cm.		
70.4	+0^r.107	+0.70	+0.70
	Total		+1.10

Decimetre Scale No 3.

1st and 2nd Centimetres				3rd and 4th Centimetres			
Temp	Excess		Reduced to 70°F	Temp	Excess		Reduced to 70°F
68°.5	+0^r.200	+1$^\mu$.30	+1$^\mu$.30	69°.4	+0^r.126	+0$^\mu$.82	+0$^\mu$.82
68.7	+0.207	+1.35	+1.35	68.6	+0.125	+0.82	+0.82
44.0	+0.190	+1.24	+1.29	44.9	+0.126	+0.82	+0.87
44.2	+0.219	+1.43	+1.48	44.0	+0.125	+0.82	+0.87
43.2	+0.205	+1.34	+1.39	44.3	+0.124	+0.81	+0.86
Means			+1.36				+0.85

5th and 6th Centimetres				7th and 8th Centimetres			
Temp	Excess		Reduced to 70°	Temp	Excess		Reduced to 70°F
72°.2	+0^r.051	+0$^\mu$.33	+0$^\mu$.33	72°.5	−0^r.063	−0$^\mu$.41	−0$^\mu$.41
71.9	+0.050	+0.33	+0.33	71.5	−0.067	−0.44	−0.44
45.2	+0.046	+0.30	+0.35	45.5	−0.068	−0.44	−0.39
43.7	+0.032	+0.21	+0.26	43.4	−0.093	−0.61	−0.55
46.0	+0.049	+0.32	+0.36	45.8	−0.073	−0.48	−0.43
Means			+0.33				−0.44

9th and 10th Centimetres

Temp	Excess		Reduced to 70°F
72°.4	+0r.020	+0μ.13	+0μ.13
71.3	+0.021	+0.14	+0.14
46.1	+0.011	+0.07	+0.11
42.6	−0.005	−0.03	+0.02
47.1	+0.006	+0.04	+0.08
Mean			+0.10

Comparisons of Decimetres.
Second Series.
Decimetre Scale No 3.

Temp	Excess		Reduced to 70°
76°.1	+0r.096	+0μ.63	+1μ.11
76.9	+0.086	+0.56	+1.10
82.3	+0.006	+0.04	+1.00
84.0	−0.012	−0.08	+1.02
74.5	+0.114	+0.74	+1.09
69.8	+0.149	+0.97	+0.95
69.9	+0.154	+1.00	+0.99
72.9	+0.128	+0.83	+1.06
72.0	+0.141	+0.92	+1.08
73.3	+0.122	+0.80	+1.06
74.8	+0.097	+0.63	+1.01
75.6	+0.100	+0.65	+1.09
76.0	+0.077	+0.50	+0.97
76.7	+0.063	+0.41	+0.92
70.9	+0.126	+0.82	+0.89
71.1	+0.137	+0.89	+0.98
76.7	+0.068	+0.44	+0.96
77.5	+0.064	+0.42	+1.01
77.8	+0.061	+0.40	+1.01
79.0	+0.063	+0.41	+1.11
Mean—			+1.01

Decimetre scale No 4.

Temp			
$77^{\circ}.5$	$+0^{\mathrm{r}}.059$	$+0^{\mu}.38$	$+0^{\mu}.37$
80.4	+0.065	+0.42	+0.41
81.4	+0.050	+0.33	+0.32
82.4	+0.067	+0.44	+0.43
71.0	+0.042	+0.27	+0.27
71.8	+0.051	+0.33	+0.33
72.9	+0.052	+0.34	+0.34
72.5	+0.050	+0.33	+0.33
75.8	+0.046	+0.30	+0.30
76.1	+0.060	+0.39	+0.39
76.8	+0.060	+0.39	+0.38
77.4	+0.057	+0.37	+0.36
77.9	+0.053	+0.35	+0.34
74.9	+0.050	+0.33	+0.33
75.8	+0.052	+0.34	+0.34
76.2	+0.049	+0.32	+0.32
77.9	+0.056	+0.37	+0.36
76.5	+0.064	+0.42	+0.41
77.6	+0.050	+0.33	+0.33

Excesses over Decimetre Donkeys in Third Series.
Decimetre scale No 3.

Temp	Excess		Reduced to 70°
$40^{\circ}.1$	$+0^{\mathrm{r}}.534$	$+3^{\mu}.48$	$+1^{\mu}.14$
44.2	+0.486	+3.17	+1.15
43.9	+0.484	+2.86	+0.82
39.5	+0.530	+3.46	+1.07
44.4	+0.474	+2.89	+0.89
41.0	+0.515	+3.36	+1.09
42.8	+0.498	+3.25	+1.12
45.7	+0.457	+2.98	+1.08
46.5	+0.455	+2.97	+1.13
46.5	+0.450	+2.93	+1.09

Decimetre Scale No 4.

Temp			
$43^{\circ}.8$	$+0^{r}.059$	$+0^{\mu}.38$	$+0^{\mu}.40$
49.1	+0.074	+0.48	+0.50
48.5	+0.070	+0.46	+0.48
43.9	+0.045	+0.29	+0.31
40.4	+0.034	+0.22	+0.24
41.3	+0.041	+0.27	+0.29
44.5	+0.058	+0.38	+0.40
40.1	+0.040	+0.26	+0.28
40.9	+0.035	+0.23	+0.25

These measures are checked in several ways. We have in two ways the

Excess of Pairs of Centimetres
of Decimetre Scale No 3
over their mean.

Cm	1st Series	2nd Series
1st and 2nd	$+0^{\mu}.95$	$+0^{\mu}.92$
3rd and 4th	+0.40	+0.41
5th and 6th	−0.13	−0.11
7th and 8th	−0.89	−0.88
9th and 10th	−0.34	−0.34

We have also in two ways the

Excess of Pairs of Centimetres of
Decimetre Scale No 4, over mean.

Cm	Addition of Cms.	Comp. of double cms.
1st and 2nd	$-0^{\mu}.14$	$-0^{\mu}.14$
3rd and 4th	−0.47	−0.44
5th and 6th	−0.03	−0.05
7th and 8th	+0.14	+0.14
9th and 10th	+0.50	+0.51

The two gitters H and I were also twice compared as follows:—

	1st Series	2nd Series
Gitter I—Gitter H	$+2^{\mu}.18$	$+2^{\mu}.19$

The two decimetre scales Nos. 3 and 4 were twice compared as follows

	By addition of Double cm	By total comparison
Decim 3–Decim 4	$+0\overset{\mu}{.}72$	$+0\overset{\mu}{.}67$

Inasmuch as in every one of these cases one or other term of the comparison is far below the standard strength of the work, either from consisting of a single one or few comparisons or from comparisons at very low temperatures or from resulting from the addition of a number of terms, their agreement is I think all that need be desired.

The coëfficients of expansion are intended to be determined when the work is complete, 1st, by the deviations, 2nd, by measurements on a Fizeau's apparatus, and 3rd, by an absolute determination of the coëfficient of the glass metre with the apparatus described in my Report on Gravity at Initial Stations. The Fizeau apparatus has not been obtained as yet; and though the coëfficient of expansion of the glass metre is well-determined by the careful comparisons with Metre 49 in the Office of Weights and Measures, yet this is of little service owing to the very imperfect comparison of the glass metre with the decimetre. At present, therefore, the coëfficients of expansion depend entirely on the measures of deviation.

The five double centimetres of Decimetre Scale No. 3 were compared with a donkey combination in January 1880, twice near 70° F and three times near 45° F. These comparisons give an accurate value of the relative expansion of No. 3 on these donkeys. Gitters H and I were compared at the same time; and these gitters with the double centimetres of Scale No. 3 were also compared at about 75° F with a donkey combination, formed of the same scales as in the winter. The comparison of the summer and winter comparisons gives the expansion of I and of scale No. 3 relatively to H, whose coëfficient is determined by the deviations. The donkeys having been differently set up in the two series of comparisons, their absolute lengths are not comparable, but the coëfficients of expansion remain the same. Decimetre scale No. 3 was, in the summer of 1879, compared with the decimetre donkeys at temperatures ranging from 70° to 84°. This gives the coëfficient of this donkey combination, and the comparison of Decimetre Scales Nos. 3 and 4 with this combination in summer and winter gives the expansion of No. 4. The single centimetres of No. 4 have been compared with No. 1 and the whole of No. 4 has been compared with all the decimetres of Glass metre No. 1 whose expansion is known by comparison with No. 49.

The following is a conspectus of the determinations of coëfficients

Coëfficients of Expansion
per degree F, expressed in millionths

Gitter H	+3.92
Gitter I—Gitter H	−0.085
Scale No 3—Gitter H	−0.435
Double cm. donkeys—Scale No 3	−0.095
Scale No 4—Scale No 3	+0.859
Dm. donkeys—Scale No 3	+0.782

Only a rough comparison has as yet been made between Decimetre Scale No. 4 and the decimetres of Glass Metre Scale No. 1. From this it appears that Decimetre No. 4 at 70° is $1^{\mu}.65$ longer than the mean decimetre of Glass metre scale No. 1 and is therefore $27^{\mu}.70$ longer than a decimetre.

Decimetre scale No. 4 has also been roughly compared with a brass decimetre, dated January 1880, which has been investigated by Mr. Schott. No. 4 is found to be $19^{\mu}.98$ longer than the brass decimetre at $55^{\circ}.9$. But the brass decimetre is standard at $53^{\circ}.62$ and its coëfficient of expansion is 9.84 per degree F. Assuming the decimetre No. 4 to have a coëfficient of expansion of 4.34 it follows that this decimetre at 70° is too long by $28^{\mu}.34$.

The mean of these results may be provisionally accepted.

On the Logic of Number

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Nobody can doubt the elementary propositions concerning number: those that are not at first sight manifestly true are rendered so by the usual demonstrations. But although we see they *are* true, we do not so easily see precisely *why* they are true; so that a renowned English logician has entertained a doubt as to whether they were true in all parts of the universe. The object of this paper is to show that they are strictly syllogistic consequences from a few primary propositions. The question of the logical origin of these latter, which I here regard as definitions, would require a separate discussion. In my proofs I am obliged to make use of the logic of relatives, in which the forms of inference are not, in a narrow sense, reducible to ordinary syllogism. They are, however, of that same nature, being merely syllogisms in which the objects spoken of are pairs or triplets. Their validity depends upon no conditions other than those of the validity of simple syllogism, unless it be that they suppose the existence of singulars, while syllogism does not.

The selection of propositions which I have proved will, I trust, be sufficient to show that all others might be proved with like methods.

Let r be any relative term, so that one thing may be said to be r of another, and the latter r'd by the former. If in a certain system of objects, whatever is r of an r of anything is itself r of that thing, then r is said to be a transitive relative in that system. (Such relatives as "lover of everything loved by ——— " are transitive relatives.) In a system in which r is transitive, let the q's of anything include that thing itself, and also every r of it which is not r'd by it. Then q may be called a fundamental relative of quantity; its properties being, first, that it is transitive; second, that everything in the system is q

of itself, and, third, that nothing is both q of and q'd by anything except itself. The objects of a system having a fundamental relation of quantity are called quantities, and the system is called a system of quantity.

A system in which quantities may be q's of or q'd by the same quantity without being either q's of or q'd by each other is called multiple;[1] a system in which of every two quantities one is a q of the other is termed simple.

Simple Quantity.

In a simple system every quantity is either "as great as" or "as small as" every other; whatever is as great as something as great as a third is itself as great as that third, and no quantity is at once as great as and as small as anything except itself.

A system of simple quantity is either continuous, discrete, or mixed. A continuous system is one in which every quantity greater than another is also greater than some intermediate quantity greater than that other. A discrete system is one in which every quantity greater than another is next greater than some quantity (that is, greater than without being greater than something greater than). A mixed system is one in which some quantities greater than others are next greater than some quantities, while some are continuously greater than some quantities.

Discrete Quantity.

A simple system of discrete quantity is either limited, semi-limited, or unlimited. A limited system is one which has an absolute maximum and an absolute minimum quantity; a semi-limited system has one (generally considered a minimum) without the other; an unlimited has neither.

A simple, discrete, system, unlimited in the direction of increase or decrement, is in that direction either infinite or super-infinite. An infinite system is one in which any quantity greater than x can be reached from x by successive steps to the next greater (or less) quantity than the one already arrived at. In other words, an infinite, discrete, simple, system is one in which, if the quantity next greater

1. For example, in the ordinary algebra of imaginaries two quantities may both result from the addition of quantities of the form $a^2 + b^2 i$ to the same quantity without either being in this relation to the other.

than an attained quantity is itself attained, then any quantity greater than an attained quantity is attained; and by the class of attained quantities is meant any class whatever which satisfies these conditions. So that we may say that an infinite class is one in which if it is true that every quantity next greater than a quantity of a given class itself belongs to that class, then it is true that every quantity greater than a quantity of that class belongs to that class. Let the class of numbers in question be the numbers of which a certain proposition holds true. Then, an infinite system may be defined as one in which from the fact that a certain proposition, if true of any number, is true of the next greater, it may be inferred that that proposition if true of any number is true of every greater.

Of a super-infinite system this proposition, in its numerous forms, is untrue.

Semi-infinite Quantity.

We now proceed to study the fundamental propositions of semi-infinite, discrete, and simple quantity, which is ordinary number.

Definitions.

The minimum number is called one.

By $x + y$ is meant, in case $x = 1$, the number next greater than y; and in other cases, the number next greater than $x' + y$, where x' is the number next smaller than x.

By $x \times y$ is meant, in case $x = 1$, the number y, and in other cases $y + x'y$, where x' is the number next smaller than x.

It may be remarked that the symbols $+$ and $\times$ are triple relatives, their two correlates being placed one before and the other after the symbols themselves.

Theorems.

The proof in each case will consist in showing, 1^{st}, that the proposition is true of the number one, and 2^{nd}, that if true of the number n it is true of the number $1 + n$, next larger than n. The different transformations of each expression will be ranged under one another in one column, with the indications of the principles of transformation in another column.

1. To prove the associative principle of addition, that

$$(x+y)+z = x+(y+z)$$

whatever numbers x, y, and z, may be. First it is true for $x = 1$; for
$(1+y)+z$
$= 1+(y+z)$ by the definition of addition, 2nd clause. Second, if true for $x = n$, it is true for $x = 1+n$; that is, if $(n+y)+z = n+(y+z)$ then $((1+n)+y)+z = (1+n)+(y+z)$. For

$$\begin{aligned} &((1+n)+y)+z \\ &= (1+(n+y))+z && \text{by the definition of addition:} \\ &= 1+((n+y)+z) && \text{by the definition of addition:} \\ &= 1+(n+(y+z)) && \text{by hypothesis:} \\ &= (1+n)+(y+z) && \text{by the definition of addition.} \end{aligned}$$

2. To prove the commutative principle of addition that

$$x+y = y+x$$

whatever numbers x and y may be. First, it is true for $x = 1$ and $y = 1$, being in that case an explicit identity. Second, if true for $x = n$ and $y = 1$, it is true for $x = 1+n$ and $y = 1$. That is, if $n+1 = 1+n$, then $(1+n)+1 = 1+(1+n)$. For $(1+n)+1$

$$\begin{aligned} &= 1+(n+1) && \text{by the associative principle:} \\ &= 1+(1+n) && \text{by hypothesis.} \end{aligned}$$

We have thus proved that, whatever number x may be, $x+1 = 1+x$, or that $x+y = y+x$ for $y = 1$. It is now to be shown that if this be true for $y = n$, it is true for $y = 1+n$; that is, that if $x+n = n+x$, then $x+(1+n) = (1+n)+x$. Now,

$$\begin{aligned} &x+(1+n) \\ &= (x+1)+n && \text{by the associative principle:} \\ &= (1+x)+n && \text{as just seen:} \\ &= 1+(x+n) && \text{by the definition of addition:} \\ &= 1+(n+x) && \text{by hypothesis:} \\ &= (1+n)+x && \text{by the definition of addition.} \end{aligned}$$

Thus the proof is complete.

3. To prove the distributive principle, first clause. The distributive principle consists of two propositions:

$$\begin{aligned} &1^{\text{st}}, && (x+y)z = xz+yz \\ &2^{\text{nd}}, && x(y+z) = xy+xz. \end{aligned}$$

We now undertake to prove the first of these. First, it is true for $x = 1$. For

$$\begin{aligned}
&(1 + y)z && \\
&= z + yz && \text{by the definition of multiplication:}\\
&= 1 \cdot z + yz && \text{by the definition of multiplication.}
\end{aligned}$$

Second, if true for $x = n$, it is true for $x = 1 + n$; that is, if $(n + y)z = nz + yz$, then $((1 + n) + y)z = (1 + n)z + yz$. For

$$\begin{aligned}
&((1 + n) + y)z && \\
&= (1 + (n + y))z && \text{by the definition of addition:}\\
&= z + (n + y)z && \text{by the definition of multiplication:}\\
&= z + (nz + yz) && \text{by hypothesis:}\\
&= (z + nz) + yz && \text{by the associative principle of addition:}\\
&= (1 + n)z + yz && \text{by the definition of multiplication.}
\end{aligned}$$

4. To prove the second proposition of the distributive principle, that

$$x(y + z) = xy + xz.$$

First, it is true for $x = 1$; for

$$\begin{aligned}
&1\,(y + z) && \\
&= y + z && \text{by the definition of multiplication:}\\
&= 1y + 1z && \text{by the definition of multiplication.}
\end{aligned}$$

Second, if true for $x = n$, it is true for $x = 1 + n$; that is, if $n(y + z) = ny + nz$, then $(1 + n)(y + z) = (1 + n)y + (1 + n)z$. For

$$\begin{aligned}
&(1 + n)(y + z) && \\
&= (y + z) + n(y + z) && \text{by the definition of multiplication:}\\
&= (y + z) + (ny + nz) && \text{by hypothesis:}\\
&= (y + ny) + (z + nz) && \text{by the principles of addition:}\\
&= (1 + n)y + (1 + n)z && \text{by the definition of multiplication.}
\end{aligned}$$

5. To prove the associative principle of multiplication; that is, that

$$(xy)z = x(yz),$$

whatever numbers x, y, and z, may be. First, it is true for $x = 1$, for

$$\begin{aligned}
&(1y)z && \\
&= yz && \text{by the definition of multiplication:}\\
&= 1 \cdot yz && \text{by the definition of multiplication.}
\end{aligned}$$

Second, if true for $x = n$, it is true for $x = 1 + n$; that is, if $(ny)z = n(yz)$, then $((1 + n)y)z = (1 + n)(yz)$. For

$$\begin{aligned}
&((1 + n)y)z && \\
&= (y + ny)z && \text{by the definition of multiplication:}\\
&= yz + (ny)z && \text{by the distributive principle:}\\
&= yz + n(yz) && \text{by hypothesis:}\\
&= (1 + n)(yz) && \text{by the definition of multiplication.}
\end{aligned}$$

6. To prove the commutative principle of multiplication; that

$$xy = yx,$$

whatever numbers x and y may be. In the first place, we prove that it is true for $y = 1$. For this purpose, we first show that it is true for $y = 1$, $x = 1$; and then that if true for $y = 1$, $x = n$, it is true for $y = 1$, $x = 1 + n$. For $y = 1$ and $x = 1$, it is an explicit identity. We have now to show that if $n1 = 1n$ then $(1 + n)1 = 1(1 + n)$. Now,

$(1 + n)1$	
$= 1 + n1$	by the definition of multiplication:
$= 1 + 1n$	by hypothesis:
$= 1 + n$	by the definition of multiplication:
$= 1(1 + n)$	by the definition of multiplication.

Having thus shown the commutative principle to be true for $y = 1$, we proceed to prove that if it is true for $y = n$, it is true for $y = 1 + n$; that is, if $xn = nx$, then $x(1 + n) = (1 + n)x$. For

$(1 + n)x$	
$= x + nx$	by the definition of multiplication:
$= x + xn$	by hypothesis:
$= 1x + xn$	by the definition of multiplication:
$= x1 + xn$	as already seen:
$= x(1 + n)$	by the distributive principle.

Discrete Simple Quantity Infinite in both directions.

A system of number infinite in both directions has no minimum, but a certain quantity is called *one,* and the numbers as great as this constitute a partial system of semi-infinite number, of which this one is a minimum. The definitions of addition and multiplication require no change, except that the *one* therein is to be understood in the new sense.

To extend the proofs of the principles of addition and multiplication to unlimited number, it is necessary to show that if true for any number $(1 + n)$ they are also true for the next smaller number n. For this purpose we can use the same transformations as in the second clauses of the former proof; only we shall have to make use of the following lemma.

If $x + y = x + z$, then $y = z$ whatever numbers x, y, and z, may be. First this is true in case $x = 1$, for then y and z are both next smaller than the same number. Therefore, neither is smaller than the other, otherwise it would not be next smaller to $1 + y = 1 + z$. But in a simple system, of any two different numbers one is smaller. Hence, y and z are equal. Second, if the proposition is true for $x = n$, it is true for $x = 1 + n$. For if $(1 + n) + y = (1 + n) + z$, then by the definition of addition $1 + (n + y) = 1 + (n + z)$; whence it would follow that $n + y = n + z$, and, by hypothesis, that $y = z$. Third, if the proposition is true for $x = 1 + n$, it is true for $x = n$. For if $n + y = n + z$, then $1 + n + y = 1 + n + z$, because the system is simple. The proposition has thus been proved to be true of 1, of every greater and of every smaller number, and therefore to be universally true.

An inspection of the above proofs of the principles of addition and multiplication for semi-infinite number will show that they are readily extended to doubly infinite number by means of the proposition just proved.

The number next smaller than one is called naught, 0. This definition in symbolic form is $1 + 0 = 1$. To prove that $x + 0 = x$, let x' be the number next smaller than x. Then,

$x + 0$	
$= (1 + x') + 0$	by the definition of x':
$= (1 + 0) + x'$	by the principles of addition:
$= 1 + x'$	by the definition of naught:
$= x$	by the definition of x'.

To prove that $x0 = 0$. First, in case $x = 1$, the proposition holds by the definition of multiplication. Next, if true for $x = n$, it is true for $x = 1 + n$. For

$(1 + n)0$	
$= 1 \cdot 0 + n \cdot 0$	by the distributive principle:
$= 1 \cdot 0 + 0$	by hypothesis:
$= 1 \cdot 0$	by the last theorem:
$= 0$	as above.

Third, the proposition, if true for $x = 1 + n$ is true for $x = n$. For, changing the order of the transformations,

$$1 \cdot 0 + 0 = 1 \cdot 0 = 0 = (1 + n)0 = 1 \cdot 0 + n \cdot 0.$$

Then by the above lemma, $n \cdot 0 = 0$, so that the proposition is proved.

A number which added to another gives naught is called the negative of the latter. To prove that every number greater than naught has a negative. First, the number next smaller than naught is the negative of one; for, by the definition of addition, one plus this number is naught. Second, if any number n has a negative, then the number next greater than n has for its negative the number next smaller than the negative of n. For let m be the number next smaller than the negative of n. Then $n + (1 + m) = 0$. But

$n + (1 + m)$	
$= (n + 1) + m$	by the associative principle of addition:
$= (1 + n) + m$	by the commutative principle of addition.

So that $(1 + n) + m = 0$. *Q. E. D.* Hence, every number greater than 0 has a negative, and naught is its own negative.

To prove that $(-x)y = -(xy)$. We have

$0 = x + (-x)$	by the definition of the negative:
$0 = 0y = (x + (-x))y$	by the last proposition but one:
$0 = xy + (-x)y$	by the distributive principle:
$-(xy) = (-x)y$	by the definition of the negative.

The negative of the negative of a number is that number. For $x + (-x) = 0$. Whence by the definition of the negative $x = -(-x)$.

Limited Discrete Simple Quantity.

Let such a relative term, c, that whatever is a c of anything is the only c of that thing, and is a c of that thing only, be called a relative of simple correspondence. In the notation of the logic of relatives,

$$c\breve{c} \prec 1, \qquad \breve{c}c \prec 1.$$

If every object, s, of a class is in any such relation, being c'd by a number of a semi-infinite discrete simple system, and if, further, every number smaller than a number c of an s is itself c of an s, then the numbers c of the s's are said to count them, and the system of correspondence is called a count. In logical notation, putting g for as great as, and n for a positive integral number,

$$s \prec \breve{c}n \qquad \breve{g}cs \prec cs.$$

If in any count there is a maximum counting number, the count is said to be finite, and that number is called the number of the count. Let $[s]$ denote the number of a count of the s's, then

$$[s] \prec cs \qquad \bar{g}cs \prec \overline{[s]}.$$

The relative "identical with" satisfies the definition of a relative of simple correspondence, and the definition of a count is satisfied by putting "identical with" for c, and "positive integral number as small as x" for s. In this mode of counting, the number of numbers as small as x is x.

Suppose that in any count the number of numbers as small as the minimum number, one, is found to be n. Then, by the definition of a count, every number as small as n counts a number as small as one. But, by the definition of one there is only one number as small as one. Hence, by the definition of single correspondence, no other number than one counts one. Hence, by the definition of one, no other number than one counts any number as small as one. Hence, by the definition of the count, one is, in every count, the number of numbers as small as one.

If the number of numbers as small as x is in some count y, then the number of numbers as small as y is in some count x. For if the definition of a simple correspondence is satisfied by the relative c, it is equally satisfied by the relative c'd by.

Since the number of numbers as small as x is in some count y, we have, c being some relative of simple correspondence,

1st. Every number as small as x is c'd by a number.

2nd. Every number as small as a number that is c of a number as small as x is itself c of a number as small as x.

3rd. The number y is c of a number as small as x.

4th. Whatever is not as great as a number that is c of a number as small as x is not y.

Now let c_1 be the converse of c. Then the converse of c_1 is c; whence, since c satisfies the definition of a relative of simple correspondence, so also does c_1. By the 3rd proposition above, every number as small as y is as small as a number that is c of a number as small as x. Whence, by the 2nd proposition, every number as small as y is c of a number as small as x; and it follows that every number as small as y is c_1'd by a number. It follows further that every number c_1 of a number as small as y is c_1 of something c_1'd by (that is, c_1 being a relative of simple correspondence, is identical with) some number as small as x. Also, "as small as" being a transitive relative, every number as small as a number c of a number as small as y is as small as x. Now by the 4th proposition y is as great as any number that is c of a number as small as x, so that what is not as small as y is not c of a number as small as x; whence whatever number is c'd by a number not as small as y is not a number as small as x. But by the 2nd proposition every number as small as x not c'd by a number not as small as y is c'd by a number as small as y. Hence, every number as small as x is c'd by a number as small as y. Hence, every number as small as a number c_1 of a number as small as y is c_1 of a number as small as y. Moreover, since we have shown that every number as small as x is c_1 of a number as small as y, the same is true of x itself. Moreover, since we have seen that whatever is c_1 of a number as small as y is as small as x, it follows that whatever is not as great as a number c_1 of a number as small as y is not as great as a number as small as x; *i. e.* ("as great as" being a transitive relative) is not as great as x, and consequently is not x. We have now shown—

1st, that every number as small as y is c_1'd by a number;

2nd, that every number as small as a number that is c_1 of a number as small as y is itself c_1 of a number as small as y;

3rd, that the number x is c_1 of a number as small as y; and

4th, that whatever is not as great as a number that is c_1 of a number as small as y is not x.

These four propositions taken together satisfy the definition of the number of numbers as small as y counting up to x.

Hence, since the number of numbers as small as one cannot in any count be greater than one, it follows that the number of numbers as small as any number greater than one cannot in any count be one.

Suppose that there is a count in which the number of numbers as small as $1 + m$ is found to be $1 + n$, since we have just seen that it cannot be 1. In this count, let m' be the number which is c of $1 + n$, and let n' be the number which is c'd by $1 + m$. Let us now consider a relative, e, which differs from c only in excluding the relation of m' to $1 + n$ as well as the relation of $1 + m$ to n' and in including the relation of m' to n'. Then e will be a relative of single correspondence; for c is so, and no exclusion of relations from a single correspondence affects this character, while the inclusion of the relation of m' to n' leaves m' the only e of n' and an e of n' only. Moreover, every number as small as m is e of a number, since every number except $1 + m$ that is c of anything is e of something, and every number except $1 + m$ that is as small as $1 + m$ is as small as m. Also, every number as small as a number e'd by a number is itself e'd by a number; for every number c'd is e'd except $1 + m$, and this is greater than any number e'd. It follows that e is the basis of a mode of counting by which the numbers as small as m count up to n. Thus we have shown that if in any way $1 + m$ counts up to $1 + n$, then in some way m counts up to n. But we have already seen that for $x = 1$ the number of numbers as small as x can in no way count up to other than x. Whence it follows that the same is true whatever the value of x.

If every S is a P, and if the P's are a finite lot counting up to a number as small as the number of S's, then every P is an S. For if, in counting the P's, we begin with the S's (which are a part of them), and having counted all the S's arrive at the number n, there will remain over no P's not S's. For if there were any, the number of P's would count up to more than n. From this we deduce the validity of the following mode of inference:

> Every Texan kills a Texan,
> Nobody is killed but by one person,
> Hence, every Texan is killed by a Texan,

supposing Texans to be a finite lot. For, by the first premise, every Texan killed by a Texan is a Texan killer of a Texan. By the second premise, the Texans killed by Texans are as many as the Texan killers of Texans. Whence we conclude that every Texan killer of a Texan is a Texan killed by a Texan, or, by the first premise, every Texan is killed by a Texan. This mode of reasoning is frequent in the theory of numbers.

NOTE.—It may be remarked that when we reason that a certain proposition, if false of any number, is false of some smaller number, and since there is no number (in a semi-limited system) smaller than every number, the proposition must be true, our reasoning is a mere logical transformation of the reasoning that a proposition, if true for n, is true for $1 + n$, and that it is true for 1.

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[NOTES AND ADDENDA TO *LINEAR ASSOCIATIVE ALGEBRA*]

[Note on the Algebra g_4]

P 188: American Journal of Mathematics 4 (1881): 132

In relative form, $i = A:A$, $j = A:B$, $k = B:A$, $l = B:B$. This algebra exhibits the general system of relationship of individual relatives, as is shown in my paper in the ninth volume of the *Memoirs of the American Academy of Arts and Sciences.* In a space of four dimensions, a vector may be determined by means of its rectangular projections on two planes such that every line in the one is perpendicular to every line in the other. Call these planes the A-plane and the B-plane, and let v be any vector. Then, iv is the projection of v upon the A-plane, and lv is its projection upon the B-plane. Let each direction in the A-plane be considered as to correspond to a direction in the B-plane in such a way that the angle between two directions in the A-plane is equal to the angle between the corresponding directions in the B-plane. Then, jv is that vector in the A-plane which corresponds to the projection of v upon the B-plane, and kv is that vector in the B-plane which corresponds to the projection of v upon the A-plane.

Professor Peirce showed that we may take i_1, j_1, k_1, as three such mutually perpendicular vectors in ordinary space, that $i = \frac{1}{2}(1 - \mathfrak{J}i_1)$, $j = \frac{1}{2}(j_1 - \mathfrak{J}k_1)$, $k = \frac{1}{2}(-j_1 - \mathfrak{J}k_1)$, $l = \frac{1}{2}(1 + \mathfrak{J}i_1)$. [See, also, Spottiswoode, *Proceedings of the London Mathematical Society,* iv, 156. Cayley, in his "Memoir on the Theory of Matrices" (1858), had shown how a quaternion may be represented by a dual matrix.] Thus $i, j, k, l,$ have all zero tensors, and j and k are vectors. In the general expression of the algebra, $q = xi + yj + zk + wl$, if $x + w = 1$ and $yz = x - x^2$, we have $q^2 = q$; if $x = -w = \sqrt{-yz}$, then $q^2 = 0$. The expression $i + l$ represents scalar unity, since it is the universal idemfactor. We have, also, $Sq = \frac{1}{2}(x + w)(i + l)$, $Vq = \frac{1}{2}(x - w)i + yj + zk + \frac{1}{2}(w - x)l$, $Tq = \sqrt{xw - yz}(i + l)$.

The resemblance of the multiplication table of this algebra to the symbolical table of §46 merits attention.

[Note on the Class of Algebras 242^3]

P 188: 190-94

It is not easy to see how the author proves that $e_3 = 0$. But it can be proved thus. $0 = k^3 = (a_3 i + b_3 j + d_3 l)k = a_3 l + a_3 d_3 j$.

The algebras of the case [242^3] are those quintuple systems in which every product containing j or l as a factor vanishes, while every product which does not vanish is a linear function of j and l. Any multiplication table conforming to these conditions is self-consistent, but it is a matter of some trouble to exclude every case of a *mixed* algebra. An algebra of the class in question is separable, if all products are similar. But this case requires no special attention; and the only other is when two dissimilar expressions U and V can be found, such that both being linear functions of i, k and m, $UV = VU = 0$. It will be convenient to consider separately, first, the conditions under which $UV - VU = 0$, and, secondly, those under which $UV + VU = 0$. To bring the subjects under a familiar form, we may conceive of i, k, m as three vectors not coplanar, so that, writing

$$U = xi + yk + zm, \qquad V = x'i + y'k + z'm,$$

we have x, y, z, and x', y', z', the Cartesian coördinates of two points in space. [We might imagine the space to be of the hyperbolic kind, and take the coëfficients of j and l as coördinates of a point on the quadric surface at infinity. But this would not further the purpose with which we now introduce geometric conceptions.] But since we are to consider only such properties of U and V as belong equally to all their numerical multiples, we may assume that they always lie in any plane

$$Ax + By + Cz = 1,$$

not passing through the origin; and then x, y, z, and x', y', z', will be the homogeneous coördinates of the two points U and V in that plane. Let it be remembered that, although i, k, m are vectors, yet their multiplication does not at all follow the rule of quaternions, but that

$$\begin{array}{lll} i^2 = b_1 j + d_1 l, & ik = b_{13} j + d_{13} l, & im = b_{15} j + d_{15} l, \\ ki = b_{31} j + d_{31} l, & k^2 = b_3 j + d_3 l, & km = b_{35} j + d_{35} l, \\ mi = b_{51} j + d_{51} l, & mk = b_{53} j + d_{53} l, & m^2 = b_5 j + d_5 l. \end{array}$$

The condition that $UV - VU = 0$ is expressed by the equations

$$(b_{13} - b_{31})(xy' - x'y) + (b_{15} - b_{51})(xz' - x'z) + (b_{35} - b_{53})(yz' - y'z) = 0,$$
$$(d_{13} - d_{31})(xy' - x'y) + (d_{15} - d_{51})(xz' - x'z) + (d_{35} - d_{53})(yz' - y'z) = 0.$$

The first equation evidently signifies that for every value of U, V must be on a straight line, that this line passes through U, and that it also passes through the point

$$P = (b_{35} - b_{53})i + (b_{51} - b_{15})k + (b_{13} - b_{31})m.$$

The second equation expresses that the line between U and V contains the point

$$Q = (d_{35} - d_{53})i + (d_{51} - d_{15})k + (d_{13} - d_{31})m.$$

The two equations together signify, therefore, that U and V may be any two points on the line between the fixed points P and Q. Linear transformations of j and l may shift P and Q to any other situations on the line joining them, but cannot turn the line nor bring the two points into coincidence.

The condition that $UV + VU = 0$ is expressed by the equations

$$\begin{aligned} &2b_1 xx' + 2b_3 yy' + 2b_5 zz' + (b_{13} + b_{31})(xy' + x'y) \\ &+ (b_{15} + b_{51})(xz' + x'z) + (b_{35} + b_{53})(yz' + y'z) = 0, \\ &2d_1 xx' + 2d_3 yy' + 2d_5 zz' + (d_{13} + d_{31})(xy' + x'y) \\ &+ (d_{15} + d_{51})(xz' + x'z) + (d_{35} + d_{53})(yz' + y'z) = 0. \end{aligned}$$

The first of these evidently signifies that for any position of V the locus of U is a line; that U being fixed at any point on that line, V may be carried to any position on a line passing through its original position; and that further, if U is at one of the two points where its line cuts the conic

$$b_1x^2 + b_3y^2 + b_5z^2 + (b_{13} + b_{31})xy + (b_{15} + b_{51})xz + (b_{35} + b_{53})yz = 0,$$

then V may be at an infinitely neighboring point on the same conic, so that tangents to the conic from V cut the locus of U at their points of tangency. The second equation shows that the points U and V have the same relation to the conic

$$d_1x^2 + d_3y^2 + d_5z^2 + (d_{13} + d_{31})xy + (d_{15} + d_{51})xz + (d_{35} + d_{53})yz = 0.$$

These conics are the loci of points whose squares contain respectively no term in j and no term in l. Their four intersections represent expressions whose squares vanish. Hence, linear transformations of j and l will change these conics to any others of the sheaf passing through these four fixed points. The two equations together, then, signify that through the four fixed points, two conics can be drawn tangent at U and V to the line joining these last points.

Uniting the conditions of $UV - VU = 0$ and $UV + VU = 0$, they signify that U and V are on the line joining P and Q at those points at which this line is tangent to conics through the four fixed points whose squares vanish. But if the algebra is pure, it is impossible to find two such points; so that the line between P and Q must pass through one of the four fixed points. In other words, the necessary condition of the algebra being pure is that one and only one nilpotent expression in i, k, m, should be a linear function of P and Q.

The two points P and Q together with the two conics completely determine all the constants of the multiplication table. Let S and T be the points at which the two conics separately intersect the line between P and Q. A linear transformation of j will move P to the point $pP + (1 - p)Q$ and will move S to the point $pS + (1 - p)T$, and a linear transformation of l will move Q and T in a similar way. The points P and S may thus be brought into coincidence, and the point Q may be brought to the common point of intersection of the two conics with the line from P to Q. The geometrical figure determining the algebra is thus reduced to a first and a second conic and a straight line having one common intersection. This figure will have special varieties due to the coincidence of different intersections, etc.

There are six cases: [1], there is a line of quantities whose squares vanish and one quantity out of the line; [2], there are four dissimilar quantities whose squares vanish; [3], two of these four quantities coincide; [4], two pairs of the four quantities coincide; [5], three of the four quantities coincide; [6], all the quantities coincide.

We may, in every case, suppose the equation of the plane to be $x + y + z = 1$.

[1]. In this case, the line common to the two conics may be taken as $y = 0$, and the separate lines of the conics as $z = 0$ and $x = 0$, respectively. We may also assume $2P = x + y$ and $2Q = x + z$. We thus obtain the following multiplication table, where the rows and columns having j and l as their arguments are omitted:

	i	k	m
i	0	$3l$	$-j$
k	$-l$	0	$3j+l$
m	j	$l-j$	0

[2]. In this case, we may take k as the common intersection of the two conics and the line, i, m, and $i - k + m$ as the other intersections of the conics. We have $Q = k$, and we may write

$$P = S = pi + (1 - p - q)k + qm,$$
$$T = rP + (1 - r)Q = rpi + (1 - rp - rq)k + rqm.$$

We thus obtain the following multiplication table:

	i	k	m
i	0	$q(q+1)j + rq(rq-1)l$	$[-2 - p(p-3) + q(q+1)]j$ $+ [-2 - rp(rp-1) + rq(rq-1)]l$
k	$q(q-3)j + rq(rq-1)l$	0	$-p(p-3)j - rp(rp-1)l$
m	$[2 - p(p+1) + q(q-3)]j +$ $[2 - rp(rp-1) + rq(rq-1)]l$	$-p(p+1)j - rp(rp-1)l$	0

[3]. Let k be the double point common to the two conics, and let i and m be their other intersections. Then all expressions of the form $ku + uk$ are similar. The line between P and Q cannot pass through k, because in that case all products would be similar. We may therefore assume that it passes through i. Then, we have $Q = i$, we may assume $S = P = i - k + m$, and we may write $T = rP + (1 - r)Q = i - rk + rm$. The equation of the common tangent to the conics at k may be written $hx + (1 - h)z = 0$. Then the equations of the two conics are

$$hxy + xz + (1 - h)yz = 0,$$
$$hxy + (h + r - hr)xz + (1 - h)yz = 0.$$

We thus obtain the following multiplication table:

	i	k	m
i	0	$(h+1)j+(h+r)l$	$2j+[h(1-r)+2r]l$
k	$(h-1)j+(h-r)l$	0	$(2-h)(j+l)$
m	$h(1-r)l$	$-h(j+l)$	0

[4]. In this case we may take i and m as the two points of contact of the conics, k as P, and $i-k+m$ as T. Then writing the equations of the two tangents

$$gy+z=0, \qquad x+hy=0,$$

the two conics become

$$gxy+xz+hyz=0,$$
$$(g+h-1)y^2+gxy+xz+hyz=0,$$

and the multiplication table is as follows:

	i	k	m
i	$(g+h-1)l$	$gj+(g+1)l$	$2l$
k	$gj+(g-1)l$	0	$hj+(h+1)l$
m	$2j$	$hj+(h-1)l$	0

[5]. In this case, we may take k as the point of osculation of the conics and i as their point of intersection. The line between P and Q must either, [51], pass through k, or, [52], pass through i.

[51]. We may, without loss of generality, take

$$P=k, \qquad Q=m,$$

and the equations of the two conics are

$$z^2+rxz=0, \qquad rxy+2qxz+2yz=0.$$

Then, the multiplication table is as follows:

	i	k	m
i	0	0	ql
k	rl	0	l
m	$rj+ql$	l	j

[52]. We have $Q = i$, we may take $T = m$, and we may assume $P = 2i - m$ and $b_{13} + b_{31} = 1$. Then, we may write the equations of the two conics,

$$2z^2 + xy + xz + ryz = 0,$$
$$-rxy + (2 - r)xz + r^2yz = 0.$$

We thus obtain the following multiplication table:

	i	k	m
i	0	$-rl$	$j - (r - 2)l$
k	$2j - rl$	0	$(r - 2)j + (r^2 + 1)l$
m	$j - (r - 2)l$	$(r - 2)j + (r^2 - 1)l$	$2j$

[6]. The conics have but one point in common. This may be taken at k. We have $Q = k$, we may take $T = i$ and $2P = 2S = i + k$. We may also take $b_1 = -1$. Then the equations of the two conics may be written

$$-x^2 + pz^2 + 2xy + 4qxz + 2ryz = 0,$$
$$(4 + pr^2)z^2 + 2xy + 4(q + r)xz + 2ryz = 0.$$

We thus find this multiplication table:

	i	k	m
i	$-j$	$j + l$	$(2q - 1)j + 2(q + r - p)l$
k	$j + l$	0	$(r + 1)j + rl$
m	$(2q + 1)j + 2(q + r + p)l$	$(r - 1)j + rl$	$pj + (4 + pr^2)l$

If this analysis is correct, only three indeterminate coëfficients are required for the multiplication tables of this class of algebras.

On the Relative Forms of the Algebras

P 188: 221–25

Given an associative algebra whose letters are *i, j, k, l,* etc., and whose multiplication table is

$$\begin{aligned} i^2 &= a_{11}i + b_{11}j + c_{11}k + \text{etc.}^1 \\ ij &= a_{12}i + b_{12}j + c_{12}k + \text{etc.} \\ ji &= a_{21}i + b_{21}j + c_{21}k + \text{etc.}, \\ &\text{etc., etc.} \end{aligned}$$

I proceed to explain what I call the relative form of this algebra.

Let us assume a number of new units, *A, I, J, K, L,* etc., one more in number than the letters of the algebra, and every one except the first, *A*, corresponding to a particular letter of the algebra. These new units are susceptible of being multiplied by numerical coëfficients and of being added together;[2] but they cannot be multiplied together, and hence are called *non-relative* units.

Next, let us assume a number of operations each denoted by bracketing together two non-relative units separated by a colon. These operations, equal in number to the square of the number of non-relative units, may be arranged as follows:

$$\begin{array}{llll} (A:A) & (A:I) & (A:J) & (A:K), \text{ etc.} \\ (I:A) & (I:I) & (I:J) & (I:K), \text{ etc.} \\ (J:A) & (J:I) & (J:J) & (J:K), \text{ etc.} \\ \text{etc.} & & & \end{array}$$

Any one of these operations performed upon a polynomial in non-relative units, of which one term is a numerical multiple of the letter following the colon, gives the same multiple of the letter

1. I have used a_{11}, etc., in place of the a_1, etc., used by my father in his text.
2. Any one of them multiplied by 0 gives 0.

preceding the colon. Thus, $(I{:}J)(aI + bJ + cK) = bI$.[3] These operations are also taken to be susceptible of associative combination. Hence $(I{:}J)(J{:}K) = (I{:}K)$; for $(J{:}K)K = J$ and $(I{:}J)J = I$, so that $(I{:}J)(J{:}K)K = I$. And $(I{:}J)(K{:}L) = 0$; for $(K{:}L)L = K$ and $(I{:}J)K = (I{:}J)(0 \cdot J + K) = 0 \cdot I = 0$. We further assume the application of the distributive principle to these operations; so that, for example,

$$\{(I{:}J) + (K{:}J) + (K{:}L)\}(aJ + bL) = aI + (a + b)K.$$

Finally, let us assume a number of complex operations denoted by i', j', k', l', etc., corresponding to the letters of the algebra and determined by its multiplication table in the following manner:

$$\begin{aligned} i' = (I{:}A) &+ a_{11}(I{:}I) + b_{11}(J{:}I) + c_{11}(K{:}I) + \text{etc.} \\ &+ a_{12}(I{:}J) + b_{12}(J{:}J) + c_{12}(K{:}J) + \text{etc.} \\ &+ a_{13}(I{:}K) + b_{13}(J{:}K) + c_{13}(K{:}K) + \text{etc.} + \text{etc.} \\ j' = (J{:}A) &+ a_{21}(I{:}I) + b_{21}(J{:}I) + c_{21}(K{:}I) + \text{etc.} \\ &+ a_{22}(I{:}J) + b_{22}(J{:}J) + c_{22}(K{:}J) + \text{etc.} \\ &+ a_{23}(I{:}K) + b_{23}(J{:}K) + c_{23}(K{:}K) + \text{etc.} + \text{etc.} \\ k' = \text{etc.} & \end{aligned}$$

Any two operations are equal which, being performed on the same operand, invariably give the same result. The ultimate operands in this case are the non-relative units. But any operations compounded by addition or multiplication of the operations i', j', k', etc., if they give the same result when performed upon A, will give the same result when performed upon any one of the non-relative units. For suppose $i'j'A = k'l'A$. We have

$$\begin{aligned} i'j'A = i'J &= a_{12}I + b_{12}J + c_{12}K + \text{etc.} \\ k'l'A = k'L &= a_{34}I + b_{34}J + c_{34}K + \text{etc.} \end{aligned}$$

so that $a_{12} = a_{34}$, $b_{12} = b_{34}$, $c_{12} = c_{34}$, etc., and in our original algebra $ij = kl$. Hence, multiplying both sides of the equation into any letter, say m, $ijm = klm$. But

$$\begin{aligned} ijm = i(a_{25}i + b_{25}j + c_{25}k + \text{etc.}) &= (a_{11}a_{25} + a_{12}b_{25} + a_{13}c_{25} + \text{etc.})i \\ &+ (b_{11}a_{25} + b_{12}b_{25} + b_{13}c_{25} + \text{etc.})j \\ &+ \text{etc.} \end{aligned}$$

But we have equally

$$\begin{aligned} i'j'm'A = (&a_{11}a_{25} + a_{12}b_{25} + a_{13}c_{25} + \text{etc.})I \\ &+ (b_{11}a_{25} + b_{12}b_{25} + b_{13}c_{25} + \text{etc.})J + \text{etc.} \end{aligned}$$

3. If $b = 0$, of course the result is 0.

So that $i'j'm'A = k'l'm'A$. Hence, $i'j'M = k'l'M$. It follows, then, that if $i'j'A = k'l'A$, then $i'j'$ into any non-relative unit equals $k'l'$ into the same unit, so that $i'j' = k'l'$. We thus see that whatever equality subsists between compounds of the accented letters i', j', k', etc., subsists between the same compounds of the corresponding unaccented letters i, j, k, so that the multiplication tables of the two algebras are the same.[4] Thus, what has been proved is that any associative algebra can be put into relative form, *i.e.* (see my *brochure* entitled *Brief Description of the Algebra of Relatives*) that every such algebra may be represented by a matrix.

Take, for example, the algebra (bd_5). It takes the relative form

$$i = (I{:}A) + (J{:}I) + (L{:}K), \quad j = (J{:}A),$$
$$k = (K{:}A) + (J{:}I) + \mathfrak{r}(L{:}I) + (I{:}K) + (M{:}K) + \mathfrak{r}(J{:}L) - (J{:}M) - \mathfrak{r}(L{:}M),$$
$$l = (L{:}A) + (J{:}K), \quad m = (M{:}A) + (\mathfrak{r}^2 - 1)(J{:}I) - (L{:}K) - \mathfrak{r}^2(J{:}M).$$

This is the same as to say that the general expression $xi + yj + zk + ul + vm$ of this algebra has the same laws of multiplication as the matrix

$$\begin{array}{cccccc}
0, & 0, & 0, & 0, & 0, & 0, \\
x, & 0, & 0, & z, & 0, & 0, \\
y, & \begin{array}{c} x+z \\ +(\mathfrak{r}^2-1)v, \end{array} & 0, & u, & \mathfrak{r}z, & -z-\mathfrak{r}^2 v, \\
z, & 0, & 0, & 0, & 0, & 0, \\
u, & \mathfrak{r}z, & 0, & x-v, & 0, & -\mathfrak{r}z, \\
v, & 0, & 0, & z, & 0, & 0.
\end{array}$$

Of course, every algebra may be put into relative form in an infinity of ways; and simpler ways than that which the rule affords can often be found. Thus, for the above algebra, the form given in the foot-note is simpler, and so is the following:

$$i = (B{:}A) + (C{:}B) + (F{:}D) + (C{:}E), \; j = (C{:}A),$$
$$k = (D{:}A) + (E{:}D) + (C{:}B) + \mathfrak{r}(F{:}B) + \mathfrak{r}(C{:}F),$$
$$l = (F{:}A) + (C{:}D), \; m = (E{:}A) + (\mathfrak{r}^2 - 1)(C{:}B) - (B{:}A) - (F{:}D) - (C{:}E).$$

These different forms will suggest transformations of the algebra. Thus, the relative form in the foot-note to (bd_5) suggests putting

$$i_1 = i + m, \; j_1 = \mathfrak{r}^2 j, \; k_1 = k + \mathfrak{r}^{-1} i + \mathfrak{r}^{-1} m, \; l_1 = \mathfrak{r} l + j, \; m_1 = -m,$$

when we get the following multiplication table, where ρ is put for $\mathfrak{r}^{-1}$:

4. A brief proof of this theorem, perhaps essentially the same as the above, was published by me in the *Proceedings of the American Academy of Arts and Sciences*, for May 11, 1875.

	i	j	k	l	m
i	0	0	0	0	j
j	0	0	0	0	0
k	0	0	i	j	l
l	0	0	ρj	0	0
m	$\rho^2 j$	0	ρl	0	j

Ordinary algebra with imaginaries, considered as a double algebra, is, in relative form,

$$1 = (X:X) + (Y:Y),\ \mathsf{J} = (X:Y) - (Y:X).$$

This shows how the operation J turns a vector through a right angle in the plane of X, Y. Quaternions in relative form is

$$\begin{aligned} 1 &= (W:W) + (X:X) + (Y:Y) + (Z:Z), \\ i &= (X:W) - (W:X) + (Z:Y) - (Y:Z), \\ j &= (Y:W) - (Z:X) - (W:Y) + (X:Z), \\ k &= (Z:W) + (Y:X) - (X:Y) - (W:Z). \end{aligned}$$

We see that we have here a reference to a space of four dimensions corresponding to X, Y, Z, W.

On the Algebras in which Division is Unambiguous

P 188: 225–29

1. In the *Linear Associative Algebra,* the coëfficients are permitted to be imaginary. In this note they are restricted to being real. It is assumed that we have to deal with an algebra such that from AB

$= AC$ we can infer that $A = 0$ or $B = C$. It is required to find what forms such an algebra may take.

2. If $AB = 0$, then either $A = 0$ or $B = 0$. For if not, $AC = A(B + C)$, although A does not vanish and C is unequal to $B + C$.

3. The reasoning of §40 holds, although the coëfficients are restricted to being real. It is true, then, that since there is no expression (in the algebra under consideration) whose square vanishes, there must be an expression, i, such that $i^2 = i$.

4. By §41, it appears that for every expression in the algebra we have

$$iA = Ai = A.$$

5. By the reasoning of §53, it appears that for every expression A there is an equation of the form

$$\Sigma_m(a_m A^m) + bi = 0.$$

But i is virtually arithmetical unity, since $iA = Ai = A$; and this equation may be treated by the ordinary theory of equations. Suppose it has a real root, α; then it will be divisible by $(A - \alpha)$, and calling the quotient B we shall have

$$(A - \alpha i)B = 0.$$

But $A - \alpha i$ is not zero, for A was supposed dissimilar to i. Hence a product of finites vanishes, which is impossible. Hence the equation cannot have a real root. But the whole equation can be resolved into quadratic factors, and some one of these must vanish. Let the irresoluble vanishing factor be

$$(A - s)^2 + t^2 = 0.$$

Then

$$\left(\frac{A - s}{t}\right)^2 = -1,$$

or, every expression, upon subtraction of a real number (*i.e.* a real multiple of i), can be converted, in one way only, into a quantity whose square is a negative number. We may express this by saying that every quantity consists of a scalar and a vector part. A quantity whose square is a negative number we here call a *vector.*

6. Our next step is to show that the vector part of the product of

two vectors is linearly independent of these vectors and of unity. That is, i and j being any two vectors,[1] if

$$ij = s + v$$

where s is a scalar and v a vector, we cannot determine three real scalars a, b, c, such that

$$v = a + bi + cj.$$

This is proved, if we prove that no scalar subtracted from ij leaves a remainder $bi + cj$. If this be true when i and j are any unit vectors whatever, it is true when these are multiplied by real scalars, and so is true of every pair of vectors. We will, then, suppose i and j to be unit vectors. Now,

$$ij^2 = -i.$$

If therefore we had

$$ij = a + bi + cj,$$

we should have

$$-i = ij^2 = aj + bij - c = ab - c + b^2 i + (a + bc)j;$$

whence, i and j being dissimilar,

$$-i = b^2 i, \qquad b^2 = -1,$$

and b could not be real.

7. Our next step is to show that, i and j being any two vectors, and

$$ij = s + v,$$

s being a scalar and v a vector, we have

$$ji = r(s - v),$$

where r is a real scalar. It will be obviously sufficient to prove this for the case in which i and j are unit vectors. Assuming them such, let us write

$$ji = s' + v', \qquad vv' = s'' + v'',$$

where s' and s'' are scalars, while v' and v'' are vectors. Then

$$ij \cdot ji = (s + v)(s' + v') = ss' + sv' + s'v + v'' + s''.$$

1. The idempotent basis having been shown to be arithmetical unity, we are free to use the letter i to denote another unit.

But we have

$$ij \cdot ji = ij^2 i = -i^2 = 1.$$

Hence,

$$v'' = 1 - ss' - s'' - sv' - s'v.$$

But v'' is the vector of vv', so that by the last paragraph such an equation cannot subsist unless v'' vanishes. Thus we get

$$0 = 1 - ss' - s'' - sv' - s'v,$$

or

$$sv' = 1 - ss' - s'' - s'v.$$

But a quantity can only be separated in one way into a scalar and a vector part; so that

$$sv' = -s'v.$$

That is,

$$ji = \frac{s'}{s}(s - v). \quad Q.E.D.$$

8. Our next step is to prove that $s = s'$; so that if $ij = s + v$ then $ji = s - v$. It is obviously sufficient to prove this when i and j are unit vectors. Now from any quantity a scalar may be subtracted so as to leave a remainder whose square is a scalar. We do not yet know whether the sum of two vectors is a vector or not (though we do know that it is not a scalar). Let us then take such a sum as $ai + bj$ and suppose x to be the scalar which subtracted from it makes the square of the remainder a scalar. Then, C being a scalar,

$$(-x + ai + bj)^2 = C.$$

But developing the square we have

$$\begin{aligned}&(-x + ai + bj)^2 \\ &= x^2 - a^2 - b^2 + abs + abs' - 2axi + 2bxj + ab\left(1 - \frac{s'}{s}\right)v = C;\end{aligned}$$

i.e.

$$ab\left(1 - \frac{s'}{s}\right)v = C - x^2 + a^2 + b^2 - abs - abs' + 2axi + 2bxj.$$

But v being the vector of ij, by the last paragraph but one the equation must vanish. Either then $v = 0$ or $1 - \dfrac{s'}{s} = 0$. But if $v = 0$, $ij = s$, and multiplying into j,

$$-i = sj,$$

which is absurd, i and j being dissimilar. Hence $1 - \frac{s'}{s} = 0$ and

$$ji = s - v. \quad Q.E.D.$$

9. The number of independent vectors in the algebra cannot be two. For the vector of ij is independent of i and j. There may be no vector, and in that case we have the ordinary algebra of reals; or there may be only one vector, and in that case we have the ordinary algebra of imaginaries.

Let i and j be two independent vectors such that

$$ij = s + v.$$

Let us substitute for j

$$j_1 = si + j.$$

Then we have

$$\begin{aligned}
&ij_1 = v, \ j_1 i = -v, \\
&j_1 v = j_1 ij_1 = -j_1^2 i = i, \ vj_1 = ij_1^2 = -i, \\
&iv = i^2 j_1 = -j_1, \ vi = ij_1 i = -j_1 i^2 = j_1.
\end{aligned}$$

Thus we have the algebra of real *quaternions*. Suppose we have a fourth unit vector, k, linearly independent of all the others, and let us write

$$\begin{aligned}
j_1 k &= s' + v', \\
ki &= s'' + v''.
\end{aligned}$$

Let us substitute for k

$$k_1 = s''i + s'j_1 + k,$$

and we get

$$\begin{aligned}
j_1 k_1 &= -s''v + v', & k_1 j_1 &= s''v - v', \\
k_1 i &= -s'v + v'', & ik_1 &= s'v - v''.
\end{aligned}$$

Let us further suppose

$$(ij_1)k_1 = s''' + v'''.$$

Then, because ij_1 is a vector,

$$k_1(ij_1) = s''' - v'''.$$

But

$$k_1 j_1 = -j_1 k_1, \qquad k_1 i = -ik_1,$$

because both products are vectors.
Hence

$$i \cdot j_1 k_1 = -i \cdot k_1 j_1 = -ik_1 \cdot j_1 = k_1 i \cdot j_1 = k_1 \cdot ij_1.$$

Hence

$$s''' + v''' = s''' - v'''$$

or $v''' = 0$, and the product of the two unit vectors is a scalar. These vectors cannot, then, be independent, or k cannot be independent of $ij = v$. Thus it is proved that a fourth independent vector is impossible, and that ordinary real algebra, ordinary algebra with imaginaries, and real quaternions are the only associative algebras in which division by finites always yields an unambiguous quotient.

Brief Description of the Algebra of Relatives

P 220: Privately printed brochure,
Baltimore: *7 January 1882*

Let A, B, C, etc., denote objects of any kind. These letters may be conceived to be finite in number or innumerable. The sum of them, each affected by a numerical coëfficient (which may equal 0), is called an *absolute term*. Let x be such a term; then we write

$$x = (x)_a A + (x)_b B + (x)_c C + \text{etc.} = \Sigma_i (x)_i I.$$

Here (x), etc., are numbers, which may be permitted to be imaginary or restricted to being real or positive, or to being roots of any given equation, algebraic or transcendental.[1] By φx, any mathematical function of the absolute term x, we mean such an absolute term that

$$(\varphi x)_i = \varphi(x)_i.$$

That is, each numerical coëfficient of φx is the function, φ, of the corresponding coëfficient of x. In particular,

$$(x + y)_i = (x)_i + (y)_i,$$
$$(x \times y)_i = (x)_i \times (y)_i.$$

Otherwise written,

$$x + y = \{(x)_a + (y)_a\}A + \{(x)_b + (y)_b\}B + \text{etc.}$$
$$x \times y = \{(x)_a \times (y)_a\}A + \{(x)_b \times (y)_b\}B + \text{etc.}$$

Two peculiar absolute terms are suggested by the logic of the subject. I call them terms of second intention. The first is zero, 0, and is defined by the equation

$$(0)_i = 0$$

1. I have usually restricted the coëfficients to one or other of two values; but the more general view was distinctly recognized in my paper of 1870.

or

$$0 = 0 \cdot A + 0 \cdot B + 0 \cdot C + \text{etc.}$$

The other is *ens* (or non-relative unity), $\bar{0}$, and is defined by the equation

$$(\bar{0})_i = 1,$$

or

$$\bar{0} = A + B + C + \text{etc.}$$

The symbol $(A:B)$ is called an *individual dual relative.* It signifies simply a pair of individual objects, $(A:B)$ and $(B:A)$ being different. An aggregate of such symbols, each affected by a numerical coëfficient, is called a *general dual relative.* The totality of pairs of letters arrange themselves with obvious naturalness in the block,

$$\begin{array}{cccc} A:A & A:B & A:C & \text{etc.} \\ B:A & B:B & B:C & \text{etc.} \\ C:A & C:B & C:C & \text{etc.} \\ \text{etc.} & \text{etc.} & \text{etc.} & \text{etc.} \end{array}$$

If l denotes any general dual relative, then the coëfficient of the pair $I{:}J$ in l is written $(l)_{ij}$. These coëfficients are thus each referred to a place in the above block, and may themselves be arranged in the block

$$\begin{array}{cccc} (l)_{aa} & (l)_{ab} & (l)_{ac} & \text{etc.} \\ (l)_{ba} & (l)_{bb} & (l)_{bc} & \text{etc.} \\ (l)_{ca} & (l)_{cb} & (l)_{cc} & \text{etc.} \\ \text{etc.} & \text{etc.} & \text{etc.} & \text{etc.} \end{array}$$

Every relative term, x, is separable into a part called 'self-x', Sx, such that

$$\mathrm{S}x = \Sigma_i (x)_{ii}(I:I)$$

and the remaining part, called 'alio-x', Vx; comprising all the terms in x not in the principal diagonal of the block; so that we write

$$x = \mathrm{S}x + \mathrm{V}x.$$

Each absolute term is considered to be equivalent to a certain relative term; namely,

$$A = (A:A) + (A:B) + (A:C) + \text{etc.}$$

or, if x be an absolute term,

$$(x)_{ij} = (x)_i.$$

The self-part of the relative equivalent to an absolute term is denoted by writing a comma after the term. Accordingly,

$$(x,)_{ii} = (x)_i, \qquad (x,)_{ij} = 0.$$

Besides 0 and $\bar{0}$, two other dual relative terms have been called terms of second intention. These are simply $S\bar{0}$ and $V\bar{0}$. The relative $S\bar{0}$ or $(\bar{0},)$ is also written 1, and is called unity, or 'identical with'. It is defined by the equations

$$(1)_{ii} = 1, \qquad (1)_{ij} = 0.$$

That is,

$$1 = (A:A) + (B:B) + (C:C) + \text{etc.}$$

The relative $V\bar{0}$ is written $\bar{1}$ or n, and is called 'not', or 'the negative of'. It is defined by the equations

$$(\bar{1})_{ii} = 0, \qquad (\bar{1})_{ij} = 1.$$

By an absolute function of a relative term is meant that function taken according to the rule for taking the function of an absolute term. That is,

$$(\varphi x)_{ij} = \varphi(x)_{ij}.$$

In particular,

$$(x + y)_{ij} = (x)_{ij} + (y)_{ij}$$
$$(x \times y)_{ij} = (x)_{ij} \times (y)_{ij}.$$

Of the various external or relative combinations that have been employed the following may be particularly specified. (1), External multiplication, defined by the equation

$$(xy)_{ij} = \Sigma_n (x)_{in} (y)_{nj}$$

(2), External progressive involution, defined by the equation

$$(x^y)_{ij} = \Pi_n \, (x)_{in}^{(y)_{nj}}$$

(3), External regressive involution, defined by the equation

$$({}^x y)_{ij} = \Pi_n (y)_{nj}^{(x)_{in}}.$$

In general, using Miss Ladd's notation[2] for the different orders of multiplication,

$$(x \underset{p}{\times} y)_{ij} = \underset{p-1}{\Pi_n} \{ (x)_{in} \underset{p}{\times} (y)_{nj} \}.$$

2. "On De Morgan's Extension of the Algebraic Processes," *Am. Jour. Math.*, Vol. III., No. 3.

Other modes of external combination have been used, but they are believed to have only a special utility. Division does not generally yield an unambiguous quotient. Indeed, I have shown that it does so only in the cases of ordinary real algebra, of imaginary algebra, and of real quaternions.

Besides the *mathematical* functions of relatives, there are various modes in which one relative may *logically* depend upon another. Thus, Sx and Vx may be said to be logical functions of x. The most important of such operations is that of taking the *converse* of a relative. The converse of x, written $\breve{x}$ or Kx, is defined by the equation

$$(\breve{x})_{ij} = (x)_{ji}.$$

The algebraical laws of all these combinations are obtained with great facility by a method of which the following are examples:—

Example 1. $\{(xy)z\}_{ij} = \Sigma_n (xy)_{in}(z)_{nj} = \Sigma_n \Sigma_m (x)_{im}(y)_{mn}(z)_{nj}$
$\{x(yz)\}_{ij} = \Sigma_m (x)_{im}(yz)_{mj} = \Sigma_m \Sigma_n (x)_{im}(y)_{mn}(z)_{nj}$
$\therefore\ (xy)z = x(yz).$

Example 2. $\{(x+y)z\}_{ij} = \Sigma_n (x+y)_{in}(z)_{nj} = \Sigma_n \{(x)_{in} + (y)_{in}\}(z)_{nj}$
$= \Sigma_n (x)_{in}(z)_{nj} + \Sigma_n (y)_{in}(z)_{nj} = (xz)_{ij} + (yz)_{ij}$
$\therefore\ (x+y)z = xz + yz.$

The following are some of the elementary formulæ so obtained. Non-relative multiplication is indicated by a comma, relative multiplication by writing the factors one after the other, without the intervention of any sign.

$$(x+y)+z = x+(y+z), \qquad x+y = y+x,$$
$$(x,y),z = x,(y,z), \qquad x,y = y,x,$$
$$(x+y),z = (x,z)+(y,z),$$
$$(xy)z = x(yz),$$
$$(x+y)z = xz+yz, \qquad x(y+z) = xy+xz,$$
$$(x^y)^z = x^{(yz)}, \qquad {}^x({}^yz) = {}^{(xy)}z, \qquad {}^x(y^z) = ({}^xy)^z,$$
$$(x,y)^z = (x^z),(y^z), \qquad {}^x(y,z) = ({}^xy),({}^xz),$$
$$x^{y+z} = (x^y),(x^z), \qquad {}^{x+y}z = ({}^xz),({}^yz),$$
$$kkx = x$$
$$k(x+y) = kx+ky, \qquad k(x,y) = kx,ky$$
$$k(xy) = (ky)(kx), \qquad k(x^y) = {}^{(ky)}(kx)$$
$$0+x = 0, \qquad 0,x = 0x = x0 = 0, \qquad x^0 = {}^0x = \bar{0},$$
$$\bar{0} \times x = x, \quad \bar{0}^x = {}^x\bar{0} = \bar{0},$$
$$1x = x1 = x^1 = {}^1x = x,$$

$$({}^x\bar{1})_{ij} = (\bar{1}^x)_{ij} = \begin{cases} 0, \text{ if } x_{ij} \neq 0. \\ 1, \text{ if } x_{ij} = 0. \end{cases}$$

Just as the different pairs of letters, A, B, C, etc., have been conceived to be arranged in a square block, so the different triplets of them may be conceived to be arranged in a cube, and the algebraical sum of all such triplets, each affected with a numerical coëfficient, may be called a *triple relative.*

Every dual relative may be regarded as equivalent to a triple relative, just as every absolute term is equivalent to a dual relative.

Every triple relative may be regarded as a sum of five parts, each being a linear expression in terms of one of the five forms,

$$(A:A):A \qquad (A:B):A \qquad (A:A):B \qquad (B:A):A \qquad (A:B):C.$$

The sign of a dual relative followed by a comma denotes that part of the equivalent triple relative which consists of terms in one of the forms

$$(A:A):(A:A) \qquad (A:B):(A:B).$$

The multiplication of triple relatives is not perfectly associative and the multiplication of two triple relatives yields a quadruple relative.

The modes of combination of a triple relative followed by two dual relatives are the same as the modes of combination of three dual relatives. This ceases to be true for quadruple and higher relatives.

Corresponding to the operation of taking the converse of a dual relative, there are five operations upon triple relatives. They are defined as follows:—

$$(Ix)_{ijk} = (x)_{jik},\ (Jx)_{ijk} = (x)_{ikj},\ (Kx)_{ijk} = (x)_{kji},\ (Lx)_{ijk} = (x)_{jki},\ (Mx)_{ijk} = (x)_{kij}.$$

Every quadruple or higher relative may be conceived as a product of triple relatives.

Thus, the essential characteristics of this algebra are (1) that it is a multiple algebra depending upon the addition of square blocks or cubes of numbers, (2) that in the external multiplication the rows of the block of the first factor are respectively multiplied by the columns of the block of the second factor, and (3) that the multiplication so resulting is, for the two-dimensional form of the algebra, always associative. I have proved in a paper presented to the American Academy of Arts and Sciences, May 11, 1875, that this algebra necessarily embraces every associative algebra.

I have here described the algebra apart from the logical interpretation with which it has been clothed. In this interpretation a letter is regarded as a name applicable to one or more objects. By a name is usually meant something representative *of* an object *to* a mind.

But I generalize this conception and regard a name as merely something in *a conjoint* relation to a second and a third, that is as a triple relative. A sum of different individual names is a name for each of the things named severally by the aggregant letters. A name multiplied by a positive integral coëfficient is the aggregate of so many different senses in which that name may be taken. The individual relative $A:B$ is the name of A considered as the first member of the pair $A:B$. The signification of the external multiplication is then determined by its algebraical definition.

Professor Sylvester, in his "New Universal Multiple Algebra," appears to have come, by a line of approach totally different from mine, upon a system which coincides, in some of its main features, with the Algebra of Relatives, as described in my four papers upon the subject,[3] and in my lectures on logic. I am unable to judge, from my unprofessional acquaintance with pure mathematics, how much of novelty there may be in my conceptions; but as the researches of the illustrious geometer who has now taken up the subject must draw increased attention to this kind of algebra, I take occasion to redescribe the outlines of my own system, and at the same time to declare my modest conviction that the logical interpretation of it, far from being in any degree special, will be found a powerful instrument for the discovery and demonstration of new algebraical theorems.

BALTIMORE, Jan. 7, 1882.

Postscript.—I have this day had the delight of reading for the first time Professor Cayley's "Memoir on Matrices," in the *Philosophical Transactions* for 1858. The algebra he there describes seems to me substantially identical with my long subsequent algebra for dual relatives. Many of his results are limited to the very exceptional cases in which division is a determinative process.

My own studies in the subject have been logical not mathematical, being directed toward the essential elements of the algebra, not towards the solution of problems.

JANUARY 16, 1882.

3. "Description of a Notation for the Logic of Relatives." *Memoirs,* American Academy of Arts and Sciences, Vol. IX. 1870. "On the Application of Logical Analysis to Multiple Algebra." *Proceedings* of the same Academy, 1875, May 11. "Note on Grassmann's Calculus of Extension." Ibid. 1877, Oct. 10. "On the Algebra of Logic." *Am. Journal of Mathematics,* Vol. III.

On the Relative Forms of Quaternions

P 226: **Johns Hopkins University Circulars**
1:13 (February 1882): 179

If X, Y, Z denote the three rectangular components of a vector, and W denote numerical unity (or a fourth rectangular component, involving space of four dimensions), and $(Y\!:Z)$ denote the operation of converting the Y component of a vector into its Z component, then

$$\begin{aligned}
1 &= (W\!:W) + (X\!:X) + (Y\!:Y) + (Z\!:Z)\\
i &= (X\!:W) - (W\!:X) - (Y\!:Z) + (Z\!:Y)\\
j &= (Y\!:W) - (W\!:Y) - (Z\!:X) + (X\!:Z)\\
k &= (Z\!:W) - (W\!:Z) - (X\!:Y) + (Y\!:X).
\end{aligned}$$

In the language of logic $(Y\!:Z)$ is a relative term whose relate is a Y component, and whose correlate is a Z component. The law of multiplication is plainly $(Y\!:Z)(Z\!:X) = (Y\!:X)$, $(Y\!:Z)(X\!:W) = 0$, and the application of these rules to the above values of $1, i, j, k$ gives the quaternion relations

$$i^2 = j^2 = k^2 = -1, \qquad ijk = -1, \text{ \&c.}$$

The symbol $a(Y\!:Z)$ denotes the changing of Y to Z and the multiplication of the result by a. If the relatives be arranged in the block

$$\begin{array}{cccc}
W\!:W & W\!:X & W\!:Y & W\!:Z\\
X\!:W & X\!:X & X\!:Y & X\!:Z\\
Y\!:W & Y\!:X & Y\!:Y & Y\!:Z\\
Z\!:W & Z\!:X & Z\!:Y & Z\!:Z,
\end{array}$$

then the quaternion $w + xi + yj + zk$ is represented by the matrix of numbers

$$\begin{array}{cccc} w & -x & -y & -z \\ x & w & -z & y \\ y & z & w & -x \\ z & -y & x & w. \end{array}$$

The multiplication of such matrices follows the same laws as the multiplication of quaternions. The determinant of the matrix = the fourth power of the tensor of the quaternion.

The imaginary $x + y\sqrt{-1}$ may likewise be represented by the matrix

$$\begin{array}{cc} x & y \\ -y & x, \end{array}$$

and the determinant of the matrix = the square of the modulus.

[On the Logic of Relatives]

MS 413: Spring–Summer 1882

[Version A]

Let A, B, C, etc. denote all the individual things in the universe. Let m be a general term, as 'man'. Then m is an aggregate of some of the letters A, B, C, etc. This may be represented by the expression

$$m = (m)_a \mathrm{A} + (m)_b \mathrm{B} + (m)_c \mathrm{C} + \text{etc.}$$

Here, $(m)_a$ etc. are numerical coëfficients which are either *zero* or *unity.* If any coëfficient say $(m)_k$ is *zero,* then the individual K does not belong to the class denoted by m. If on the other hand $(m)_k$ is unity, K is an m. But it will be found convenient in reference to these coëfficients to disregard all distinctions of numbers greater than zero, and write

$$\mathrm{A} = 2\mathrm{A} = 3\mathrm{A} = \text{etc.}$$

the sign of equality signifying merely that the symbol on one side is equal to that on the other, to a finite positive coëfficient *près.*

A dual relative term, such as l, 'lover of ———', may be treated in the same way, except that the individuals in this case are pairs. That is we may write

$$\begin{aligned} l = {} & (l)_{aa}\, \mathrm{A{:}A} + (l)_{ab}\, \mathrm{A{:}B} + (l)_{ac}\, \mathrm{A{:}C} + \text{etc.} \\ & + (l)_{ba}\, \mathrm{B{:}A} + (l)_{bb}\, \mathrm{B{:}B} + (l)_{bc}\, \mathrm{B{:}C} + \text{etc.} \\ & + (l)_{ca}\, \mathrm{C{:}A} + (l)_{cb}\, \mathrm{C{:}B} + (l)_{cc}\, \mathrm{C{:}C} + \text{etc.} \\ & + \text{etc.} \end{aligned}$$

Here, as before each coëfficient is either *zero* or greater than *zero.*

By means of this notation we may define the eight simple modes of combination of relatives as follows where every individual in the universe is to be successively substituted for x:—

$$
\begin{aligned}
(ls)_{ij} &= \Sigma_x (l)_{ix}(s)_{xj} & (\overline{ls})_{ij} &= \Pi_x \{0^{(l)_{ix}} + 0^{(s)_{xj}}\} \\
(l^s)_{ij} &= \Pi_x \{(l)_{ix}^{(s)_{xj}}\} & (\overline{l^s})_{ij} &= \Sigma_x \{0^{(l)_{ix}} (s)_{xj}\} \\
({}^l s)_{ij} &= \Pi_x \{(s)_{xj}^{(l)_{ix}}\} & (\overline{{}^l s})_{ij} &= \Sigma_x \{(l)_{ix}\, 0^{(s)_{xj}}\} \\
(l \circ s)_{ij} &= \Sigma_{ij} 0^{(l)_{ix} + (s)_{xj}} & (\overline{l \circ s})_{ij} &= \Pi_x \{(l)_{ix} + (s)_{xj}\}
\end{aligned}
$$

Of these eight operations, we shall find it convenient to deal in the future only with the first and last; and we may use the dagger to denote this last, thus:—

$$l \dagger s = \overline{l \circ s}.$$

We shall have then

$$
\begin{aligned}
l^s &= l \dagger \bar{s} & \overline{l^s} &= \bar{l} s \\
{}^l s &= \bar{l} \dagger s & \overline{{}^l s} &= l \bar{s} \\
\overline{l \circ s} &= l \dagger s & l \circ s &= \bar{l} \bar{s}
\end{aligned}
$$
$$\overline{ls} = \bar{l} \dagger \bar{s}.$$

We now apply the notation to the deduction of the formulae.

To prove that $l(sw) = (ls)w$.
This is proved if for every M and Q

$$\{l(sw)\}_{mq} = \{(ls)w\}_{mq}$$

where it must be remembered that equality only means that the two members vanish together. Now

$$\{l(sw)\}_{mq} = \Sigma_n (l)_{mn} (sw)_{nq}$$

There are no negative quantities in question. Hence, the second member vanishes or not, according as for every N

$$(l)_{mn} (sw)_{nq} = 0$$

But

$$(l)_{mn} (sw)_{nq} = (l)_{mn} \Sigma_p (s)_{np} (w)_{pq} = \Sigma_p (l)_{mn} (s)_{np} (w)_{pq}$$

This vanishes or not, according as every term of the summation vanishes or not. Hence $l(sw)$ vanishes or not according as for every four individuals M, N, P, Q, we have

$$\text{either } (l)_{mn} = 0 \quad \text{or} \quad (s)_{np} = 0 \quad \text{or} \quad (w)_{pq} = 0.$$

But from the symmetry of the definitions this is also the condition of the vanishing of *(ls)w.* Hence the two expressions are equivalent.

[Version B]

Let A, B, C, etc. denote all the individual things in the universe. Let m be a general term, as 'man'. Then m is an aggregate of some of the letters A, B, C, etc. This fact may be represented by writing

$$m = (m)_a \mathrm{A} + (m)_b \mathrm{B} + (m)_c \mathrm{C} + \text{etc.} = \Sigma_i (m)_i \mathrm{I}$$

Here, $(m)_a$ etc. are numerical coëfficients which are each of them either *zero* or *unity,* according as the individual which it multiplies is excluded from or included under the class m.

For ordinary nonquantitative problems of logic, it is unnecessary and inconvenient to insist upon the coëfficient which is the sign of existence being exactly unity. It is better to allow it to take any value greater than zero; and all values greater than *zero* are to be considered as equivalent, so that to say that two coëfficients are equal shall only mean that if either vanishes the other does likewise.

A dual relative term such as l, 'lover of ———' may be represented in a similar way, thus:—

$$\begin{aligned} l = {} & (l)_{aa}\,\mathrm{A}:\mathrm{A} + (l)_{ab}\,\mathrm{A}:\mathrm{B} + (l)_{ac}\,\mathrm{A}:\mathrm{C} + \text{etc.} \\ & + (l)_{ba}\,\mathrm{B}:\mathrm{A} + (l)_{bb}\,\mathrm{B}:\mathrm{B} + (l)_{bc}\,\mathrm{B}:\mathrm{C} + \text{etc.} \\ & + (l)_{ca}\,\mathrm{C}:\mathrm{A} + (l)_{cb}\,\mathrm{C}:\mathrm{B} + (l)_{cc}\,\mathrm{C}:\mathrm{C} + \text{etc.} \\ & + \text{etc.} = \Sigma_{ij} (l)_{ij}\,\mathrm{I}:\mathrm{J} \end{aligned}$$

A combination of relatives such as, *ls,* 'lover of a servant of ———', will be defined as soon as the value of $(ls)_{ij}$ or the coëfficient of the general individual relative I:J in the development of *ls* is given.

There are two modes of combination of relatives which we shall here principally consider, relative multiplication and relative addition.[1] The latter is symbolized by a dagger †, and l†s means 'lover of everything but servants of ———'. The two operations are defined as follows:—

$$(ls)_{ik} = \Sigma_j (l)_{ij} (s)_{jk}$$

$$(l \dagger s)_{ik} = \Pi_j \{(l)_{ij} + (s)_{jk}\}$$

1. The analogy of the last to the aggregation of non-relative terms, though not very close, is sufficient to suggest the name I have given to it.

The combination *ls* vanishes if and only if every coëfficient $(ls)_{ik}$ vanishes. Each of these vanishes if and only if every term of the summation in the above equation vanishes, and each of these vanishes if and only if one of the two factors vanishes. Hence, *ls* vanishes if and only if for any three individuals I, J, K, we have

$$\text{either } (l)_{ij} = 0 \quad \text{or} \quad (s)_{jk} = 0.$$

The combination $l \dagger s$ only vanishes if every coëfficient $(l \dagger s)_{ik}$ vanishes. This can only happen if some factor of the continued product in the above equation vanishes, and this can only be if both terms of the factor vanish. Hence $l \dagger s$ vanishes only if for every pair of individuals I, K, a third J can be found such that

$$\text{both } \quad (l)_{ij} = 0 \quad \text{and} \quad (s)_{jk} = 0.$$

From this view of the matter we may easily deduce the fundamental formulae. Thus, to prove that $l(sb) = (ls)b$. The condition of the vanishing of $l(sb)$ is that for every three individuals H, I, K,

$$\text{either } (l)_{hi} = 0 \quad \text{or} \quad (sb)_{ik} = 0$$

Or again that for every four individuals, H, I, J, K,

$$\text{either } (l)_{hi} = 0 \quad \text{or} \quad (s)_{ij} = 0 \quad \text{or} \quad (b)_{jk} = 0.$$

But this is precisely the condition of the vanishing of *(ls)b.*

To prove that

$$l \dagger (s \dagger b) = (l \dagger s) \dagger b.$$

The coëfficient $\{l \dagger (s \dagger b)\}_{hk}$ vanishes if and only if some individual I can be found such that

$$\text{both } (l)_{hi} = 0 \quad \text{and} \quad (s \dagger b)_{ik} = 0,$$

or again, only if two individuals I and J can be found such that we have at once

$$(l)_{hi} = 0 \quad (s)_{ij} = 0 \quad (b)_{jk} = 0.$$

This is precisely the condition of the vanishing of $\{(l \dagger s) \dagger b\}_{hk}$.

Let us compare $(l \dagger s)b$ with $l \dagger sb$. The condition of the vanishing of $\{(l \dagger \mathrm{s})b\}_{hk}$ is that for every individual J

$$\text{either } (l \dagger s)_{hj} = 0 \quad \text{or} \quad (b)_{jk} = 0.$$

The condition of the first equation is that some individual I can be found such that

$$\text{both } (l)_{hi} = 0 \quad \text{and} \quad (s)_{ij} = 0.$$

Different I's may belong to different J's. The condition of the vanishing of $(l \dagger sb)_{hk}$ is that one individual I can be found such that

$$\text{both } (l)_{hi} = 0 \quad \text{and} \quad (sb)_{ik} = 0.$$

The condition of the last equation is that for every J, with one and the same I,

$$\text{either } (s)_{ij} = 0 \quad \text{or} \quad (b)_{jk} = 0.$$

Suppose then that $(l \dagger sb)_{hk}$ vanishes. If J be such that $(b)_{jk} = 0$, then, for this J the condition of $\{(l \dagger s)b\}_{hk} = 0$ is fulfilled. If however $(b)_{jk}$ does not vanish there is an I such that

$$\text{both } (l)_{hi} = 0 \quad \text{and} \quad (s)_{ij} = 0$$

and again the condition of $\{(l \dagger s)b\}_{hk} = 0$ is fulfilled. We have, therefore,

$$(l \dagger s)b \prec l \dagger sb;$$

and by the symmetry of the definitions,

$$l(s \dagger b) \prec ls \dagger b.$$

These two are extremely important formulæ.

The *converse* of a relative may be defined as follows:—

$$(\breve{l})_{ij} = (l)_{ji}$$

From this it obviously follows that

$$\breve{\overline{ls}} = \breve{s}\breve{l}$$

$$\breve{\overline{l \dagger s}} = \breve{s} \dagger \breve{l}.$$

The *negative* may be defined as follows:—

$$(\bar{l})_{ij} = 0^{(l)_{ij}},$$

it being understood that $0^0 = 1$. From this we easily get

$$\overline{ls} = \bar{l} \dagger \bar{s}$$

$$\overline{l \dagger s} = \bar{l}\bar{s}.$$

The two operations combined give

$$\breve{\overline{ls}} = \breve{\bar{s}} \dagger \breve{\bar{l}}$$

$$\overline{l \dagger s} = \breve{\bar{s}}\breve{\bar{l}}.$$

Suppose we have the proposition that every lover of a servant of is a benefactor of or $ls \prec b$. This implies merely that if $(b)_{ik}$ vanishes then $(ls)_{ik}$ vanishes, and consequently that for every individual, J,

$$\text{either } (l)_{ij} = 0 \qquad \text{or} \qquad (s)_{jk} = 0.$$

Denote the vanishing of $(b)_{ik}$ $(l)_{ij}$ and $(s)_{jk}$ by a, b, c, respectively. Then the proposition $ls \prec b$ is equivalent to

$$a \prec b + c$$

a formula of non-relative logic, which we know takes the transformations

$$\bar{b} \prec \bar{a} + c$$

$$\bar{c} \prec \bar{a} + b.$$

The first of these equations signifies that if $(l)_{ij}$ does not vanish, either $(b)_{ik}$ does not vanish or $(s)_{jk}$ does vanish. This is the same as to say that if $(\bar{l})_{ij} = 0$, then either $(\bar{b})_{ik} = 0$ or $(\breve{s})_{kj} = 0$. But if this is so for every three individuals I, J, K, we have that if $(\bar{l})_{ij} = 0$ then $(\bar{b}\breve{s})_{ij} = 0$. Consequently

$$\bar{b}\breve{s} \prec \bar{l}$$

or by conversion

$$s\breve{\bar{b}} \prec \breve{\bar{l}}.$$

And similarly, $\breve{\bar{b}}l \prec \breve{\bar{s}}$. Hence the rule is that having a formula of the form $ls \prec b$, the three letters may be cyclically advanced one place in the order of writing, those which are carried from one side of the copula to the other being both negatived and converted.

We have, then, the following twelve propositions, all equivalent

$$\begin{array}{lll}
ls \prec b & s\breve{\bar{b}} \prec \breve{\bar{l}} & \breve{\bar{b}}l \prec \breve{\bar{s}} \\
\breve{s}\,\breve{l} \prec \breve{b} & \bar{b}\breve{s} \prec \bar{l} & \breve{l}\,\bar{b} \prec \bar{s} \\
\bar{b} \prec \bar{l} \dagger \bar{s} & \breve{l} \prec \bar{s} \dagger \breve{b} & \breve{s} \prec \breve{b} \dagger l \\
\breve{\bar{b}} \prec \breve{\bar{s}} \dagger \breve{\bar{l}} & l \prec b \dagger \breve{\bar{s}} & s \prec \breve{\bar{l}} \dagger b
\end{array}$$

There are in all 64 such sets of 12 equivalent propositions.

[On Relative Terms]

MS 414: Spring–Summer 1882

Relative terms are divisible into such as contain no individual relatives of the form A : A, such as contain some such individuals and exclude some, and such as exclude all such individuals. They are also divisible into such as contain no individual relatives of the form A : B, such as contain some such individuals and exclude some, and such as exclude no such individuals. There are thus nine classes. But four of these consist of only one relative each. Namely, the relative 0 is the only one which excludes all individuals of both forms, and ∞ is the only one which includes all of both forms. The relative 'identical with ———', 1, is the only one which includes all of the form A : A and excludes all of the form A : B; and 'not' or 'other than ———', n, is the only one which excludes all of the form A : A and includes all of the form A : B. There remain five classes. Those which contain some and exclude some of both forms, are called *free* relatives. Those which contain only individuals of the form A : A are called *self-relatives,* and are the negatives of those which contain all of the form A : B. Those that contain only individuals of the form A : B are called *alio-relatives,* and are the negatives of such as contain all of the form A : A.

Every alio-relative and every negative of an alio-relative is the converse of a relative of the same class. Every self-relative and negative of a self-relative is its own converse.

Let a and b be alio-relatives, so that $(a)_{ii} = 0$, $(b)_{ii} = 0$. Then

$$(a \dagger b)_{ii} = \Pi_j \{(a)_{ij} + (b)_{ji}\}$$

But when $j = i$ the factor vanishes and $(a \dagger b)_{ii} = 0$. So that the relative sum of two alio-relatives is an alio-relative, and the relative product of two negatives of alio-relatives belongs to the same class. Suppose that $(a)_{ij} = 0$; then $(a \dagger b)_{ij} = 0$. For

$$(a \dagger b)_{ij} = \Sigma_k \{(a)_{ik} + (b)_{kj}\}.$$

Now when $k = j$, we have $(a)_{ij} = 0$, $(b)_{jj} = 0$. Hence, a and b being alio-relatives

$$a \dagger b \prec a$$
$$a \dagger b \prec b.$$

And a and b being negatives of alio-relatives, we have

$$a \prec ab,$$
$$b \prec ab.$$

The product of two self-relatives evidently belongs to the same class, is independent of the order of the factors, and consists of all those individuals which are common to the two factors. The relative sum of two negatives of self-relatives belongs to the same class, is independent of the order of the terms, and consists of all the individuals in either of those terms.

The relative sum of two self-relatives vanishes, when the universe contains more than two individuals. The relative product of the negatives of two self-relatives embraces (under the same limitation) the universe.

The relative product of a self-relative and an alio-relative is another alio-relative contained under the former. The relative sum of an alio-relative and a self-relative is another self-relative contained under the former. The relative sum of the negative of a self-relative and the negative of an alio-relative is another negative of an alio-relative embracing the former. The relative product of the negative of an alio-relative and the negative of a self-relative is another negative of a self-relative embracing the former.

To every non-relative term, as 'man', m, corresponds the self-relative 'man that is ———', denoted by writing a comma after the symbol of the non-relative term; and also corresponds the negative of this self-relative. The last may be denoted by an inverted comma after the symbol of the non-relative term. The non-relative product of two terms is the term whose corresponding self-relative is the relative product of the self-relatives corresponding to the two terms.

$$(a \times b), = (a,)(b,).$$

The non-relative sum of two terms is in like manner defined by the equation

$$(a + b)^{\text{‘}} = (a^{\text{‘}}) \dagger (b^{\text{‘}}).$$

Every relative term is in one way, and one way only, the non-relative sum of a self-relative and an alio-relative. We may denote the self-part and alio-part of a relative by a capital S and A before the symbol of the relative. Thus,

$$l = \mathrm{S}l + \mathrm{A}l.$$

Every relative is in like manner, in one way only, the non-relative product of the negative of a self-relative and the negative of an alio-relative. Thus,

$$l = \overline{\mathrm{S}}l \times \overline{\mathrm{A}}l.$$

In fact,

$$\overline{\mathrm{S}}l = \mathrm{n} + \mathrm{S}l$$
$$\overline{\mathrm{A}}l = l + \mathrm{A}l$$

Remarks on [B. I. Gilman's "On Propositions and the Syllogism"]

P 230: **Johns Hopkins University Circulars**
1:17 (August 1882): 240

Mr. Peirce remarked that the propositional system of De Morgan supposes the universe of things to be limited (in this sense, that there is some logically possible combination of characters which does not occur in any existing thing), but supposes the universe of characters to be unlimited (in the sense that there is no aggregate of things which do not possess some common mark). Mr. Peirce undertook to show what propositional system is necessary in case both universes are permitted to be limited, so that nothing of a general nature can be assumed respecting the relations of things and of characters. There will, in the first place, be *universal* propositions asserting that two classes of objects have no common breadth, and *particular* propositions asserting that two classes of objects have a common breadth. These are purely *extensive* propositions. There will also be purely intensive, or (better), *comprehensive* propositions, asserting that two groups of marks have or have not a common depth.

Next there will be propositions asserting some relation between a group of things and a group of marks. These will be of twelve intransmutable species, as follows:

First, *affirmative* propositions, asserting the possession of characters by things.

α. Every one of a named class of things is said to possess every one of a named group of characters.

β. *Some* one or more (without saying what), of a class of things is said to possess every one of a group of characters.

γ. Every one of a named group of characters is said to be possessed by some one or other of a named class of things. [γ follows strictly from β.]

δ. Some one or more (without saying what), of a named group of characters is said to be possessed by every one of a named class of things.

ϵ. Every one of a class of things is said to possess some one or other of a named group of characters. [ϵ follows from δ.]

ζ. Some one of a class of things is said to possess some one of a group of characters.

Second, six *negative* propositions, denying the possession of characters by things will correspond exactly to the affirmative forms.

These may be called propositions of the *first* order, because the relative term "possessing as a character" enters into them once.

A proposition of the second order will be such as this. "Every S possesses every character not possessed by some T." There will be eighty-eight species of this order. The number of orders is infinite.

Propositions of even orders are evidently of two families, the *extensive* and the *comprehensive.*

In immediate inferences, the order of the conclusion agrees with that of the premise in respect to being even or odd; and in simple immediate inference the order is either unchanged or increased or diminished by two. An immediate inference from a universal to a particular proposition is impossible.

In syllogisms, it is necessary and sufficient that there should be a distributed middle term, and the order of the conclusion is the sum of the orders of the premises. Of course, immediate inference from the conclusion is often possible. [What De Morgan calls spurious propositions may be inferred from two premises having an undistributed middle.]

The treatment of the subject is greatly facilitated by the application of the algebra of relatives. For this purpose, the propositions may be so transformed as to make 1 or "what is identical with" in every case the subject; and only one copula will be necessary, owing to the terms being identified with relatives. To obtain an inference the premises are to be relatively multiplied together, and then all inferences conform to one or other of these formulae:—

$$(a \dagger b)c \prec a \dagger bc$$
$$a(b \dagger c) \prec ab \dagger c.$$

The dagger here denotes the operation of "relative addition." If l denote "lover of" and s "servant of," then ls denotes "lover of a servant of," and $l \dagger s$ denotes "lover of everything but a servant of."

A universal proposition stating that one relation is a special case of another is most naturally written in the form $l \prec s$, which particular example means that to be a lover is to be a servant. But we have the general formulae

$$\begin{array}{ll} 1x = x & x \dagger \mathfrak{n} = x \\ x \dagger 1 = x & \mathfrak{n} \dagger x = x \end{array}$$

when $\mathfrak{n}$ means 'not', or 'other than'. By means of these formulae we bring the proposition $l \prec s$ into the forms

$$\begin{array}{ll} 1l \prec s & l \prec s \dagger \mathfrak{n} \\ l1 \prec s & l \prec \mathfrak{n} \dagger s. \end{array}$$

We now apply one of the rules for transposition. These rules are, first, that the proposition $xy \prec z$ is equivalent to $\breve{\bar{z}}x \prec \breve{\bar{y}}$ and to $y\breve{\bar{z}} \prec \breve{\bar{x}}$ (where the straight line over a term *negatives* it and the curved line *converts* the relation), and second, that the proposition $x \prec y \dagger z$ is equivalent to $\breve{\bar{z}} \prec x \dagger \breve{\bar{y}}$ and to $\breve{\bar{y}} \prec z \dagger \breve{\bar{x}}$. The application of these rules to the four forms just given yields the following equivalents of $l \prec s$:

$$\begin{array}{ll} l\breve{\bar{s}} \prec \mathfrak{n} & 1 \prec \breve{\bar{l}} \dagger s \\ \breve{\bar{s}}l \prec \mathfrak{n} & 1 \prec s \dagger \breve{\bar{l}}. \end{array}$$

A particular proposition asserting that there is a pair of objects such that the first is at once in two relations to the second, cannot, in general, be expressed by the algebra of *dual* relatives without the use of a negative copula. But this can be done in the special case in which the relatives have every object in the universe for correlate to each relate. Now a non-relative term such as 'man' may be considered as a relative term of which the relates are the different men and the correlates all objects in the universe; so that whatever is a man is a man for every object in the universe. With that understanding, if m denote 'man' and h 'honorable', we can express that some man is honorable by writing $1 \prec \breve{m}h$ or $1 \prec \breve{h}m$.

Suppose now that we have given the two premises $1 \prec a \dagger b$ and $1 \prec \breve{\bar{b}} \dagger c$. Multiplying them we have

$$1 \prec (a \dagger b)(\breve{\bar{b}} \dagger c).$$

But

$$(a \dagger b)(\breve{\bar{b}} \dagger c) \prec a \dagger b(\breve{\bar{b}} \dagger c) \prec a \dagger b\breve{\bar{b}} \dagger c.$$

Now we have the general formula

$$b\breve{\bar{b}} \prec \mathfrak{n}.$$

Thus we infer

$$1 \prec a \dagger \mathfrak{n} \dagger c.$$

But

$$a \dagger \mathfrak{n} = a,$$

so that we have

$$1 \prec a \dagger c,$$

which is the syllogistic conclusion. In like manner from the two premises $1 \prec a \dagger b$ and $1 \prec \breve{\bar{b}}c$, we get

$$1 \prec (a \dagger b)\breve{\bar{b}}c$$

and thence, successively,

$$1 \prec (a \dagger b\breve{\bar{b}})c$$

$$1 \prec (a \dagger \mathfrak{n})c$$

$$1 \prec ac$$

which is the syllogistic conclusion.

But from the two premises $1 \prec ab$ and $1 \prec \breve{\bar{b}}c$ we only get

$$1 \prec (ab)(\breve{\bar{b}}c)$$

$$1 \prec a\mathfrak{n}c$$

which is a spurious proposition.

The extension of this method to the syllogistic of the higher orders of propositions affords no difficulty.

REPORT OF A CONFERENCE ON GRAVITY DETERMINATIONS, HELD AT WASHINGTON, D. C., IN MAY, 1882

[Edited by Charles S. Peirce]

[*Introduction*]

P 260: **Coast Survey Report 1882,** ***503***

In pursuance of a correspondence between Major (now Lieut. Col.) J. Herschel, R. E., and the Superintendent of the United States Coast and Geodetic Survey, relative to the most advantageous mode of prosecuting pendulum observations and the scientific value of the same, the following-named gentlemen met at the Coast Survey Office, May 13, 1882, for an informal conference: The Superintendent of the Coast and Geodetic Survey; Major Herschel, R. E.; Prof. C. S. Peirce, Prof. S. Newcomb (on the part of astronomy), and Messrs. George Davidson and C. A. Schott (on the part of geodesy). Maj. J. W. Powell, Director of the United States Geological Survey (on the part of geology), was unable to attend.

The proceedings of the conference consisted in the reading and discussion of papers and the adoption of resolutions. These papers, with the comments of the different gentlemen present, and the resolutions adopted form the contents of this report. Their titles are as follows:—

1. Letter from Professor Hilgard to Major Herschel
2. Reply of Major Herschel
3. Six Reasons for the Prosecution of Pendulum Experiments; by C. S. Peirce
4. Notes on Determinations of Gravity; by Assistant C. A. Schott
5. General Remarks upon Gravity Determinations; by Major Herschel
6. Opinions concerning the Conduct of Gravity Work; by Mr. Peirce
7. Resolutions.

Letter from Professor Hilgard to Major Herschel

P 260: 503

UNITED STATES COAST AND GEODETIC SURVEY OFFICE,
Washington, May 4, 1882.

Maj. J. HERSCHEL, R. E., *Brevoort House, New York:*

MY DEAR SIR: In pursuance of my letter of yesterday's date, I will now submit to you the proposition that, as Superintendent, &c., I invite at once, or at your earliest convenience, a conference on gravity observations, the participants in which would be, beside yourself and Peirce, Newcomb, on the part of astronomy, King and Powell, on the part of geology, and Davidson and Schott, on the part of geodesy. During such conference the greatest range of discussion would of course be in place, but its outcome, I conceive, must necessarily be formulated in a few propositions, some of which would be mainly intended to recite the scientific objects and usefulness of such work, and commend it to public patronage, others, to define the degree of accuracy to be attained in the observations, in order to entitle them to be ranked as contributions to science. As neither you nor ourselves are charged with any special powers in the premises, it appears to me that no other useful results can be reached by a conference than some such public declarations, the value of which rests upon the standing of the party making them. If this proposition meets your views, I shall be happy to make such arrangements for the earliest day you may find convenient. I regret that it will be necessary to tax you with coming to Washington, as all other parties are here, and being officially engaged it would be out of our power to meet you elsewhere.

It will be well if you will formulate in advance such expressions of opinion as appear to you desirable in the premises, in order that after comparing notes we may be able to submit propositions that will readily meet the assent of the conference.

Yours very truly,

J. E. HILGARD,
Superintendent.

Reply of Major Herschel

P 260: 504–5

NEW YORK, *May* 5, 1882.

Prof. J. E. HILGARD:

MY DEAR SIR: I am very glad to learn that you are well inclined towards the idea of a conference, and that now, in fact, it rests with me to indicate when I can be in Washington for the purpose.

As it is not a matter which presses until you have issued invitations, when of course it should not be delayed, and as it will be well to give a few days' notice in any case, I will not consider myself in any way required to hasten my departure from New York, but only to give you as early an indication as I can when I can undertake to be in Washington. At this moment I am not in a position to say precisely, but it will almost certainly be within a week from this date. I shall most likely be able to leave this city about Wednesday next.

With regard to the lines of discussion, it must depend to some extent on the degree of publicity which the proceedings would have. It is not to be denied or concealed that there is coming into existence a certain rivalry between what may be called the German and the English schools. I am anxious that the former shall not wrongly claim American adhesion on the one hand, and on the other that American opinion shall not be wrongfully interpreted as favoring the German system. With reference to this last, for instance, I have just received the following from M. C. Wolf, in the course of a reply to my Washington letter: "Je vous félicite vivement de vos travaux sur le pendule, et surtout d'avoir pris une autre voie que celle dans laquelle les Américains se sont lancés, à la suite des Allemands et des Suisses. J'ai eu ici de vives discussions avec M. Ch. Peirce au sujet de ses expériences avec le pendule réversible de Repsold, instrument qui me paraît construit dans de déplorables conditions de stabilité."

Now there is just enough truth in this to make one regret the misapprehension as to the American position. But so long as your survey uses a reversible pendulum, without some very distinct statements as to the principles, such misapprehensions will continue, and the Germans will deny that the Americans stand by the differential method.

I hold it to be a very lamentable thing that men of zeal, eager to advance science, should continue to be misled by the old school of physics into launching upon the difficult and precarious enterprise of absolute determinations of gravity, generally in ignorance of the real difficulties of the research, and *always* indifferent to the utility of such determination. The German school is responsible for this.

This brings before us prominently the question of utility, a question which has always been shirked or disposed of by common-places, devoid of any real force. I know this through having urged (for nearly twenty years), very much in vain, the views which I hold at this day, and which I now see gaining ground so slowly. I sum it up in the broad statement that *we do actually know* the mean figure of the earth *as well as we can know it* so long as the irregularities which deform it remain unknown. It is not the force of gravity which we seek, but the irregularities of the surface.

Now this is one of the points on which, at a conference, I should wish to find unanimity, if it is true, or if not, then a better and more indisputable dogma to take its place.

With this as a foundation, the question becomes one of ways and means to study the irregularities with advantage. Here there is great room for difference of opinion. What can be done depends on the cost, in its most general sense, of doing it. Absolute measurements are indefinitely costly, and may be put aside. Differential measurements, also, are frightfully costly, if conducted as I have been conducting these; but I have had in view to prove incontestably that results of practical value can be obtained with a tenth, perhaps a twentieth, the labor that I devoted to them. All depends on the method.

Another point involved in the question of utility is, as you say, as to the degree of precision demanded. All stations of observation should be recognized from the first as belonging to one of two categories—either they are *points d'appui* or they are not. In the latter case the precision demanded is governed by the degree of irregularity which experience teaches as governing the quantity measured, the distances which separate the points being taken into account. A high degree of precision is plainly needless (for points of the second order) if they are widely scattered, whereas if a number of such points are crowded in a small area, their precision ought to be higher because of the information to be gained by intercomparison. Points of the second order widely scattered have no present value other than as indicating tentatively the degree of disturbance. From this point of view there would seem to be an advantage in placing the stations always *in pairs* so as to indicate the variability as well as the variation.

I must now go further. Scientific observation has two distinct aspects. Viewed in one way, it is seen as a means of livelihood, as an intellectual enjoyment, as an employment, as a pursuit worthy of recognition and encouragement, for every reason except that of its ultimate utility. Viewed in the other way, it is a source of expenditure and a drain upon the available power of the time and country, which can only be justified if it attains useful ends in a reasonably expeditious way. For myself, I doubt if I could conscientiously recommend the expenditure of public money on pendulum observations on the ground of their utility; although I could and would recommend it for pendulum *experiments*, having for their object to increase the facility of observation; for I imagine, as things now stand, the prospect of obtaining results in sufficient number and frequency to enable us to study the irregularities successfully is very remote. You will doubtless recognize in this the ground of my inability to offer my services, backed by hopes of support from the British Government, for the prosecution of differential work on this continent. But I have no business to press such considerations on other people, nor to bring them forward at a conference, except incidentally in discussing the proper distribution of stations and the degree of precision demanded. The same arguments and motives had a successful campaign in dictating my latitude work in India; and there is room for their application in the present case. They point out the urgent need for economy in every detail of installation and observation—in the choice of stations and the buildings to be occupied, in the distribution of time to be taken up by the observations, and by the calculations respectively, so as to get, in short, as many results of a sufficient degree of accuracy, and no more, as possible within the year. All this, and much more, seems to me to be involved in the broad question whether or not pendulum research can be satisfactorily carried on with a view to studying the earth's irregularities.

Another phase of this question should deal with the distinction between a study of the large and of the small irregularities. There is a vast difference between such work as that of Malaspina, of Freycinet, of Sabine, of Foster, of Lütke, and of all the other explorers; and that of Kater in England, and of Basevi in India. The work before you here has or may have the characters of both; for the vastness of your disposable area demands a large plan, while the numerous opportunities for prosecuting minuter internal exploration require more special consideration. The degree of precision to be aimed at must be governed partly by what we know of possible variation and partly by what the instruments are capable of. Here, as in other branches of research, we should bear in mind that there is almost always a point in the scale of precision where it becomes questionable whether it would not be wiser to change the whole system if higher precision is wanted. Below that point there is no difficulty. Above it the price to be paid becomes onerous.

You will readily perceive that I fully recognize, as one of the chief subjects upon which discussion should turn, this of requisite precision. At the same time I doubt if it can be discussed to much advantage by those who are not intimate with the figures actually to hand. I would therefore avoid the *vexata quæstio* of probable errors and keep to principles. It is by the latter alone that plans of operation can be governed reasonably. "Frequency to be preferred to accuracy," for example, is a principle easy to limit or extend as may be desired, and far more widely intelligible to the uninitiated than any specification in figures suited to certain categories of cases. Above all we should aim at being intelligible. Without that there will be no outside interest and no support. . . .

Yours truly,

J. HERSCHEL.

Six Reasons for the Prosecution of Pendulum Experiments

P 261 (P 260): 506–8

1. The first scientific object of a geodetical survey is unquestionably the determination of the earth's figure. Now, it appears probable that pendulum experiments afford the best method of determining the amount of oblateness of the spheroid of the earth; for the calculated probable error in the determination of the quantity in question from the pendulum work already executed does not exceed that of the best determination from triangulation and latitude observations, and the former determination will shortly be considerably improved. Besides, the measurements of astronomical arcs upon the surface of the earth cover only limited districts, and the oblateness deduced from them is necessarily largely affected, as Mr. Schott has remarked, by the old arc of Peru, the real error of which no doubt greatly exceeds that which the calculation attributes to it so that we cannot really hold it probable that the error of this method is so small as it is calculated by least squares to be. On the other hand, the pendulum determinations are subject to no great errors of a kind which least squares cannot ascertain; they are widely scattered over the surface of the earth; they are very numerous; they are combined to obtain the ellipticity by a simple arithmetical process; and, all things considered, the calculated probable error of the oblateness deduced from them is worthy of unusual confidence. In this connection it is very significant, as pointed out by Colonel Clarke (*Geodesy,* p. vi), that while the value derived from pendulum work has for a long time remained nearly constant, that derived from measurements of arcs has altered as more data have been accumulated, and the change has continually been in the direction of accord with the other method. It is needless to say that the comparison of the expense

of the two methods of obtaining this important quantity is immensely in favor of pendulum work.

2. Recent investigations also lead us to attach increased importance to experiments with the pendulum in their connection with metrology. The plan of preserving and transmitting to posterity an exact knowledge of the length of the yard after the metallic bar itself should have undergone such changes as the vicissitudes of time bring to all material objects, was at one time adopted by the British Government. It was afterwards abandoned because pendulum operations had fallen into desuetude, and because doubts had been thrown upon the accuracy of Kater's original measure of the length of the seconds pendulum. Yet I do not hesitate to say that this plan should now be revived, for the following reasons.

First, because measurements of the length of the seconds pendulum, although formerly subject to grave uncertainties, are now secure against all but very small errors. Indeed, we now know that the determinations by Kater and his contemporaries, after receiving certain necessary corrections, are by no means so inaccurate as they were formerly suspected to be. Secondly, metallic bars have now been proved, by the investigations of Professor Hilgard and others, to undergo unexpected spontaneous alterations of their length, so that some check upon these must be resorted to. To this end the late Henri Ste.-Claire Deville and Mascart constructed for the International Geodetical Association a metre ruled upon a sort of bottle of platin-iridium, with the idea that the cubic contents of this bottle should be determined from time to time, so as to ascertain whether its dimensions had undergone any change. I am myself charged with, and have nearly completed, a very exact comparison of the length of a metre bar with that of a wave of light, for the same purpose. Neither of these two methods is infallible, however, for the platin-iridium bottle may change its three dimensions unequally, and the solar system may move into a region of space in which the luminiferous ether may have a slightly different density (or elasticity), so that the wave-length of the ray of light used would be different. These two methods should therefore be supplemented by the comparatively simple and easy one of accurately comparing the length of the seconds pendulum with the metre or yard bar. Thirdly, I do not think it can be gainsaid by any one who examines the facts that the measurements of the length of the seconds pendulum by Borda and by Biot in Paris and by Bessel in Berlin do, as a matter of fact, afford us a better and more secure knowledge of the length of their standard bars than we can attain in any other way. So also I have more confidence in the value of the ratio of the yard to the metre obtained by the comparison of the measurements of the length of the seconds pendulum at the Kew observatory by Heaviside in terms of the yard and by myself in terms of the metre than I have in all the elaborate and laborious comparisons of bars which have been directed to the same end. I will even go so far as to say that a physicist in any remote station could ascertain the length of the metre accurately to a one hundred thousandth part more safely and easily by experiments with an invariable reversible pendulum than by the transportation of an ordinary metallic bar.

A new application of the pendulum to metrology is now being put into practice by me. Namely, I am to oscillate simultaneously a yard reversible pendulum and a metre reversible pendulum. I shall thus ascertain with great precision the ratio of their lengths without any of those multiform comparisons which would be necessary if this were done by the usual method. These two pendulums will be swung, the yard one in the office of the Survey, at a temperature above 62° F., which is the standard temperature of the yard, the other nearly at 0° C., which is the standard temperature of the metre; and thus we shall have two bars compared at widely different temperatures, which, according to ordinary processes, is a matter of great difficulty. The knife-edges of the pendulums will be interchanged and the experiments repeated. Finally, the yard pendulum will be compared with a yard bar and the metre pendulum with a metre bar, and last of all the yard pendulum with its yard bar will be sent to England, the metre pendulum with its metre bar to France, for comparison with the primary standards; and thus it is believed the ratio of yard to metre will be ascertained with the highest present attainable exactitude.

3. Geologists affirm that from the values of gravity at different points useful inferences can be drawn in regard to the geological constitution of the underlying strata. For instance, it has been found that when the gravity upon high lands and mountains is corrected for difference of centrifugal force and distance from the earth's centre, it is very little greater than at the sea-level. Consequently, it cannot be that there is an amount of extra matter under these elevated stations equal to the amount of rock which projects above the sea-level; and the inference is that the elevations have been mainly

produced by vertical and not by horizontal displacements of material. On the other hand, Mendenhall has found that gravity on Fujisan, the well-known volcanic cone of Japan, which is about 12,000 feet high, and which is said to have been upheaved in a single night, about 300 B.C., is as much less than that in Tokio as if the mountain had been wholly produced by horizontal transfer. This conclusion, if correct, must plainly have a decisive bearing upon certain theories of volcanic action. Again, it has long been known that gravity is in excess upon islands, and I have shown that this excess is fully equal to the attraction of the sea-water. This shows that the interior of the earth is not so liquid and incompressible that the weight of the sea has pressed away to the sides the underlying matter. But in certain seas gravity is even more in excess than can be due to the attraction of the ocean, as if they had been receptacles of additional matter washed down from the land. It is evident that only the paucity of existing data prevents inferences like these from being carried much further. On the two sides of the great fault in the Rocky Mountains gravity must be very different, and if we knew how great this difference was we should learn something more about the geology of this region; and many such examples might be cited.

4. Gravity is extensively employed as a unit in the measurement of forces. Thus, the pressure of the atmosphere is, in the barometer, balanced against the weight of a measured column of mercury; the mechanical equivalent of heat is measured in foot-pounds, etc. All such measurements refer to a standard which is different in different localities, and it becomes more and more important to determine the amounts of these differences as the exactitude of measurement is improved.

5. It may be hoped that as our knowledge of the constitution of the earth's crust becomes, by the aid of the pendulum investigations, more perfected, we shall be able to establish methods by which we can securely infer from the vertical attractions of mountains, etc., what their horizontal attractions and the resulting deflections of the plumb-line must be.

6. Although in laying out the plan of a geodetical survey the relative utility of the knowledge of different quantities ought to be taken into account, and such account must be favorable to pendulum work, yet it is also true that nothing appertaining to such a survey ought to be neglected, and that too great stress ought not to be put upon the demands of the practically useful. The knowledge of the

force of gravity is not a mere matter of utility alone, it is also one of the fundamental kinds of quantity which it is the business of a geodetical survey to measure. Astronomical latitudes and longitudes are determinations of the direction of gravity; pendulum experiments determine its amount. The force of gravity is related in the same way to the latitude and longitude as the intensity of magnetic force is related to magnetical declination and inclination; and as a magnetical survey would be held to be imperfect in which measurements of intensity were omitted, to the same extent must a geodetical survey be held to be imperfect in which the determinations of gravity had been omitted; and such would be the universal judgment of the scientific world.

Notes on Determinations of Gravity, by C. A. Schott

P 260: 508–10

The conference was invited by the Superintendent of the Coast and Geodetic Survey for the purpose of eliciting an interchange of views respecting the utility and best means of prosecuting pendulum research in the interest of science in general, and with especial regard to the future work of the Coast and Geodetic Survey.

Major Herschel, R. E., having expressed his willingness to favor the meeting with his presence and give it the benefit of his great experience in pendulum work, the time of meeting must be considered extremely favorable.

The following rough notes are offered with a view of inviting discussion on some points considered of importance and interest.

Respecting the question of the utility to geodesy and geology of pendulum work as bearing on the figure and density of the earth, it is sufficiently answered by the resumption of this work in recent years in the leading government surveys conducted in Europe, Asia, and America; but in carrying on these operations different opinions continue to be held as to the best and most economical means both with regard to form of instrument and method of observation.

It may be added that the results already reached are in themselves sufficient to stimulate the further prosecution of the work, since they render it almost certain that still more valuable deductions may be reached.

The pendulum work executed for some years past under the direction of the late Superintendent of the Coast and Geodetic Survey had for its immediate object the study, theoretical and practical, of the best methods available, and to gather the results at various important pendulum stations in Europe, to bring them into strict comparability, and to form a connected system which may be used for combination with similar operations commenced in the United States.

Mr. C. S. Peirce, Assistant, Coast and Geodetic Survey, having brought this work to a close in Europe,[1] its future prosecution at home now claims renewed attention, both with respect to the economy and efficiency of the plans which it may be desirable to adopt.

The value of the pendulum results depending largely upon their direct comparability and the geographical extent, it would in the first place appear most desirable, in order to form a second and independent connection of the pendulum work executed on the other side of the Atlantic, to swing the American pendulums at the two stations, Washington and Hoboken, just occupied by Major Herschel with the old pendulums belonging to the Royal Society, and to add thereto at least one more American station in order to secure three stations of satisfactory accord between these instruments.

It is, perhaps, the general opinion that differential measures are at present more desirable than absolute measures, since undoubtedly greater accuracy can be reached in the former and a greater number may be secured with the same expenditure; indeed, the determination of the length of a seconds pendulum is, in geodesy, of less importance than a knowledge of ratios of times of oscillation of an invariable pendulum swung at stations on a line selected for investigation.

1. [Mr. Peirce remarked that that work was not yet quite completed.]

The determination of the length of a seconds pendulum is quite a special operation, to be undertaken only at a base station.

While the mean figure of the earth may be considered as tolerably well known from the fact of the close approach of the value of the compression as deducted from purely geodetic operations and from pendulum work, yet this may be taken only as an encouragement for the joint prosecution of both operations.[2]

On the other hand, our knowledge of the magnitude of the mean figure of the earth is, in the opinion of some, not quite as satisfactory, and in support of this it may be stated that the recent abandonment, in the Coast and Geodetic Survey, of the Besselian spheroid of revolution for that of Clarke, involving in our latitude an increase of the radii vectores between one-third and one-half of a statute mile, was no inconsiderable change; and though we cannot look forward to any future change of such a magnitude, the difference was sufficiently large to make itself felt in our oblique arc lying along the Atlantic coast between Maine and Georgia.

The combination of the Peruvian arc, the only one in America as yet worked in with the meridional arcs measured in the eastern hemisphere, with the two arcs measured by the Coast and Geodetic Survey, viz., the Nantucket arc and the Pamlico-Chesapeake arc, showed a satisfactory accord (that is, within limits that may be explained by local deflections). This seems to prove that the curvature of North America does not sensibly differ from the curvature in the same latitudes of the eastern hemisphere; yet the conclusion is weakened by the fact that the Peruvian arc is extremely short, and, what is worse, is supported by but two astronomical latitudes, and that in a region where local deflection probably exists of an excessive magnitude. It is true the computed corrections to the two latitudes are small, and this might lead to too great a confidence in the assigned value of the magnitude of the earth's axis. A remeasure and extension of this arc to be supplied with a considerable number of astronomical latitudes would seem to be a great desideratum, especially when we consider the important position of the arc, giving it, so to say, undue leverage in comparison with the position of other arcs. It is not at all

2. [Major HERSCHEL. I do not regard the agreement of geodetic and gravity figures an argument for the latter. I can never regard the geodetic figure, derived from the comparisons of the curvatures of certain *land* portions only, as a true indication of a figure which is two-thirds sea. There is every reason to regard the land curvatures as too great.]

unlikely that the results of its remeasure and extension may have an important effect on our knowledge of the probable uncertainty in the assigned value for the resulting mean figure of the earth.[3]

This mean figure might be defined as that of a geometrical solid whose surface most nearly approaches the equipotential surface of the mean sea-level, intersecting it so that the aggregate of the volumes above and below it may be equal and a minimum. It would be the object of geodesy to trace out on this geometrical surface the boundaries of these areas, and to determine their elevations above or depression below it; in fact, work out the actual irregularities with reference to this ideal mean figure.

For pendulum research the region of the Mississippi Valley would seem to be very favorable, both in regard to its geological structure, as presenting broad features, and with respect to gradual changes in elevation of surface between New Orleans and our northern boundary, near the forty-ninth parallel, the land rising but little above 1,000 feet. Here a study of the law of change of gravity with the latitude seems inviting.

Supplementary to the above line, the thirty-ninth parallel might be chosen for the study of the law of change of gravity with altitude, starting from the sea-level and passing over the inconsiderable elevations of about 2,500 feet on the Appalachian range and the descent to the Mississippi Valley, we have the gradual rise of the great plains up to 8,000 feet, and next the lower Rocky Mountain plateau, with a final return to the sea-level. While on the first named line about 6 or 8 stations might suffice, on the second from 12 to 15 ought to be contemplated.

Respecting the kind of pendulum most suitable for differential measures of gravity, there may be little difference in practice between the use of two invariable pendulums, the one to check the other, and an unchangeable pendulum of a plain rod (of lenticular cross-section) having two fixed knife-edges symmetrically disposed;

3. [Major HERSCHEL. I should hardly advocate a remeasurement of the Peruvian arc as a step towards a better determination of the earth's figure. It has the fatal disadvantage of position in a valley between vast mountainous tracts.

Mr. PEIRCE. Major Herschel's objection to the important scheme of remeasuring the Peruvian arc would apply, *à fortiori*, against allowing that arc to enter into the determination of the figure. In my humble judgment an American figure of the earth, wholly from geodetic measurements on these continents, is so greatly wanted that it is the duty of this Survey to undertake it. Although the Peruvian arc is at present bad, I should think that if sufficiently extended and provided with an adequate number of latitude determinations, the objections to it would nearly disappear.]

the means for correcting for difference of temperature and for difference of pressure from respective mean quantities to be determined at a base station. Observations to be made in 4 positions (upper knife-edge, lower knife-edge, face front, and face back). The accord of the 4 results will furnish a criterion for the unaltered condition of the pendulum.

A reversible pendulum of outer symmetrical form may also be made to answer the purpose, provided it be swung only with heavy end down (face front and back) and no change whatever is made in the supporting knife-edge or in any other part of the instrument. Two such pendulums would seem desirable in order to detect any change due to accident. With such a pendulum the correction for difference of pressure can be applied with greater certainty than in one of the other forms.

Respecting the stand of the Repsold apparatus, experience has shown it to be unfit for the work, and stiffer support should be provided.

If pendulums could be swung through twenty-four hours the result could be made independent of variations in the clock rate due to the daily variation of temperature and pressure. The same standard time stars should be observed each night. For shorter durations of swing, say for six hours only, this advantage might in a measure be secured either by making four fresh starts and thus continue the work during twenty-four hours, or if that be too laborious, to observe on the first day, say from 6 to 12 a.m. and p.m., and on the following day from 0 to 6 p.m. and a.m., and unite the results into one, or in general, for any station by a *symmetrical distribution* of the swings over the twenty-four hours.

Time furnished telegraphically by an observatory whose clock is protected from changes of temperature and pressure will be preferable to any local determination at a field station.

Should the duration of swing be too limited for this scheme, night work may be recommended, with a set of transit observations just before and another immediately after the close of a swing, the same two sets of stars to be used each night and for several stations as long as practicable.

Three days successful work at any one station may suffice, and about two weeks might be estimated for the time required for occupation during the best season. The observatory to be prepared by an advance party.

The method of coincidences furnishes all needful accuracy, but if, in the absence of a clock or otherwise, a chronometer be used (as more portable and less liable to injury), coincidences of the chronometer beat with the transit of the pendulum over a vertical line might be tried.

The question whether or not it is advisable to swing in a vacuum chamber (say at a density just below any that might naturally be expected at a place which it is proposed to visit) would seem to depend largely upon the time a pendulum can be made to swing advantageously. If its sectional dimensions are such as to displace much air and require it to do much work against friction, the duration of swing may be so short as to demand the use of an exhausted receiver. What the experience is with the new reversible pendulums of the pattern of the one sent last summer to one of the polar research stations of the Signal Corps the writer is not informed.

The above notes are respectfully submitted.

MAY 13, 1882.

General Remarks upon Gravity Determinations, by John Herschel

P 260: 510–12

The following propositions are *from my point of view,* but seem likely to be assented to in the main by other members of the conference.

1. *Figure of the earth.*—By this we imply the actual (or conceivable) continuous water surface as exemplified by the mean sea-level; which surface may be everywhere nearly, though nowhere fully,

represented by some assumed simple geometrical figure, such as an elliptic spheroid, to be known *ad hoc* as the *mean* figure.

2. *Object of pendulum research.*—If we regard the mean figure as known, then the object of pendulum research is, in the first place, to trace out the degree of separation everywhere subsisting between the actual and the mean figures; or, if it should appear that by a change of the mean figure there would result a less degree of separation, then to ascertain, first, what should be the amount of this alteration, and then to trace out the residual separation. Bearing in mind the large body of past work, which has undoubtedly sufficed to indicate very closely what the mean figure is, it should now be recognized as more particularly the object of pendulum research to enlarge our knowledge of the *irregularities of figure* rather than to aim at improving the *mean* figure; which after all can never be anything more than one *of reference,* by which to describe the actual figure.

3. *Extension of research among the irregularities.*—This is *prima facie* desirable, especially when geodetic surveys are in progress, or are certain to be instituted as civilization advances. But gravimetrical exploration in regions which can never be reached by surveying operations is of scarcely less importance.

4. As regards *distribution of stations* of observation, there seems to be nearly equal advantage in laying them out in a *linear series* at sufficiently close intervals, or superficially scattered over a limited selected area, with a view to tracing out the *sectional* or *solid* forms of the existing *irregularities.*

5. *The absolute force of gravity.*—If this also be admitted as an ultimate object of pendulum research, it must be remembered that it can only be determined for the whole earth when the exact relation of the place of observation to the whole surface is correctly known. It follows that a precise knowledge of the absolute force of gravity for the earth as a whole is not at present attainable. There are, nevertheless, reasons for now determining, with all the precision at present possible, *the length of the seconds pendulum* at different places on the earth's surface.

6. *Reasons for prosecuting absolute determinations.*—Regarding the local force of gravity as a constant, the length of a pendulum is a function of its rate of oscillation; or, in other words, its rate is a measure of its length. From this it follows that lengths, otherwise incommensurable, can be compared through their corresponding times of oscillation, because we have means (in the pendulum itself,

for instance) of comparing together, with any desired degree of precision, these times. Thus, for example, the metre and the yard can be compared by this means (as I understand) with greater precision than by the complicated system of linear comparisons requisite to measure their difference in terms of each.

7. *Constancy of gravity tested against constancy of length.*—This is another reason for determining with the utmost precision the length of the seconds pendulum in terms of this or that standard. For if, in the far distant future, there should appear a concurrence of testimony indicating change, it might be brought home to either of the bars, or even to gravity itself, according to the evidence. The absence of the requisite evidence in the past would be a grave reproach hereafter.

8. *The invariable pendulum.*—The impossibility of ascertaining the exact relation of any station to the whole surface, short of a general knowledge of the latter, calls necessarily for such explorations as are set forth in Article 2. It is generally acknowledged that the differential pendulum—of which the "invariable" may be regarded as the type—is best adapted for such work. The pattern known as Kater's has hitherto been without a rival; but any pattern will answer the purpose in which the principle of invariability—*i.e.*, fixity of knife-edge and absence of all movable parts—is embodied.

9. *The reversible pendulum* is recognized as having many excellent qualities; and is capable of being used *temporarily* as an invariable pendulum. But its proper field is the absolute measurement for which it was designed; for if its knife-edges are interchangeable it is liable at any time to have its invariable character destroyed, either intentionally or accidentally.

10. With regard to the degree of precision to be aimed at, nothing very definite can be laid down, since it depends largely on the circumstances. A gross error in a solitary arctic station, for instance, might be of little consequence, while an error of even a small fraction of a second in the difference between two central points would entail far-reaching consequences. When the object is tentative exploration only, accuracy may well be sacrificed to expedition and frequency. And in general it should be remembered that the local disturbance varies with the change of site. What the rate of this change may be can only be guessed until data are obtained. A group of contiguous determinations of a low order of accuracy would always be more valuable than a single one of the very highest order. A solitary station

can contribute only to the general problem of mean figure and will of necessity be vitiated by the amount of the local disturbance, as to which there is no evidence. If the range of such disturbance *on the whole of that parallel* were known, it would not be unreasonable to take one-fourth part of that as the range of probable error permissible in the determination itself. Every consideration which takes into account the existence of local disturbance points to the preference to be given to frequency of distribution rather than accuracy of result. Moreover, it is difficult, if not impossible, to estimate the probable error in any case whatever. The history of pendulum observation abounds with inexplicable contradictions and anomalies indicative of unknown causes of error; and hardly a single observer has ventured to estimate the probable error of his result. Practically, the question of precision is cut by a variety of circumstantial exigencies; and it would seem best to leave it at the discretion of the observer, or director of the work.

11. *Other modes of research.*—The foregoing indicates so plainly the need of tentative exploration of a low order of accuracy that it is very much to be desired that some simpler means should be found of obtaining at least a rude measure of the local deflection. Various statical modes have been proposed, but none has yet shown a satisfactory test. That a "stathmometer"—a term designed to leave untouched the present use of "gravimeter"—will some day be invented is highly probable. It might be, perhaps, the sooner if the very great need for it were more widely known, and if, at the same time, it were understood that its object would be served even though it should fail to rival the pendulum in accuracy.

[Mr. PEIRCE. The conception which Major Herschel has presented of the purpose of gravity determinations requires thorough study. Considered from a purely mathematical point of view, it is certain that if we know the distribution of gravity over the whole earth, or even over a large region, we can deduce corrections of the earth's radius vector. Within 20° of the station whose radius vector was to be corrected an accurate knowledge of the residuals of gravity would be necessary, while beyond that point a rougher determination would suffice. But whether this conception of the nature of pendulum work could be usefully adopted at the present time, or until two or three times the existing number of stations have been occupied, is a practical question in regard to which there is something to be said on both sides. The views of Major Herschel, though founded on known propositions of mathematics, are so novel and so far-reaching in their consequences that we cannot commit ourselves to an immediate decision in regard to them. But they offer much food for reflection and study, and I am quite sure that apart from the important service that Major Herschel has done us in connecting the American (and through that the continental European) system of stations directly with the great *réseau* of the English work by means of the Kater invariable pendulums, American geodetical science is under great obligation to him for the suggestions contained in the paper he has presented to the conference.]

Opinions concerning the Conduct of Gravity Work

P 262 (P 260): 512–16

The following are opinions concerning the conduct of gravitation work that are held by me but that others may consider questionable.

I. There are six reasons for determining gravity, which I have already set forth.

II. In determining the compression of the earth's spheroid from the variation of gravity, it is best, for the present, to reject all experiments not made with Kater's invariable pendulums. But the completion of Major Herschel's history of pendulum determinations is greatly to be desired.

[Major Herschel thought the limitation to Kater's invariable pendulum too narrow; and pointed out that it would exclude the work of Freycinet and of Duperrey as well as a great part of that of Foster.]

III. The ordinary correction for continental attraction is vastly too great. It should be omitted.

[Major Herschel remarked: "Admitting this as a conclusion drawn from the facts, it must not be forgotten that this is nothing but an *à posteriori* dogma. I do not see how it can be lawfully acted upon, unless the assumption that it has a true *à priori* cause is kept continually in view *as such.*" Mr. Peirce replies as follows: "In my opinion, the correction for continental attraction is not only refuted by observation but it has no *à priori* support from premises which we have any reason to suppose true. If we could make our pendulum experiments underground at the level surface of which the sea-level is a part, there would be no correction to be made for continental attraction. Since they cannot be made there, the observed gravity had to be reduced to what it would be at that level. The coëfficient of this reduction depends entirely on the distances of the successive level surfaces without reference to the situation of the material masses, except so far as this situation affects those distances. To calculate the reduction exactly upon this principle would be impossible; but we approximate to it within the limits of other neglected terms if we use Young's rule[4] without the term depending on continental attraction. Stokes reaches this same result; but having reached it, he remarks that if this theoretically correct procedure were used the figure of the earth would be less regular than in using the old rule. He offers no proof of this, however; and the facts which have been ascertained since his memoir was written prove that the contrary is true. Young's rule supposes that if all the rock rising above the sea-level were annihilated, the present level surface would remain a level surface, which is certainly not true. When Major Herschel admits, as he seems to do, that a certain conclusion is proved by the facts but at the same time maintains it cannot be 'lawfully' acted upon, he seems to be using the language of a game with conventional rules. I would propose to act upon any proposition that seems to be true." Mr. Schott agreed with Mr. Peirce.

NOTE BY MAJOR (NOW LIEUTENANT-COLONEL) HERSCHEL.—I should like the issue between Mr. Peirce and myself, on the general question of the reduction on account of continental or mountain attraction, to be somewhat differently stated than it appears here. In the first place, what I have said about an "*à posteriori* dogma" had reference, if I remember rightly, not so much to the rejection of the continental reduction *in toto* as to its modification by an arbitrary constant, about which Mr. Peirce is now silent. However, my words

4. This so-called rule is identical with *Bouguer's* formula for the same.

are general enough, no doubt, to cover this rejection in any form, but all I maintain is that the assumption on which it rests shall be plainly presented and never disregarded. Mr. Peirce contends that the reduction for continental attraction has no claim to any such apologetic treatment, urging that, as it has no rational foundation, it should go; the displacement of matter, which appears as land elevation, being in all probability a merely vertical displacement, while for the continental attraction to have any jurisdiction, it would be necessary to show the existence of at least a very considerable *lateral* displacement as the cause, or part cause, of elevation. Now it is just here that I would step in and urge the claim of the latter, of which there is ample proof in the enormous thickness and extent of stratified deposits, all of which must be due to erosion and removal horizontally. Something also might be said for glacial transfer, and for lava streams.]

IV. The residuals of the different stations are materially diminished by subtracting the entire downward attraction of the ocean, liberally estimated.

[Mr. Peirce admitted that this would involve a falsification of the earth's figure, so as to give a sort of mean figure. That is to say, gravity determinations at the sea-level will without any correction show the actual form of that surface, at least so far as the attraction of the land can be neglected. If, however, the values given by such determinations are altered in any way, they will no longer show the actual figure. The modification proposed only serves to reduce the residuals. It involves the kind falsification that belongs to all generalization.

Major HERSCHEL. The addition of the sea attraction has a legitimate *raison d'être,* as it is reasonable to affirm that the *sea* matter is *added* matter.

Mr. PEIRCE. It seems to me if the attraction of the sea is to be allowed for because it is added or horizontally displaced matter, then the attraction of the continents should not be allowed for, because it is not added, that is, is only vertically displaced matter.]

V. The occupation of additional arctic stations, if done well, would probably improve the value of the compression. New equatorial stations are also desirable, but new stations in middle latitudes can hardly affect the value of the compression.

[Major HERSCHEL. The actual distribution is shown in a diagram given in my Appendix to Vol. V of the India Survey. This diagram shows how very restricted is the area actually occupied by differential stations. The southern hemisphere is very poorly represented.]

VI. In middle latitudes, the main thing at present is to study the relation of gravity to geographical and geological conditions.

[Major Herschel concurred.]

VII. Gravity determinations should be made at intervals on lines of geodetic levels, and the levels be corrected accordingly.

[Mr. Schott concurred.]

VIII. Economical questions should, as far as possible, be solved by the application of mathematics.

IX. The invariable reversible pendulum reunites the advantages of the two instruments possessing the one and the other of these characters, and is to be recommended under the limitation implied in No. II above.

[Major HERSCHEL. I am obstinately opposed to any attempted combination of the invariable and reversible principles in one instrument. They are incompatible; and the combination is impossible without so modifying the invariable principle that it is practically abandoned altogether. It is very undesirable that any new element of doubt should be imported into the already much abused term "invariable." It was first used by Godin, as well as by Bouguer, and by la Condamine. They all meant a rigid pendulum with fixed knife-edge. Kater borrowed the word from the French, but as he at the same time introduced a "convertible" pendulum, with two fixed knife-edges, which made a great noise abroad, the two got mixed up, and the German text-books (copying from Muncke[5]) flagrantly confused the two. Still, the German use is strict in denoting a rigid pendulum with fixed knife-edges. But Mr. Peirce now intends to upset this last stronghold of the "invariable" pendulum by making it variable at the will of the observer. The invariable pendulum proper *can* undergo no change without violating its name. Closely connected with the term "invariable," as designating a particular form of construction, is the term "invariability" as denoting a principle involved in its design. I cannot possibly demur to the construction of any form of pendulum which may be thought desirable; but I do urgently protest against the designation of it in a way to create needless confusion. The principle of the invariable pendulum supposes it to continue unchanged as long as human carelessness will

5. Gehler's *Physikalisches Wörterbuch.* Art. "Pendel." VII, pp. 304–407. Leipzig, 1833. By far the ablest treatise, historical and otherwise, of its day, and perhaps so still.

permit, or longer if possible. But by making its knife-edges interchangeable, with a view to giving it a greater range of utility, this first characteristic is voluntarily destroyed; and in becoming reversible *in the full sense of the word* it ceases to be invariable. Why, then, adopt a self-contradictory compound name which serves no purpose but to ruin the word as well?

At the same time I must say that Mr. Peirce seems to *read the word* differently.

Mr. PEIRCE. By an invariable reversible pendulum, I mean one in which the knives remain in place from one station to another. Major Herschel's objections seem to be directed against the use of the word invariable as applied to such an instrument; but it is not so much the word as the thing that I advocate. The Geodetical Association has unanimously recommended the reversible pendulum, and I should certainly think that their opinion ought to be respected, even if I did not share it. On the other hand, there is much to be said in favor of differential instruments. I am not aware that Major Herschel has brought forward any objection to reuniting the differential or invariable and the reversible principles in one instrument except this, that if the knives can be changed they might be changed by carelessness. But it appears to me that the whole weight of this argument, such as it is, is against the invariable pendulum. For there is no fabrication of human hands that cannot be changed by carelessness. Can a Kater invariable pendulum be safely exposed to careless treatment? The difference between the ordinary invariable pendulum and the invariable reversible pendulum in this respect is that if the former suffers injury the work is hopelessly vitiated, while if the latter is injured, it is only necessary to fall back on the reversible principle. The following are the advantages which I think I see in the use of the invariable reversible pendulum:

1st. It satisfies the requirements of those who advocate the reversible pendulum, who constitute the greater weight of living authority.

2nd. It ought to satisfy those persons who advocate the invariable pendulum.

3rd. It determines gravity in two nearly independent ways, without more experiments than are necessary for a single determination. When these results agree they may be assumed to be correct.

4th. If the instrument be considered as a differential one, the difference in the reduced time of oscillation with heavy end down and with heavy end up must remain unchanged so long as the instrument is invariable and can hardly escape change otherwise. And from this change the necessary correction can be calculated and applied. If on the other hand the instrument be considered as an absolute one, the same difference is the best test of the accuracy of the work.

Mr. SCHOTT. For the strict intercomparability of results at two or more stations, I think it to be almost essential to satisfactory work that an absolutely invariable pendulum be employed. This condition would, however, not exclude the use of a pendulum having interchangeable knife-edges, provided that between any two stations no such interchange took place, while the interchange might be effected after the particular comparative measures were secured.

NOTE BY MAJOR (NOW LIEUTENANT-COLONEL) HERSCHEL.—The view of this subject here presented by Mr. Schott, in this last paragraph, is so sensibly correct that only a strong conviction that it does not meet the whole case, and is directly opposed to the principle of inviolability which I wish to see recognized, would tempt me to add to this discussion. We are agreed, and universal practice shows it to have been widely recognized, that invariability must be maintained during at least the whole course of a series of differential determinations. [In the East Indian series, for instance, it was maintained during eight or nine years, and at more than thirty installations.] No one pretends to set any limit, either to the time or to the number of stations, which is to restrict a series of differential measures. But it is said, "when the series is completed there is no longer any need to guard or preserve the pendulum from change; its work is done." But it is just at this point, I contend, that we ought, on the contrary, to be growing more and more solicitous for the protection of the pendulum. The more stations it has visited, the more intimate is our knowledge of its time of vibration, or vibration-number, or whatever be the function we may adopt by which the results of observation are to be expressed. Even if, at the time, only one of the stations visited was a "known" station, we ought yet to contemplate and anticipate the time when, by the superposition of later series, the fundamental vibration-number (*i.e.*, its equatorial vibration-number) shall rest on more than one, perhaps on many known stations. Even if such considerations as these fail to convince, some weight will surely be conceded to the argument that, as one continuous series is better than two or more, covering the same stations, and as by merely guarding the pendulum stringently during the temporary pause between two sets of operations, otherwise called series, these will in fact constitute one only, it is right to take the proper precautions to bring this about. I confess I am surprised, not that this principle has not been acted upon, in times past, but that it should at this day need more than the most cursory enunciation, and that we are even now debating whether we shall not continue to throw away one-half of the net results of each set of observations with invariable pendulums. We do no less, when we break off a series and, by interchange of knife-edges, interrupt the continuity of a series.]

X. Four classes of errors affect the observed period of oscillation, as follows:

1st. Those which are nearly constant throughout the work at any one station. Such arise, for example, from the flexure of the support, from an error in the adopted coëfficient of expansion at a tropical or arctic station, and from other causes.

2nd. Those which remain nearly constant for a considerable time, say an hour or a day, but which vary from day to day. Such arise, for example, from the knife resting differently on the supports on different days, from erroneous determinations of temperature, or from similar causes.

3rd. Those which are continually varying throughout the observations.

4th. Those which arise from errors in the comparison of the pendulum with the time-piece.

The first class of errors demand the most solicitous scrutiny. The other three classes may be distinguished by the study of the residuals of the observations. The third class is the most important of the last three.

Further insight into the nature of the errors is obtained by comparing the residuals with large and with small arcs, and by comparing the residuals of the reversible pendulum in its two positions.

XI. Small arcs and heavy pendulums are to be recommended.

[Major HERSCHEL. In recommending "small arcs," Mr. Peirce leaves us to guess what magnitude he contemplates. In setting out upon my recent experience, I intended to swing in arcs as small as I could anyway *see* them, certainly below 30″. But I found that both Sir G. Airy and Professor Stokes were strongly opposed to such a course, and I abandoned the intention in favor of arcs falling from say 70′ to 7′. The objections urged were all theoretical. I should still advocate the practical testing of the doubt in any series of observations of an experimental character.

Mr. PEIRCE. I find the errors of observation are not increased by continuing the oscillation down to arcs of 1′.]

XII. The method of coincidences as perfected by Major Herschel is to be recommended, especially in connection with a clock whose pendulum swings from knives.

XIII. The experiments should be continued for twenty-four hours, beginning and ending with star observations, when this is convenient. But this should not be absolutely required.

XIV. The swinging *in vacuo* is to be recommended.

[Major Herschel dissented.]

XV. The flexure of the support may be advantageously avoided by swinging two pendulums simultaneously on the same support with opposite phases. When this is not done the flexures should be measured, and in doing this the measures must be made at the middle of the knife-support or else the position of the instantaneous axis of motion must be determined.

XVI. The separation of the atmospheric effect into two parts is requisite for an exact temperature correction.

XVII. The influence of atmospheric moisture ought to be studied.

XVIII. The use of rollers in place of knives is to be condemned.

XIX. The probable accidental error of a determination of gravity must not exceed 5 millionths ($\frac{1}{200000}$), and the total which may reasonably be feared must not exceed 10 millionths ($\frac{1}{100000}$).

[Professor Newcomb and others agreed to this, but Major Herschel and Mr. Schott objected to any numerical criterion of this sort.]

XX. A good gravimeter is an important desideratum.

Resolutions

P 260: 516

1. The main object of pendulum research is the determination of the figure of the earth. From a sufficient number of observations suitably distributed over the surface of the earth the actual figure may be determined.

2. A complete geodetic survey should include determinations of the intensity of gravity. These determinations should be made at as many critical points of local deflection and physical structure within the area of the survey as possible; and these should be combined with others distributed over the whole globe.

3. A minute gravimetric survey of some limited region is at present of such interest as to justify its execution.

4. Extended linear gravimetric exploration is desirable, to be ultimately followed by similar work distributed over large areas.

5. Each series of such determinations should be made with the same apparatus, so that the differential results should not be affected by constant errors peculiar to the apparatus.

6. While it is inadvisable at present to strictly fix a numerical limit of the permissible probable error of pendulum work, yet such determinations ought commonly to be accurate to the $\frac{1}{200000}$ part.

7. Since different pendulums may be used in different regions, all should be compared at some central station.

8. Determinations of absolute gravity will probably prove useful in comparing the yard and the metre, and they should at any rate be made in order to test the constancy of gravity against the constancy of length of a metallic bar.

9. In the present state of our experience, unchanged pendulums are decidedly to be preferred for ordinary explorations.

Introductory Lecture on the Study of Logic

P 225: **Johns Hopkins University Circulars**
2:19 (November 1882): 11–12.
(Presented September 1882)

It might be supposed that logic taught that much was to be accomplished by mere rumination, though every one knows that experiment, observation, comparison, active scrutiny of facts, is what is wanted, and that mere *thinking* will accomplish nothing even in mathematics. Logic had certainly been defined as the "art of thinking," and as the "science of the normative laws of thought." But those are not true definitions. *"Dyalectica,"* says the logical text-book of the middle ages, *"est ars artium et scientia scientiarum, ad omnium aliarum scientiarum methodorum principia viam habens,"* and although the logic of our day must naturally be utterly different from that of the Plantagenet epoch, yet this general conception that it is the *art of devising methods of research,*—the *method of methods,*—is the true and worthy idea of the science. Logic will not undertake to inform you what kind of experiments you ought to make in order best to determine the acceleration of gravity, or the value of the Ohm; but it will tell you how to proceed to form a plan of experimentation.

It is impossible to maintain that the superiority of the science of the moderns over that of the ancients is due to anything but a better *logic.* No one can think that the Greeks were inferior to any modern people whatever in natural aptitude for science. We may grant that their opportunities for research were less; and it may be said that ancient astronomy could make no progress beyond the Ptolemaic system until sufficient time had elapsed to prove the insufficiency of Ptolemy's tables. The ancients could have no dynamics so long as no important dynamical problem had presented itself; they could have no theory of heat without the steam-engine, etc. Of course, these causes had their influence, and of course they were not the main reason of the defects of the ancient civilization. Ten years' astronomical observations with instruments such as the ancients could have constructed would have sufficed to overthrow the old astronomy. The great mechanical discoveries of Galileo were made with no apparatus to speak of. If, in any direction whatever, the ancients had once commenced research by right methods, opportunities for new advances would have been brought along in the train of those that went before. But read the logical treatise of Philodemus; see how he strenuously argues that inductive reasoning is not utterly without value, and you see where the fault lay. When such an elementary point as that needed serious argumentation it is clear that the conception of scientific method was almost entirely wanting.

Modern methods have created modern science; and this century, and especially the last twenty-five years, have done more to create new methods than any former equal period. We live in the very age of methods. Even mathematics and astronomy have put on new faces. Chemistry and physics are on completely new tracks. Linguistic, history, mythology, sociology, biology, are all getting studied in new ways. Jurisprudence and law have begun to feel the impulse, and must in the future be more and more rapidly influenced by it.

This is the age of methods; and the university which is to be the exponent of the living condition of the human mind, must be the university of methods.

Now I grant you that to say that this is the age of the development of new methods of research is so far from saying that it is the age of the theory of methods, that it is almost to say the reverse. Unfortunately practice generally precedes theory, and it is the usual fate of mankind to get things done in some boggling way first, and find out afterward how they could have been done much more easily and perfectly. And it must be confessed that we students of the science of modern methods are as yet but a voice crying in the wilderness, and saying prepare ye the way for this lord of the sciences which is to come.

Yet even now we can do a little more than that. The theory of any act in no wise aids the doing of it, so long as what is to be done is of a narrow description, so that it can be governed by the unconscious part of our organism. For such purposes, rules of thumb or no rules at all are the best. You cannot play billiards by analytical mechanics nor keep shop by political economy. But when new paths have to be struck out, a spinal cord is not enough; a brain is needed, and that brain an organ of mind, and that mind perfected by a liberal education. And a liberal education—so far as its relation to the understanding goes—means *logic.* That is indispensible to it, and no other one thing is.

I do not need to be told that science consists of specialties. I know all that, for I belong to the guild of science, have learned one of its trades and am saturated with its current notions. But in my judgment there are scientific men, all whose training has only served to belittle them, and I do not see that a mere scientific specialist stands intellectually much higher than an artisan. I am quite sure that a young man who spends his time exclusively in the laboratory of physics or chemistry or biology, is in danger of profiting but little more from his work than if he were an apprentice in a machine shop.

The scientific specialists—pendulum swingers and the like—are doing a great and useful work; each one very little, but altogether something vast. But the higher places in science in the coming years are for those who succeed in adapting the methods of one science to the investigation of another. That is what the greatest progress of the passing generation has consisted in. Darwin adapted to biology the methods of Malthus and the economists; Maxwell adapted to the theory of gases the methods of the doctrine of chances, and to electricity the methods of hydrodynamics. Wundt adapts to psychology the methods of physiology; Galton adapts to the same study the methods of the theory of errors; Morgan adapted to history a method from biology; Cournot adapted to political economy the calculus of variations. The philologists have adapted to their science the methods of the decipherers of dispatches. The astronomers have learned the methods of chemistry; radiant heat is investigated with an ear trumpet; the mental temperament is read off on a vernier.

Now although a man needs not the theory of a method in order to apply it as it has been applied already, yet in order to adapt to his own science the method of another with which he is less familiar, and to properly modify it so as to suit it to its new use, an acquaintance with the principles upon which it depends will be of the greatest benefit. For that sort of work a man needs to be more than a mere specialist; he needs such a general training of his mind, and such knowledge as shall show him how to make his powers most effective in a new direction. That knowledge is logic.

In short, if my view is the true one, a young man wants a physical education and an aesthetic education, an education in the ways of the world and a moral education, and with all these logic has nothing in particular to do; but so far as he wants an intellectual education, it is precisely logic that he wants; and whether he be in one lecture-room or another, his ultimate purpose is to improve his logical power and his knowledge of methods. To this great end a young man's attention ought to be directed when he first comes to the university; he ought to keep it steadily in view during the whole period of his studies; and finally, he will do well to review his whole work in the light which an education in logic throws upon it.

I should be the very first to insist that logic can never be learned from logic-books or logic lectures. The material of positive science must form its basis and its vehicle. Only relatively little could be done by the lecturer on method even were he master of the whole circle of the sciences. Nevertheless, I do think that I can impart to you something of real utility, and that the theory of method will shed much light on all your other studies.

The impression is rife that success in logic requires a mathematical head. But this is not true. The habit of looking at questions in a mathematical way is, I must say, of great advantage, and thus a turn for mathematics is of more or less service in any science, physical or moral. But no brilliant talent for mathematics is at all necessary for the study of logic.

The course which I am to give this year begins with some necessary preliminaries upon the theory of cognition. For it is requisite to form a clear idea at the outset of what knowledge consists of, and to consider a little what are the operations of the mind by which it is produced. But I abridge this part of the course as much as possible, partly because it will be treated by other instructors, and partly because I desire to push on to my main subject, the method of science.

I next take up syllogism, the lowest and most rudimentary of all forms of reasoning, but very fundamental because it is rudimentary. I treat this after the general style of De Morgan, with references to the old traditional doctrine. Next comes the logical algebra of Boole, a subject in itself extremely easy, but very useful both from a theoretical point of view and also as giving a method of solving certain rather frequently occurring and puzzling problems. From this subject, I am naturally led to the consideration of relative terms. The logic of relatives, so far as it has been investigated, is clear and easy, and at the same time it furnishes the key to many of the difficulties of logic, and has already served as the instrument of some discoveries in mathematics. An easy application of this branch of logic is to the doctrine of breadth and depth or the relations between objects and characters. I next introduce the conception of number, and after showing how to treat certain statistical problems, I take up the doctrine of chances. A very simple and elegant mathematical method of treating equations of finite differences puts the student into possession of a powerful instrument for the solution of all problems of probability that do not import difficulties extraneous to the theory of probability itself.

We thus arrive at the study of that kind of probable inference that is really distinctive; that is to say, Induction in its broadest sense—Scientific Reasoning. The general theory of the subject is carefully worked out with the aid of real examples in great variety, and rules for the performance of the operation are given. These rules have not been picked up by hazard, nor are they merely such as experience recommends; they are deduced methodically from the general theory.

Finally, it is desirable to illustrate a long concatenation of scientific inferences. For this purpose we take up Kepler's great work, *De Motibus Stellae Martis,* the greatest piece of inductive reasoning ever produced. Owing to the admirable and exceptional manner in which the work is written, it is possible to follow Kepler's whole course of investigation from beginning to end, and to show the application of all the maxims of induction already laid down.

In order to illustrate the method of reasoning about a subject of a more metaphysical kind, I shall then take up the scientific theories of the constitution of matter.

Last of all, I shall give a few lectures to show what are the lessons that a study of scientific procedure teaches with reference to philosophical questions, such as the conception of causation and the like.

On a Class of Multiple Algebras

***P 224:* Johns Hopkins University Circulars**
2:19 (November 1882): 3–4.
(Presented 18 October 1882)

It is evident that every substitution is a relative term. Thus, the transposition of AB to BA is in relative form $(A:B) + (B:A)$, and the circular substitution $\binom{BCA}{ABC}$ is $(B:A) + (C:B) + (A:C)$. In this point of view, we see that substitutions may be added and multiplied by scalars, although the results will usually no longer be substitutions. A group of substitutions may, then, be linear expressions in an associative multiple algebra of a lower order than that of the group.

The object of this paper is to show what algebras express all the substitutions of two, of three, and of four letters; and to put these algebras into familiar forms.

Of two letters, there are two substitutions $(X:X) + (Y:Y)$ and $(X:Y) + (Y:X)$. We may denote these by α and β respectively, so that taking A and B as indeterminate coëfficients, the general expression of the algebra is $A\alpha + B\beta$, or in the form of a matrix is

$$\begin{matrix} A B \\ B A. \end{matrix}$$

Assume i and j such that

$$i = \tfrac{1}{2}(\alpha + \beta)$$

$$j = \tfrac{1}{2}(\alpha - \beta).$$

Then the multiplication table of i and j is as follows:

	i	j
i	i	0
j	0	j

The algebra is, therefore, a mixture of two ordinary simple algebras of B. Peirce's form (a_1).

Of three letters, there are *six* substitutions. Let A, B, Γ, Δ, E, Z be indeterminate coëfficients. Then, the general expression of the algebra is equivalent to

$$\begin{matrix} \mathrm{A} & \mathrm{B} & \Gamma \\ \Gamma & \mathrm{A} & \mathrm{B} \\ \mathrm{B} & \Gamma & \mathrm{A} \end{matrix} + \begin{matrix} \Delta & \mathrm{E} & \mathrm{Z} \\ \mathrm{E} & \mathrm{Z} & \Delta \\ \mathrm{Z} & \Delta & \mathrm{E} \end{matrix}$$

Or, denoting the six substitutions by $\alpha, \beta, \gamma, \delta, \epsilon, \zeta$, we may write the general expression as $\mathrm{A}\alpha + \mathrm{B}\beta + \Gamma\gamma + \Delta\delta + \mathrm{E}\epsilon + \mathrm{Z}\zeta$. There is an equation between these substitutions, namely:

$$\alpha + \beta + \gamma = \delta + \epsilon + \zeta.$$

Assume 5 relatives h, i, j, k, l, such that

$$\begin{aligned} h &= \tfrac{1}{3}(\alpha + \beta + \gamma) \\ i &= \tfrac{1}{2}(\alpha - \epsilon) \\ j &= \tfrac{1}{3}(\beta - \gamma + \delta - \zeta) \\ k &= \tfrac{1}{4}(-\beta + \gamma + \delta - \zeta) \\ l &= \tfrac{1}{6}(2\alpha - \beta - \gamma - \delta + 2\epsilon - \zeta). \end{aligned}$$

In matricular form,

$$h = \begin{matrix} 1 & 1 & 1 \\ 1 & 1 & 1 \\ 1 & 1 & 1 \end{matrix} \qquad 2i = \begin{matrix} 1 & -1 & 0 \\ -1 & 1 & 0 \\ 0 & 0 & 0 \end{matrix} \qquad 3j = \begin{matrix} 1 & 1 & -2 \\ -1 & -1 & +2 \\ 0 & 0 & 0 \end{matrix} \qquad 4k = \begin{matrix} 1 & -1 & 0 \\ 1 & -1 & 0 \\ -2 & +2 & 0 \end{matrix}$$

$$6l = \begin{matrix} 1 & 1 & -2 \\ 1 & 1 & -2 \\ -2 & -2 & +4. \end{matrix}$$

The multiplication table of h, i, j, k, l, is as follows:

	h	i	j	k	l
h	h	0	0	0	0
i	0	i	j	0	0
j	0	0	0	i	j
k	0	k	l	0	0
l	0	0	0	k	l

The algebra is a mixture of ordinary single algebra (a_1) with the algebra of Hamilton's biquaternions (g_4).

In six letters, there are twenty-four substitutions. Using the capital Greek letters for indeterminate coëfficients, the general linear expression in these substitutions is equivalent to

$$\begin{array}{cccc} \mathrm{A} & \Delta & \Gamma & \mathrm{B} \\ \Gamma & \mathrm{B} & \mathrm{A} & \Delta \\ \mathrm{B} & \Gamma & \Delta & \mathrm{A} \\ \Delta & \mathrm{A} & \mathrm{B} & \Gamma \end{array} + \begin{array}{cccc} \mathrm{E} & \mathrm{Z} & \Theta & \mathrm{H} \\ \mathrm{Z} & \mathrm{E} & \mathrm{H} & \Theta \\ \Theta & \mathrm{H} & \mathrm{E} & \mathrm{Z} \\ \mathrm{H} & \Theta & \mathrm{Z} & \mathrm{E} \end{array} + \begin{array}{cccc} \mathrm{I} & \Lambda & \mathrm{K} & \mathrm{M} \\ \mathrm{M} & \mathrm{K} & \Lambda & \mathrm{I} \\ \Lambda & \mathrm{I} & \mathrm{M} & \mathrm{K} \\ \mathrm{K} & \mathrm{M} & \mathrm{I} & \Lambda \end{array}$$

$$+ \begin{array}{cccc} \mathrm{N} & \mathrm{O} & \Pi & \Xi \\ \mathrm{O} & \mathrm{N} & \Xi & \Pi \\ \Xi & \Pi & \mathrm{O} & \mathrm{N} \\ \Pi & \Xi & \mathrm{N} & \mathrm{O} \end{array} + \begin{array}{cccc} \mathrm{P} & \Upsilon & \Sigma & \mathrm{T} \\ \Sigma & \mathrm{T} & \mathrm{P} & \Upsilon \\ \Upsilon & \mathrm{P} & \mathrm{T} & \Sigma \\ \mathrm{T} & \Sigma & \Upsilon & \mathrm{P} \end{array} + \begin{array}{cccc} \Phi & \mathrm{X} & \Psi & \Omega \\ \Omega & \Psi & \mathrm{X} & \Phi \\ \Psi & \Omega & \Phi & \mathrm{X} \\ \mathrm{X} & \Phi & \Omega & \Psi \end{array}$$

Using the twenty-four small Greek letters to denote the twenty-four substitutions, so that the general linear expression is $A\alpha + B\beta +$, etc., an attentive observation of the above scheme will show that the following equations subsist:

$$\alpha + \beta + \gamma + \delta = \epsilon + \zeta + \eta + \theta = \iota + \kappa + \lambda + \mu$$

$$= \nu + \xi + o + \pi = \rho + \sigma + \tau + \upsilon = \varphi + \chi + \psi + \omega.$$

Also,

$$\begin{array}{lll} \alpha + \beta = \nu + \xi & \epsilon + \zeta = \nu + o & \iota + \kappa = \nu + \pi \\ \alpha + \gamma = \rho + \sigma & \epsilon + \eta = \rho + \tau & \iota + \lambda = \rho + \upsilon \\ \alpha + \delta = \varphi + \chi & \epsilon + \theta = \varphi + \psi & \iota + \mu = \varphi + \omega. \end{array}$$

It is plain that these equations are all independent, and not difficult to see that there are no more. Since they are fourteen in number, a ten-fold algebra is required to express the twenty-four substitutions.

Assume the ten relatives $h, i, j, k, l, m, n, o, p, q$, such that

$$\begin{aligned} h &= \tfrac{1}{4}(\alpha + \beta + \gamma + \delta) \\ i &= \tfrac{1}{4}(\epsilon + \zeta - \eta - \theta) \\ j &= \tfrac{1}{4}(\alpha - \beta + \gamma - \delta) \\ k &= \tfrac{1}{4}(\iota - \kappa - \lambda + \mu) \\ l &= \tfrac{1}{4}(\iota - \kappa + \lambda - \mu) \\ m &= \tfrac{1}{4}(\epsilon - \zeta - \eta + \theta) \\ n &= \tfrac{1}{4}(\alpha + \beta - \gamma - \delta) \\ o &= \tfrac{1}{4}(\alpha - \beta - \gamma + \delta) \\ p &= \tfrac{1}{4}(\iota + \kappa - \lambda - \mu) \\ q &= \tfrac{1}{4}(\epsilon - \zeta + \eta - \theta). \end{aligned}$$

In matricular form, these are as follows, (where + is written for + 1 and − for −1).

$$4h = \begin{matrix} + & + & + & + \\ + & + & + & + \\ + & + & + & + \\ + & + & + & + \end{matrix}$$

$$4i = \begin{matrix} + & + & - & - \\ + & + & - & - \\ - & - & + & + \\ - & - & + & + \end{matrix} \qquad 4j = \begin{matrix} + & - & + & - \\ + & - & + & - \\ - & + & - & + \\ - & + & - & + \end{matrix} \qquad 4k = \begin{matrix} + & - & - & + \\ + & - & - & + \\ - & + & + & - \\ - & + & + & - \end{matrix}$$

$$4l = \begin{matrix} + & + & - & - \\ - & - & + & + \\ + & + & - & - \\ - & - & + & + \end{matrix} \qquad 4m = \begin{matrix} + & - & + & - \\ - & + & - & + \\ + & - & + & - \\ - & + & - & + \end{matrix} \qquad 4n = \begin{matrix} + & - & - & + \\ - & + & + & - \\ + & - & - & + \\ - & + & + & - \end{matrix}$$

$$4o = \begin{matrix} + & + & - & - \\ - & - & + & + \\ - & - & + & + \\ + & + & - & - \end{matrix} \qquad 4p = \begin{matrix} + & - & + & - \\ - & + & - & + \\ - & + & - & + \\ + & - & + & - \end{matrix} \qquad 4q = \begin{matrix} + & - & - & + \\ - & + & + & - \\ - & + & + & - \\ + & - & - & + \end{matrix}$$

The multiplication table is as follows:

	h	*i*	*j*	*k*	*l*	*m*	*n*	*o*	*p*	*q*
h	*h*	0	0	0	0	0	0	0	0	0
i	0	*i*	*j*	*k*	0	0	0	0	0	0
j	0	0	0	0	*i*	*j*	*k*	0	0	0
k	0	0	0	0	0	0	0	*i*	*j*	*k*
l	0	*l*	*m*	*n*	0	0	0	0	0	0
m	0	0	0	0	*l*	*m*	*n*	0	0	0
n	0	0	0	0	0	0	0	*l*	*m*	*n*
o	0	*o*	*p*	*q*	0	0	0	0	0	0
p	0	0	0	0	*o*	*p*	*q*	0	0	0
q	0	0	0	0	0	0	0	*o*	*p*	*q*

The algebra is, therefore, a mixture of ordinary algebra with that of my nonions.

[Of course, the representation of quaternions as linear expressions in substitutions of three letters, the sum of the coëfficients being zero, is equivalent to finding *a theorem of plane geometry corresponding to each theorem of solid geometry expressible by quaternions.* For instance, let the three letters which are interchanged by the coördinates x, y, z, of a point in space. Then, as above, let

$$\alpha = \begin{pmatrix} x,y,z \\ x,y,z \end{pmatrix} \qquad \beta = \begin{pmatrix} z,x,y \\ x,y,z \end{pmatrix} \qquad \gamma = \begin{pmatrix} y,z,x \\ x,y,z \end{pmatrix}$$

$$\delta = \begin{pmatrix} x,z,y \\ x,y,z \end{pmatrix} \qquad \epsilon = \begin{pmatrix} y,x,z \\ x,y,z \end{pmatrix} \qquad \zeta = \begin{pmatrix} z,y,x \\ x,y,z \end{pmatrix}.$$

Thus, β and γ represent the operations of rotation through one-third of a circumference, the one forward, the other backward, about an axis passing through the origin and the point (1, 1, 1); while δ, ϵ, ζ represent three perversions with reference to axes passing through the origin and the points (0, 1, -1), (1, -1, 0), and (1, 0, -1), respectively. Quaternions may be represented thus:

$$1 = \frac{1}{3}(2\alpha - \beta - \gamma)$$

$$i = -\frac{1}{3}(2\epsilon - \delta - \zeta)\sqrt{-1}$$

$$j = \frac{1}{\sqrt{3}}(\beta - \gamma)$$

$$k = \frac{1}{\sqrt{3}}(\delta - \zeta)\sqrt{-1}.$$

We have here a new geometrical interpretation of quaternions. Since the sum of the coëfficients of the substitutions is equal to zero in the values of every one of the quaternion elements, it follows that under this interpretation any quaternion operating upon any point brings it into the plane.

$$x + y + z = 0.$$

Hence, every quaternion equation has an interpretation relating to points in this plane. The reason why a quaternion, which has a fourfold multiplicity, is no more than adequate to expressing operations upon points in space, is that the operations are of such a nature that different ones may have the same effect upon single points. But a *real* quaternion has no greater multiplicity than the real and imaginary points of a plane; and the geometrical effects of different real quaternions upon points in the plane $x + y + z = 0$ under the new interpretation are different upon all points except the origin.

For the axes of x, y, z, in trilinear coördinates, take three lines meeting in one point and equally inclined to one another. To plot the point $x = a + b\sqrt{-1}$, $y = c + d\sqrt{-1}$, $z = e + f\sqrt{-1}$, plot the point $x = a$, $y = c$, $z = e$ in *blue*, and the point $x = b$, $y = d$, $z = f$ in *red*. Then the effects of the different quaternion elements upon points in the plane $x + y + z = 0$ are as follows: 1 leaves every point unchanged. The vector i reverses the position of a blue point with reference to the line $z = 0$ and changes it to red, and reverses the position of a red point with reference to the line $x = y$ and changes it to blue. The vector j rotates every point through a quadrant round the origin in the direction from $x = 0$ to $y = z$, without changing the color. The vector k reverses the position of a blue point with reference to the line through the origin that bisects the angle between $y = 0$ and $y = z$ and changes it to red, and reverses the position of a red point with reference to the line through the origin that bisects the angle between $x = 0$ and $x = z$ and changes it to blue.—*Added*, Oct. 30.]

On Irregularities in the Amplitude of Oscillation of Pendulums

P 218: **American Journal of Science and Arts,**
3rd ser. 24 (October 1882): 254–55

The pendulum experiments conducted by me for the Coast Survey exhibit considerable differences in the rate of descent of the arc on different days. Mr. O. T. Sherman (this *Journal,* xxiv, 176) having suggested that this might be due to a periodic variation of the amplitude, I feel called upon to say,—what a study of my published observations will show,—that this supposition is inadmissible. For, in order to account, in this manner, for the observed discrepancies, it would be necessary to suppose a periodic variation too great to escape direct observation. In most of my observations, I have used an arc accurately divided into thousandths of the radius. The reading telescope has a sidereal magnifying power of from 60 to 150 diameters, and is usually placed at a distance of about fifteen feet. The observations are made by placing the wire of this telescope successively in coincidence with the different lines of the graduated arc, and accurately noting the moment at which the point of the pendulum is just bisected by the wire at the extremity of its swing. It would thus be quite impossible to overlook a variation of the amplitude amounting to one ten-thousandth of the radius, while that of the pendulum was, say one-thirtieth of the radius.

The motion of a pendulum upon a flexible support has two harmonic constituents. One of these has nearly the natural period of the pendulum, the other nearly the natural period of the oscillation of the support. If the amplitude of the second motion is sensible, an irregularity of the arc of oscillation, often of a plainly periodic character, will necessarily result. The ratio of the amplitude of the second harmonic motion to that of the first depends upon the manner in which the pendulum is started; and upon a very flexible stand it is easy to start the pendulum in such a way as to produce a considerable variation in the amplitude. But theory shows that if the pendulum be started by pushing it to one side by a force applied at the centre of oscillation and then letting it go, the second harmonic constituent vanishes. Now, this is the manner in which I always endeavor to start a pendulum. That, in point of fact, the second harmonic constituent is insensible is shown by the fact that it hardly shows itself even in the oscillation of the support, where it is relatively many times larger than in that of the pendulum. The equations showing this are given in my paper appended to the report of the Stuttgart meeting of the International Geodetic Association.

Mr. Sherman deduces the consequences which would result from the motion of the support having a different period from that of the pendulum. But for the considerable number of supports that I have examined in this respect, the mean period of the oscillation of support and pendulum have been the same. This is proved by the fact that, however long the experiment is continued, the oscillations of the one and of the other appear to be synchronous. There may, it is true, be a portion of the motion of the stand which is not synchronous with the main part of the motion of the pendulum; but this circumstance will have no appreciable effect on the period of the pendulum, if the latter is properly started.

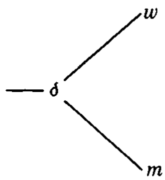
Of the periodic phenomena observed by Mr. Sherman, I can propose no explanation, because I am unacquainted with the details of his experiments. But similar phenomena might result from a faulty mode of starting a pendulum upon a very flexible support.

*[*On Junctures and Fractures in Logic*]*

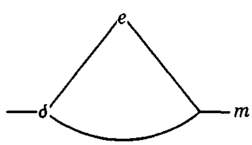
MS 427: Fall–Winter 1882

[. . .] essay; because if we are to use enveloped expressions to signify propositions, then all terms are to be considered in reference to their sole use, which is to be joined together to form such an enveloped, or let us say *entire,* expression. Now, since every such enveloped expression is its own correlate, it follows that we must not consider the relate (as the notation referred to does) as being any more different from the correlates than the different correlates of a plural relative are from one another. We must consider a dual relative simply as a fragment broken from the middle of an enveloped expression having two *fractures,* or points where it has been broken off and where it can again be joined to other fragments to make up a new entire expression. So a non-relative term (or the converse of one) is to be considered simply as the fragment from the end of an entire expression having two *fractures;* and a plural relative is a fragment having three or more such fractures or points of possible junction. Just so, in chemistry, we have univalent, bivalent, trivalent, etc. atoms presenting one, two, three, etc. points where they can be joined to others to make up a complete molecule possessing no such fracture. Chemistry here offers us two suggestions. First, it suggests that instead of subscript numbers, lines extending from one symbol to another should be used to distinguish arrangement of the junctions; and second, it suggests that to constitute an entire expression, all that is necessary is that there should be no fracture unfilled, whether univalent fragments enter into its composition or not. Applying these ideas, we first note that we have to distinguish the relate and correlate of a dual relative, the relate, first correlate, and second correlate of a triple expression, etc. This we may do by dividing an

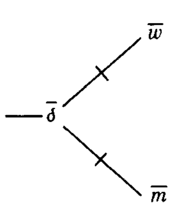
imaginary circle about the symbol of the term into as many parts as there are fractures beginning at the left hand point and going round in the direction of the hands of a clock, and supposing the different fractures to be at the beginning of the different divisions. Thus, if *w* denote 'woman', we may write 'denouncer to a woman of a man' thus



To write δem, 'denouncer of a man to an enemy of him', we have to introduce the triple relative of identity 'identical at once with ——— and with ———'. The symbol for this is (1,) but the symbol may be omitted. Then we write

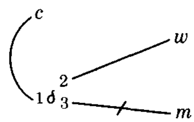


We have next to observe that there are two modes of junction, by relative multiplication and by relative addition. The first may be represented by a simple line of connection; the second by a line crossed by a short mark. Thus, 'what does not denounce any man to a woman' may be written

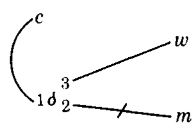


One mode of combination is to be regarded as the negative of the other; and the rule for taking the negative of any expression is to negative every letter and every junction in it.

When both modes of combination occur in the same expression the order of their application is material, and may be indicated either by parentheses or by little numbers applied wherever convenient. Thus, putting *c* for 'child',



means 'Everything except men is denounced by some child to some woman', while



means 'There is a certain woman to whom every *[. . .]*

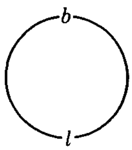
Letter, Peirce to O. H. Mitchell

L 294: 21 December 1882

St. Augustine Fl.
1882 Dec 21

My dear Mitchell

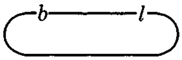
The notation of the logic of relatives can be somewhat simplified by spreading the formulae over two dimensions. For instance suppose we write



to express the proposition that something is at once benefactor and lover of something. That is,

$$\Sigma_x \Sigma_y b_{xy} l_{xy} > 0.$$

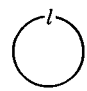
We can write



to mean that something is at once benefactor and loved of something, that is, something is benefactor of a lover of itself

$$\Sigma_x \Sigma_y b_{xy} l_{yx} > 0$$

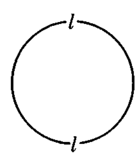
Then



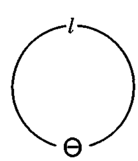
will mean that something is a lover of itself

$$\Sigma_x l_{xx} > 0$$

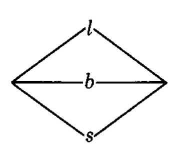
The expression



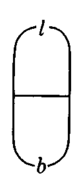
might be used to mean that something is a lover of something. Or we could write this



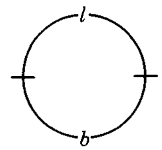
using Θ as the negative of 0 instead of ∞. We can write



to express that something is lover, benefactor, & servant of something. A triple relative of identity here enters, but the symbol (1,) is omitted. With the use of the same relative, we write



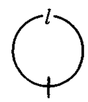
something is at once lover and benefactor of itself. We may also write



to mean that everything is either a lover or a benefactor of everything.

$$\Pi_x \Pi_y (l_{xy} + b_{xy}) > 0.$$

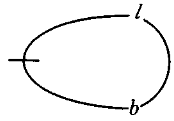
In the same way



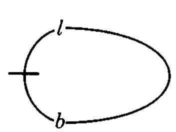
will mean that everything is a lover of itself

$$\Pi_x l_{xx} > 0.$$

The order of attachment of like bonds is immaterial, that of unlike bonds is material. We can use shorter and straighter lines to represent later attached bonds. Thus

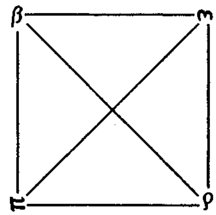


will mean $\Sigma_y \Pi_x (l_{xy} + b_{xy}) > 0$ that is, there is something of which everything is either lover or benefactor; while



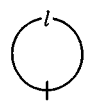
will mean $\Pi_x \Sigma_y (l_{xy} b_{xy}) > 0$ or everything is at once lover and benefactor of something or other.

We thus do away with the distinction of relative & non-relative operations, by discarding the latter altogether. It is true we might do this well enough with any notation that was adequate for triple relatives, but it is precisely the advantage of the spread formulae that they make the treatment of triple relatives easy. For instance let β be betrayer to ——— of ———, δ denouncer to ——— of ———, ϵ excuser to ——— of ———, Π preferrer to ——— of ———. The lines representing the relates and two correlates are to be the 1st 2nd and 3rd respectively going round the letter in the direction of the hands of a watch beginning at the back. Then

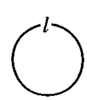


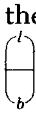
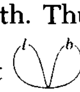
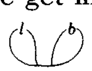
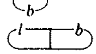
will express the proposition that there are six objects related to one another in a way easily read from the diagram.

The following are some of the rules of procedure. From

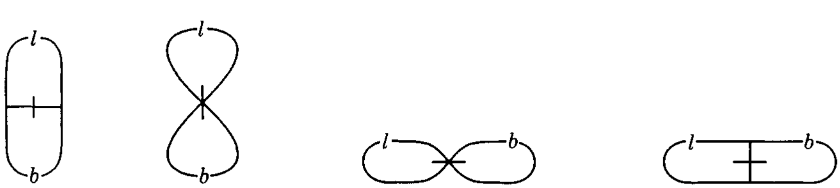


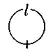
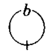
we can (assuming that something exists) infer

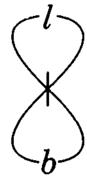


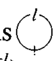
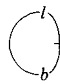
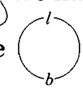
The shortest bond in an entire proposition can be reduced to a point and if there are then two loops one of them can be twisted and the bond again be given a certain length. Thus, suppose we have . We get first  and then by a twist  from which we get either  or 

So we have the following all equivalent.

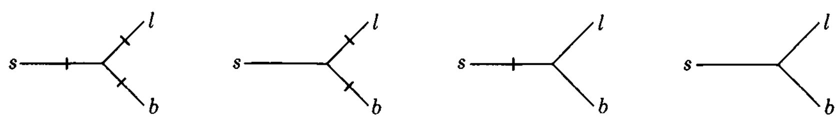


The two propositions   can be welded into one thus

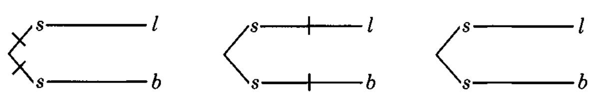


From the two propositions   we infer . From  we have the immediate inference  and from  the inference 

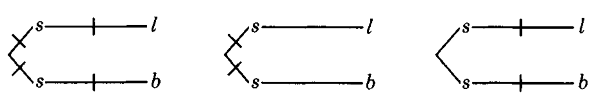
Wherever there is a split, thus:



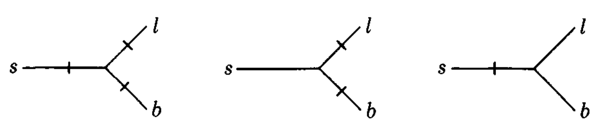
we can except in the first case continue the split thus



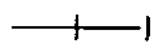
On the other hand from



we can infer



Other formulae are similar to those of the ordinary algebra of relatives such as that the fragment s—l—+—b can be replaced by s——l + b. The negative may be represented thus l——+——b.

means *lnb.*  may always be erased.  may be replaced by the negative.

yours faithfully
C. S. Peirce

[Beginnings of a Logic Book]

MS 443: Winter–Spring 1883

What is logic? It cannot be expected that this question should be satisfactorily answered in advance of the study of the subject. Yet thus much may be said, that logic has a practical aim, to teach us how to reach the truth.

It has always been a question whether logic is a science or an art. In our day, especially in Germany where the most original and singular systems of logic have been produced, very many logicians would deny that logic has essentially any practical aim. Yet historically it is certain that this view has been predominant, on the whole. There was so little logic before Aristotle that he is justly regarded as the father of the doctrine; and with him the practical aim is apparent. In the middle ages, when the greatest attention was paid to the study, every boy learned his logic from the *Summulae Logicales* of Petrus Hispanus (Pope John XXI), and this book gives the following definition of logic, "Dyalectica est ars artium, scientia scientiarum, ad omnium methodorum principia viam habens." Logic is the art relating to arts, the science relating to sciences, which holds the road to the principles of all methods. One of the most valuable and certainly the most popular of modern treatises on the subject is entitled *L'Art de penser.*

In view of the historical facts, then, it may be said with confidence that logic is a doctrine which centres about a certain practical aim which gives to the subject whatever unity it possesses.

What that aim is, is a question which can only be answered by introducing a logical distinction. The investigation of truth consists, according to the conception of logic, of two parts: *observation* and *reasoning.* This distinction is not in truth an absolute one. Modern psychology shows us that there is no such thing as pure observation free from reasoning nor as pure reasoning without any observational element. But taking reasoning to mean that part of our intellectual operations which is voluntary, we may say that logic is the art of reasoning rightly, or that it is a science which centres about this art.

If I am asked whether the study of logic really makes a man reason better, I am obliged to confess that in most cases it has the directly opposite result. It makes a captious reasoner, who appears to himself and others to reason in a superior manner, who is consecutive in his thoughts, self-conscious, free from contradictions, but whose thought is not nearly so good as that of the perfectly untutored person for the purpose of finding out what is true. The average uneducated woman has a mind far better adapted to that purpose than the average graduate of Oxford; and the reason seems to be that the latter has been sophisticated with logic,—not directly from treatises but from conversation and reading. The young man who makes a course in logic with a feeling of satisfaction at having gained something, has by that token certainly got nothing but a mental morbid diathesis of which he is very unlikely ever to be cured. It is the object of this little book, so to inoculate the student with an innocent form of the malady that he may be exempt from any malignant seizure.

There are various schools of logic now active. They may be roughly classed as students of Formal Logic, of the theory of cognition, of transcendental logic, and of scientific methodology. These are really so many different sciences or parts of one science and are not essentially hostile. Formal logic is occupied with syllogism etc. An important branch of this subject is logical algebra. See De Morgan's *Syllabus of Logic*. The theory of cognition is a mixed doctrine, chiefly psychological. Überweg's *Logic* is an example. Transcendental Logic follows mainly Hegel. Everett's *Logic* may be recommended. Scientific Methodology has been treated by various English authors, as Whewell, J. S. Mill, and Jevons.

[On Propositions]

MS 444: Winter–Spring 1883

Every proposition has three elements. 1^{st} an indication of the universe to which it relates, 2^{nd} its general terms, 3^{d} the connection of its terms.

This needs explanation. I speak of a proposition. But it is to be understood that proposition, judgment, and belief are logically equivalent (though in other respects different) and since the proposition is more tangible it is more convenient, in formal logic, to speak of that. First, as to the universe. Every proposition relates to something which can only be pointed out or designated but cannot be specified in general terms. "No admittance except on business," over a door is a general proposition; but it relates to that door which may have no qualities different from those of some other door in some other planet or in some other tridimensional space of which there may be any number scattered through a quinquidimensional continuum without anywhere touching one another. But the hanging of the sign over this door indicates that this is the one referred to. The indescribable but designatable object to which a proposition refers always has connected with it a variety of possibilities, often an endless variety. In the example we have taken, these possibilities are all the actions that can have a relation to that door. The proposition declares that among all these actions there is not to be found any permitted passage through the door except on business. So if I say "Hamlet's purposes were sometimes undecided," I refer to the fictitious world created by Shakespeare. This world cannot be described in general terms, for another just like it might issue from some other poet's brain, or exist in reality. In that world I am contemplating the variety of modifications of Hamlet's consciousness and I declare that among them there are to be found undecided purposes. Again the proposition "all men are mortal," refers to the actual universe, which cannot be described in general terms, for 'actual' or 'existent', as Kant has shown, is not a general description. The countless objects in the universe, past and future, may be considered as so many possibilities in this sense that any one of them is one of the things in which a given character as 'immortal humanity' might be found. Using possibility in this sense, the universe of a proposition may be defined as a series of possibilities to which the proposition refers but whose limits cannot be described in general terms but can only be indicated in some other way.

A proposition, as Professor Mitchell has shown, may relate to several such universes. This is the case with the last example, which declares that whatever object in the actual universe you take, it is either not a man *[. . .]*

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STUDIES IN LOGIC

Preface

P 268a: Boston:
Little, Brown, & Company, 1883: iii–vi

These papers, the work of my students, have been so instructive to me, that I have asked and obtained permission to publish them in one volume.

Two of them, the contributions of Miss Ladd (now Mrs. Fabian Franklin) and of Dr. Mitchell, present new developments of the logical algebra of Boole. Miss Ladd's article may serve, for those who are unacquainted with Boole's *Laws of Thought*, as an introduction to the most wonderful and fecund discovery of modern logic. The followers of Boole have altered their master's notation, mainly in three respects.

1st. A series of writers, Jevons in 1864, Peirce in 1867, Grassmann in 1872, Schröder in 1877, and McColl in 1877, successively and independently declared in favor of using the sign of addition to unite different terms into one aggregate, whether they be mutually exclusive or not. Thus, we now write

European + Republican

to stand for all Europeans and republicans taken together, without intending to count twice over the European republicans. Boole and Venn (his sole living defender) would insist upon our writing

European + Non-European Republican

or

Non-Republican European + Republican.

The two new authors both side with the majority in this respect.

2nd. Mr. McColl and I find it to be absolutely necessary to add some new sign to express *existence;* for Boole's notation is only capable of representing that some description of thing does *not* exist, and cannot say that anything *does* exist. Besides that, the sign of equality, used by Boole in the desire to assimilate the algebra of logic to that of number, really expresses, as De Morgan showed forty years ago, a complex relation. To say that

$$\text{African} = \text{Negro}$$

implies two things, that every African is a negro and that every negro is an African. For these reasons, Mr. McColl and I make use of signs of inclusion and of non-inclusion. Thus, I write

$$\text{Griffin} \prec \text{breathing fire}$$

to mean that every griffin (if there be such a creature) breathes fire, i.e., no griffin not breathing fire exists; and I write

$$\text{Animal} \overline{\prec} \text{Aquatic}$$

to mean that some animals are not aquatic, or that a non-aquatic animal does exist. Mr. McColl's notation is not essentially different.

Miss Ladd and Mr. Mitchell also use two signs expressive of simple relations involving existence and non-existence; but in their choice of these relations they diverge both from McColl and me and from one another. In fact, of the eight simple relations of terms signalized by De Morgan, Mr. McColl and I have chosen two, Miss Ladd two others, Dr. Mitchell a fifth and sixth. The logical world is thus in a situation to weigh the advantages and disadvantages of the different systems.

3rd. The third important modification of Boole's original notation consists in the introduction of new signs so as to adapt it to the expression of relative terms. This branch of logic which has been studied by Leslie Ellis, De Morgan, Joseph John Murphy, Alexander MacFarlane and myself, presents a rich and new field for investigation. A part of Dr. Mitchell's paper touches this subject in an exceedingly original and interesting way.

The method of using the Boolian calculus,—already greatly simplified by Schröder and by McColl,—receives still further improvements at the hands both of Miss Ladd and of Dr. Mitchell, and it is surprizing to see with what facility their methods yield solutions of problems more intricate and difficult than any that have hitherto been proposed.

The volume contains two other papers relating to deductive logic.

In one of these, Mr. Gilman develops those rules for the combination of relative numbers of which the general principles of probabilities are special cases. In the other, Dr. Marquand shows how a counting machine on a binary system of numeration will exhibit De Morgan's eight modes of universal syllogism.

There are besides two papers upon inductive logic. In the first, Dr. Marquand explains the deeply interesting views of the Epicureans, known to us mainly through the work of Philodemus, *περὶ σημείων καὶ σημειώσεων*, which exists in a fragmentary state in a Herculaneum papyrus.

The other paper is one which, at the desire of my students, I have contributed to the collection. It contains a statement of what appear to me to be the true theory of the inductive process and the correct maxims for the performance of it. I hope that the thoughts that a long study has suggested to me may be found not altogether useless to those who occupy themselves with the application of this kind of reasoning.

I have to thank the Trustees of the Johns Hopkins University, for a very liberal contribution toward the expenses of this publication.

BALTIMORE, Dec. 12, 1882.

A Theory of Probable Inference

P 268b: 126–81

I.

The following is an example of the simplest kind of probable inference:—

About two per cent of persons wounded in the liver recover;
This man has been wounded in the liver:
Therefore, there are two chances out of a hundred that he will recover.

Compare this with the simplest of syllogisms, say the following:—

Every man dies;
Enoch was a man:
Hence, Enoch must have died.

The latter argument consists in the application of a general rule to a particular case. The former applies to a particular case a rule not absolutely universal, but subject to a known proportion of exceptions. Both may alike be termed deductions, because they bring information about the uniform or usual course of things to bear upon the solution of special questions; and the probable argument may approximate indefinitely to demonstration as the ratio named in the first premise approaches to unity or to zero.

Let us set forth the general formulæ of the two kinds of inference in the manner of formal logic.

FORM I.

Singular Syllogism in Barbara.

Every M is a P;
S is an M:
Hence, S is a P.

FORM II.

Simple Probable Deduction.

The proportion ρ of the M's are P's;
S is an M:
It follows, with probability ρ, that S is a P.

It is to be observed that the ratio ρ need not be exactly specified. We may reason from the premise that not more than two per cent of persons wounded in the liver recover, or from "not less than a certain proportion of the M's are P's," or from "no very large nor very small proportion, etc." In short, ρ is subject to every kind of indeterminacy; it simply excludes some ratios and admits the possibility of the rest.

The analogy between syllogism and what is here called probable deduction is certainly genuine and important; yet how wide the differences between the two modes of inference are, will appear from the following considerations:—

1. The logic of probability is related to ordinary syllogistic as the

quantitative to the qualitative branch of the same science. Necessary syllogism recognizes only the inclusion or non-inclusion of one class under another; but probable inference takes account of the proportion of one class which is contained under a second. It is like the distinction between projective geometry, which asks whether points coincide or not, and metric geometry, which determines their distances.

2. For the existence of ordinary syllogism, all that is requisite is that we should be able to say, in some sense, that one term is contained in another, or that one object stands to a second in one of those relations: "better than," "equivalent to," etc., which are termed *transitive* because if A is in any such relation to B, and B is in the same relation to C, then A is in that relation to C. The universe might be all so fluid and variable that nothing should preserve its individual identity, and that no measurement should be conceivable; and still one portion might remain inclosed within a second, itself inclosed within a third, so that a syllogism would be possible. But probable inference could not be made in such a universe, because no signification would attach to the words "quantitative ratio." For that there must be counting; and consequently units must exist, preserving their identity and variously grouped together.

3. A cardinal distinction between the two kinds of inference is, that in demonstrative reasoning the conclusion follows from the existence of the objective facts laid down in the premises; while in probable reasoning these facts in themselves do not even render the conclusion probable, but account has to be taken of various subjective circumstances,—of the manner in which the premises have been obtained, of there being no countervailing considerations, etc.; in short, good faith and honesty are essential to good logic in probable reasoning.

When the partial rule that the proportion ρ of the M's are P's is applied to show with probability ρ that S is a P, it is requisite, not merely that S should *be* an M, but also that it should be an instance drawn *at random* from among the M's. Thus, there being four aces in a picquet pack of thirty-two cards, the chance is one-eighth that a given card not looked at is an ace; but this is only on the supposition that the card has been drawn at random from the whole pack. If, for instance, it had been drawn from the cards discarded by the players at picquet or euchre, the probability would be quite different. The instance must be drawn at random. Here is a maxim of conduct. The volition of the reasoner (using what machinery it may) has to choose *S* so that it shall be an *M;* but he ought to restrain himself from all further preference, and not allow his will to act in any way that might tend to settle what particular *M* is taken, but should leave that to the operation of chance. Willing and wishing, like other operations of the mind, are *general* and imperfectly determinate. I wish for a horse, —for some particular kind of horse perhaps, but not usually for any individual one. I will to act in a way of which I have a general conception; but so long as my action conforms to that general description, how it is further determined I do not care. Now in choosing the instance *S*, the general intention (including the whole plan of action) should be to select an *M,* but beyond that there should be no preference; and the act of choice should be such that if it were repeated many enough times with the same intention, the result would be that among the totality of selections the different sorts of *M*'s would occur with the same relative frequencies as in experiences in which volition does not intermeddle at all. In cases in which it is found difficult thus to restrain the will by a direct effort, the apparatus of games of chance,—a lottery-wheel, a roulette, cards, or dice, —may be called to our aid. Usually, however, in making a simple probable deduction, we take that instance in which we happen at the time to be interested. In such a case, it is our interest that fulfils the function of an apparatus for random selection; and no better need be desired, so long as we have reason to deem the premise "the proportion ρ of the *M*'s are *P*'s" to be equally true in regard to that part of the *M*'s which are alone likely ever to excite our interest.

Nor is it a matter of indifference in what manner the other premise has been obtained. A card being drawn at random from a picquet pack, the chance is one-eighth that it is an ace, if we have no other knowledge of it. But after we have looked at the card, we can no longer reason in that way. That the conclusion must be drawn in advance of any other knowledge on the subject is a rule that, however elementary, will be found in the sequel to have great importance.

4. The conclusions of the two modes of inference likewise differ. One is necessary; the other only probable. Locke, in the *Essay concerning Human Understanding,* hints at the correct analysis of the nature of probability. After remarking that the mathematician positively knows that the sum of the three angles of a triangle is equal to two right angles because he apprehends the geometrical proof, he

then continues: "But another man who never took the pains to observe the demonstration, hearing a mathematician, a man of credit, affirm the three angles of a triangle to be equal to two right ones, *assents* to it; that is, receives it for true. In which case, the foundation of his assent is the probability of the thing, the proof being such as, for the most part, carries truth with it; the man on whose testimony he receives it not being wont to affirm anything contrary to or besides his knowledge, especially in matters of this kind." Those who know Locke are accustomed to look for more meaning in his words than appears at first glance. There is an allusion in this passage to the fact that a probable argument is always regarded as belonging to a *genus* of arguments. This is, in fact, true of any kind of argument. For the belief expressed by the conclusion is determined or caused by the belief expressed by the premises. There is, therefore, some general rule according to which the one succeeds the other. But, further, the reasoner is conscious of there being such a rule, for otherwise he would not know he was reasoning, and could exercise no attention or control; and to such an involuntary operation the name reasoning is very properly not applied. In all cases, then, we are conscious that our inference belongs to a general class of logical forms, although we are not necessarily able to describe the general class. The difference between necessary and probable reasoning is that in the one case we conceive that such facts as are expressed by the premises are never, in the whole range of possibility, true, without another fact, related to them as our conclusion is to our premises, being true likewise; while in the other case we merely conceive that, in reasoning as we do, we are following a general maxim that will usually lead us to the truth.

So long as there are exceptions to the rule that all men wounded in the liver die, it does not necessarily follow that because a given man is wounded in the liver he cannot recover. Still, we know that if we were to reason in that way, we should be following a mode of inference which would only lead us wrong, in the long run, once in fifty times; and this is what we mean when we say that the probability is one out of fifty that the man will recover. To say, then, that a proposition has the probability ρ means that to infer it to be true would be to follow an argument such as would carry truth with it in the ratio of frequency ρ.

It is plainly useful that we should have a stronger feeling of confidence about a sort of inference which will oftener lead us to the truth than about an inference that will less often prove right,—and such a sensation we do have. The celebrated law of Fechner is, that as the force acting upon an organ of sense increases in geometrical progression, the intensity of the sensation increases in arithmetical progression. In this case the odds (that is, the ratio of the chances in favor of a conclusion to the chances against it) take the place of the exciting cause, while the sensation itself is the feeling of confidence. When two arguments tend to the same conclusion, our confidence in the latter is equal to the sum of what the two arguments separately would produce; the *odds* are the product of the *odds* in favor of the two arguments separately. When the value of the *odds* reduces to unity, our confidence is null; when the *odds* are less than unity, we have more or less confidence in the negative of the conclusion.

II.

The principle of probable deduction still applies when S, instead of being a single M, is a set of M's,—n in number. The reasoning then takes the following form:—

FORM III.

Complex Probable Deduction.

Among all sets of n M's, the proportion q consist each of m P's and of $n - m$ not-P's;
S, S', S'', etc. form a set of n objects drawn at random from among the M's:
Hence, the probability is q that among S, S', S'', etc. there are m P's and $n - m$ not-P's.

In saying that S, S', S'', etc. form a set drawn at random, we here mean that not only are the different individuals drawn at random, but also that they are so drawn that the qualities which may belong to one have no influence upon the selection of any other. In other words, the individual drawings are independent, and the set as a whole is taken at random from among all possible sets of n M's. In strictness, this supposes that the same individual may be drawn several times in the same set, although if the number of M's is large compared with n, it makes no appreciable difference whether this is the case or not.

The following formula expresses the proportion, among all sets of n M's, of those which consist of m P's and $n - m$ not-P's. The letter

r denotes the proportion of P's among the M's, and the sign of admiration is used to express the continued product of all integer numbers from 1 to the number after which it is placed. Thus, $4! = 1 \cdot 2 \cdot 3 \cdot 4 = 24$, etc. The formula is

$$q = n! \times \frac{r^m}{m!} \times \frac{(1-r)^{n-m}}{(n-m)!}.$$

As an example, let us assume the proportion $r = \frac{2}{3}$ and the number of M's in a set $n = 15$. Then the values of the probability q for different numbers, m, of P's, are fractions having for their common denominator 14,348,907, and for their numerators as follows:—

m	Numerator of q.
0	1
1	30
2	420
3	3640
4	21840
5	96096
6	320320
7	823680

m	Numerator of q.
8	1667360
9	2562560
10	3075072
11	2795520
12	1863680
13	860160
14	122880
15	32768

A very little mathematics would suffice to show that, r and n being fixed, q always reaches its maximum value with that value of m that is next less than $(n + 1)r$,[1] and that q is very small unless m has nearly this value.

Upon these facts is based another form of inference to which I give the name of statistical deduction. Its general formula is as follows:—

FORM IV.

Statistical Deduction.

The proportion r of the M's are P's;
S', S'', S''', etc., are a *numerous* set, taken at random from among the M's:
Hence, *probably and approximately*, the proportion r of the S's are P's.

As an example, take this:—

1. In case $(n + 1)r$ is a whole number, q has equal values for $m = (n + 1)r$ and for $m = (n + 1)r - 1$.

A little more than half of all human births are males:
Hence, probably a little over half of all the births in New York during any one year are males.

We have now no longer to deal with a mere probable inference, but with a *probable approximate* inference. This conception is a somewhat complicated one, meaning that the probability is greater according as the limits of approximation are wider, conformably to the mathematical expression for the values of q.

This conclusion has no meaning at all unless there be more than one instance; and it has hardly any meaning unless the instances are somewhat numerous. When this is the case, there is a more convenient way of obtaining (not exactly, but quite near enough for all practical purposes) either a single value of q or the sum of successive values from $m = m_1$ to $m = m_2$ inclusive. The rule is first to calculate two quantities which may conveniently be called t_1 and t_2 according to these formulæ:—

$$t_1 = \frac{m_1 - (n+1)r}{\sqrt{2nr(1-r)}} \qquad t_2 = \frac{1 + m_2 - (n+1)r}{\sqrt{2nr(1-r)}}$$

where $m_2 > m_1$. Either or both the quantities t_1 and t_2 may be negative. Next with each of these quantities enter the table below, and take out $\frac{1}{2}\Theta t_1$ and $\frac{1}{2}\Theta t_2$ and give each the same sign as the t from which it is derived. Then

$$\Sigma q = \frac{1}{2}\Theta t_2 - \frac{1}{2}\Theta t_1 .$$

$$\textit{Table of } \Theta t = \frac{2}{\sqrt{\pi}}\int_0^t e^{-t^2}\,dt.$$

t	Θt	t	Θt	t	Θt
0.0	0.000	1.0	$0.84\overline{3}$	2.0	0.99532
0.1	0.112	1.1	0.880	2.1	0.99702
0.2	$0.22\overline{3}$	1.2	0.910	2.2	$0.9981\overline{4}$
0.3	$0.32\overline{9}$	1.3	0.934	2.3	$0.9988\overline{6}$
0.4	0.428	1.4	0.952	2.4	0.99931
0.5	0.520	1.5	0.966	2.5	0.99959
0.6	$0.60\overline{4}$	1.6	0.976	2.6	0.99976
0.7	$0.67\overline{8}$	1.7	$0.98\overline{4}$	2.7	$0.9998\overline{7}$
0.8	0.742	1.8	0.989	2.8	0.99992
0.9	$0.79\overline{7}$	1.9	$0.99\overline{3}$	2.9	0.99996
1.0	$0.84\overline{3}$	2.0	0.995	3.0	$0.9999\overline{8}$

t	Θ
4	0.999999989
5	0.9999999999984
6	0.999999999999999982
7	0.9999999999999999999958

In rough calculations we may take Θt equal to t for t less than 0.7, and as equal to *unity* for any value above $t = 1.4$.

III.

The principle of statistical deduction is that two proportions,—namely, that of the P's among the M's, and that of the P's among the S's,—are probably and approximately equal. If, then, this principle justifies our inferring the value of the second proportion from the known value of the first, it equally justifies our inferring the value of the first from that of the second, if the first is unknown but the second has been observed. We thus obtain the following form of inference:—

FORM V.

Induction.

S', S'', S''', etc., form a numerous set taken at random from among the M's;
S', S'', S''', etc., are found to be—the proportion ρ of them—P's:
Hence, *probably* and *approximately* the same proportion, ρ, of the M's are P's.

The following are examples. From a bag of coffee a handful is taken out, and found to have nine-tenths of the beans perfect; whence it is inferred that about nine-tenths of all the beans in the bag are probably perfect. The United States Census of 1870 shows that of native white children under one year old, there were 478,774 males to 463,320 females; while of colored children of the same age there were 75,985 males to 76,637 females. We infer that generally there is a larger proportion of female births among negroes than among whites.

When the ratio ρ is *unity* or *zero*, the inference is an ordinary induction; and I ask leave to extend the term induction to all such inference, whatever be the value of ρ. It is, in fact, inferring from a sample to the whole lot sampled. These two forms of inference, statistical deduction and induction, plainly depend upon the same principle of equality of ratios, so that their validity is the same. Yet the nature of the probability in the two cases is very different. In the statistical deduction, we know that among the whole body of *M*'s the proportion of *P*'s is ρ; we say, then, that the *S*'s being random drawings of *M*'s are probably *P*'s in about the same proportion,—and though this may happen not to be so, yet at any rate, on continuing the drawing sufficiently, our prediction of the ratio will be vindicated at last. On the other hand, in induction we say that the proportion ρ of the sample being *P*'s, probably there is about the same proportion in the whole lot; or at least, if this happens not to be so, then on continuing the drawings the inference will be, not *vindicated* as in the other case, but *modified* so as to become true. The deduction, then, is probable in this sense, that though its conclusion may in a particular case be falsified, yet similar conclusions (with the same ratio ρ) would generally prove approximately true; while the induction is probable in this sense, that though it may happen to give a false conclusion, yet in most cases in which the same precept of inference was followed, a different and approximately true inference (with the right value of ρ) would be drawn.

IV.

Before going any further with the study of Form V, I wish to join to it another extremely analogous form.

We often speak of one thing being very much like another, and thus apply a vague quantity to resemblance. Even if qualities are not subject to exact numeration, we may conceive them to be approximately measurable. We may then measure resemblance by a scale of numbers from zero up to unity. To say that *S* has a 1-likeness to a *P* will mean that it has every character of a *P*, and consequently *is* a *P*. To say that it has a 0-likeness will imply total dissimilarity. We shall then be able to reason as follows:—

FORM II(*bis*).

Simple probable deduction in depth.

Every *M* has the simple mark *P*;
The *S*'s have an *r*-likeness to the *M*'s:
Hence, the probability is *r* that every *S* is *P*.

It would be difficult, perhaps impossible, to adduce an example of such kind of inference, for the reason that *simple marks* are not known to us. We may, however, illustrate the complex probable deduction in depth (the general form of which it is not worth while to set down) as follows: I forget whether, in the ritualistic churches, a bell is tinkled at the elevation of the Host or not. Knowing, however, that the services resemble somewhat decidedly those of the Roman Mass, I think that it is not unlikely that the bell is used in the ritualistic, as in the Roman, churches.

We shall also have the following:—

FORM IV(*bis*).

Statistical deduction in depth.

Every M has, for example, the numerous marks P', P'', P''', etc.
S has an r-likeness to the M's:
Hence, probably and approximately, S has the proportion r of the marks P', P'', P''', etc.

For example, we know that the French and Italians are a good deal alike in their ideas, characters, temperaments, genius, customs, institutions, etc., while they also differ very markedly in all these respects. Suppose, then, that I know a boy who is going to make a short trip through France and Italy; I can safely predict that among the really numerous though relatively few respects in which he will be able to compare the two people, about the same degree of resemblance will be found.

Both these modes of inference are clearly deductive. When $r = 1$, they reduce to Barbara.[2]

Corresponding to induction, we have the following mode of inference:—

2. When $r = 0$, the last form becomes

M has all the marks P;
S has no mark of M:
Hence, S has none of the marks P.

When the universe of marks is unlimited (see a note appended to this paper for an explanation of this expression), the only way in which two terms can fail to have a common mark is by their together filling the universe of things; and consequently this form then becomes,

M is P;
Every non-S is M:
Hence, every non-S is P.

This is one of De Morgan's syllogisms.

In putting $r = 0$ in Form II (*bis*) it must be noted that, since P is simple in depth, to say that S is not P is to say that it has no mark of P.

FORM V(*bis*).

Hypothesis.

M has, for example, the numerous marks P', P'', P''', etc.
S has the proportion r of the marks P', P'', P''', etc.:
Hence, probably and approximately, S has an r-likeness to M.

Thus, we know, that the ancient Mound-builders of North America present, in all those respects in which we have been able to make the comparison, a limited degree of resemblance with the Pueblo Indians. The inference is, then, that in all respects there is about the same degree of resemblance between these races.

If I am permitted the extended sense which I have given to the word "induction," this argument is simply an induction respecting qualities instead of respecting things. In point of fact P', P'', P''', etc. constitute a random sample of the characters of M, and the ratio r of them being found to belong to S, the same ratio of all the characters of M are concluded to belong to S. This kind of argument, however, as it actually occurs, differs very much from induction, owing to the impossibility of simply counting qualities as individual things are counted. Characters have to be weighed rather than counted. Thus, antimony is bluish-gray: that is a character. Bismuth is a sort of rose-gray; it is decidedly different from antimony in color, and yet not so very different as gold, silver, copper, and tin are.

I call this induction of characters *hypothetic inference*, or, briefly, *hypothesis*. This is perhaps not a very happy designation, yet it is difficult to find a better. The term "hypothesis" has many well established and distinct meanings. Among these is that of a proposition believed in because its consequences agree with experience. This is the sense in which Newton used the word when he said, *Hypotheses non fingo.* He meant that he was merely giving a general formula for the motions of the heavenly bodies, but was not undertaking to mount to the causes of the acceleration they exhibit. The inferences of Kepler, on the other hand, were hypotheses in this sense; for he traced out the miscellaneous consequences of the supposition that Mars moved in an ellipse, with the sun at the focus, and showed that both the longitudes and the latitudes resulting from this theory were such as agreed with observation. These two components of the motion were observed; the third, that of approach to or regression from the earth, was supposed. Now, if in Form V(*bis*) we put $r = 1$, the inference is the drawing of a hypothesis in this sense. I take the liberty of extending the use of the word by permitting *r* to have any value from zero to unity. The term is certainly not all that could be desired; for the word hypothesis, as ordinarily used, carries with it a suggestion of uncertainty, and of something to be superseded, which does not belong at all to my use of it. But we must use existing language as best we may, balancing the reasons for and against any mode of expression, for none is perfect; at least the term is not so utterly misleading as "analogy" would be, and with proper explanation it will, I hope, be understood.

V.

The following examples will illustrate the distinction between statistical deduction, induction, and hypothesis. If I wished to order a font of type expressly for the printing of this book, knowing, as I do, that in all English writing the letter *e* occurs oftener than any other letter, I should want more *e*'s in my font than other letters. For what is true of all other English writing is no doubt true of these papers. This is a statistical deduction. But then the words used in logical writings are rather peculiar, and a good deal of use is made of single letters. I might, then, count the number of occurrences of the different letters upon a dozen or so pages of the manuscript, and thence conclude the relative amounts of the different kinds of type required in the font. That would be inductive inference. If now I were to order the font, and if, after some days, I were to receive a box containing a large number of little paper parcels of very different sizes, I should naturally infer that this was the font of types I had ordered; and this would be hypothetic inference. Again, if a dispatch in cipher is captured, and it is found to be written with twenty-six characters, one of which occurs much more frequently than any of the others, we are at once led to suppose that each character represents a letter, and that the one occurring so frequently stands for *e*. This is also hypothetic inference.

We are thus led to divide all probable reasoning into deductive and ampliative, and further to divide ampliative reasoning into induction and hypothesis. In deductive reasoning, though the predicted ratio may be wrong in a limited number of drawings, yet it will be approximately verified in a larger number. In ampliative reasoning the ratio may be wrong, because the inference is based on but a limited number of instances; but on enlarging the sample the ratio will be changed till it becomes approximately correct. In induction, the instances drawn at random are numerable things; in hypothesis they are characters, which are not capable of strict enumeration, but have to be otherwise estimated.

This classification of probable inference is connected with a preference for the copula of inclusion over those used by Miss Ladd and by Mr. Mitchell.[3] De Morgan established eight forms of simple propositions; and from a purely formal point of view no one of these has a right to be considered as more fundamental than any other. But formal logic must not be too purely formal; it must represent a fact of psychology, or else it is in danger of degenerating into a mathematical recreation. The categorical proposition, "every man is mortal," is but a modification of the hypothetical proposition, "if humanity, then mortality;" and since the very first conception from which logic springs is that one proposition follows from another, I hold that "if *A*, then *B*" should be taken as the typical form of judgment. Time flows; and, in time, from one state of belief (represented by the premises of an argument) another (represented by its conclusion) is developed. Logic arises from this circumstance, without which we could not learn anything nor correct any opinion. To say that an inference is correct is to say that if the premises are true the conclusion is also true; or that every possible state of things in which the premises should be true would be included among the possible states of things in which the conclusion would be true. We are thus led to the copula of inclusion. But the main characteristic of the relation of inclusion is that it is transitive,—that is, that what is included in something included in anything is itself included in that thing; or, that if *A* is *B* and *B* is *C*, then *A* is *C*. We thus get *Barbara* as the primitive type of inference. Now in Barbara we have a *Rule*, a *Case* under the *Rule*, and the inference of the *Result* of that rule in that case. For example:—

Rule.	All men are mortal;
Case.	Enoch was a man:
Result.	Enoch was mortal.

The cognition of a rule is not necessarily conscious, but is of the nature of a habit, acquired or congenital. The cognition of a case is of the general nature of a sensation; that is to say, it is something

3. I do not here speak of Mr. Jevons, because my objection to the copula of identity is of a somewhat different kind.

which comes up into present consciousness. The cognition of a result is of the nature of a decision to act in a particular way on a given occasion.[4] In point of fact, a syllogism in Barbara virtually takes place when we irritate the foot of a decapitated frog. The connection between the afferent and efferent nerve, whatever it may be, constitutes a nervous habit, a rule of action, which is the physiological analogue of the major premise. The disturbance of the ganglionic equilibrium, owing to the irritation, is the physiological form of that which, psychologically considered, is a sensation; and, logically considered, is the occurrence of a case. The explosion through the efferent nerve is the physiological form of that which psychologically is a volition, and logically the inference of a result. When we pass from the lowest to the highest forms of innervation, the physiological equivalents escape our observation; but, psychologically, we still have, first, habit,—which in its highest form is understanding, and which corresponds to the major premise of *Barbara;* we have, second, feeling, or present consciousness, corresponding to the minor premise of *Barbara;* and we have, third, volition, corresponding to the conclusion of the same mode of syllogism. Although these analogies, like all very broad generalizations, may seem very fanciful at first sight, yet the more the reader reflects upon them the more profoundly true I am confident they will appear. They give a significance to the ancient system of formal logic which no other can at all share.

Deduction proceeds from Rule and Case to Result; it is the formula of Volition. Induction proceeds from Case and Result to Rule; it is the formula of the formation of a habit or general conception, —a process which, psychologically as well as logically, depends on the repetition of instances or sensations. Hypothesis proceeds from Rule and Result to Case; it is the formula of the acquirement of secondary sensation,—a process by which a confused concatenation of predicates is brought into order under a synthetizing predicate.

We usually conceive Nature to be perpetually making deductions in Barbara. This is our natural and anthropomorphic metaphysics. We conceive that there are Laws of Nature, which are her Rules or major premises. We conceive that Cases arise under these laws; these

4. See my paper on "How to Make Our Ideas Clear."—*Popular Science Monthly,* January, 1878.

cases consist in the predication, or occurrence, of *causes*, which are the middle terms of the syllogisms. And, finally, we conceive that the occurrence of these causes, by virtue of the laws of Nature, results in effects which are the conclusions of the syllogisms. Conceiving of nature in this way, we naturally conceive of science as having three tasks,—(1) the discovery of Laws, which is accomplished by induction; (2) the discovery of Causes, which is accomplished by hypothetic inference; and (3) the prediction of Effects, which is accomplished by deduction. It appears to me to be highly useful to select a system of logic which shall preserve all these natural conceptions.

It may be added that, generally speaking, the conclusions of Hypothetic Inference cannot be arrived at inductively, because their truth is not susceptible of direct observation in single cases. Nor can the conclusions of Inductions, on account of their generality, be reached by hypothetic inference. For instance, any historical fact, as that Napoleon Bonaparte once lived, is a hypothesis; we believe the fact, because its effects—I mean current tradition, the histories, the monuments, etc.—are observed. But no mere generalization of observed facts could ever teach us that Napoleon lived. So we inductively infer that every particle of matter gravitates toward every other. Hypothesis might lead to this result for any given pair of particles, but it never could show that the law was universal.

VI.

We now come to the consideration of the Rules which have to be followed in order to make valid and strong Inductions and Hypotheses. These rules can all be reduced to a single one; namely, that the statistical deduction of which the Induction or Hypothesis is the inversion, must be valid and strong.

We have seen that Inductions and Hypotheses are inferences from the conclusion and one premise of a statistical syllogism to the other premise. In the case of hypothesis, this syllogism is called the *explanation*. Thus in one of the examples used above, we suppose the cryptograph to be an English cipher, because, as we say, this *explains* the observed phenomena that there are about two dozen characters, that one occurs more frequently than the rest, especially at the ends of words, etc. The explanation is,—

Simple English ciphers have certain peculiarities;
This is a simple English cipher:
Hence, this necessarily has these peculiarities.

This explanation is present to the mind of the reasoner, too; so much so, that we commonly say that the hypothesis is adopted *for the sake of* the explanation. Of induction we do not, in ordinary language, say that it explains phenomena; still, the statistical deduction, of which it is the inversion, plays, in a general way, the same part as the explanation in hypothesis. From a barrel of apples, that I am thinking of buying, I draw out three or four as a sample. If I find the sample somewhat decayed, I ask myself, in ordinary language, not "Why is this?" but "How is this?" And I answer that it probably comes from nearly all the apples in the barrel being in bad condition. The distinction between the "Why" of hypothesis and the "How" of induction is not very great; both ask for a statistical syllogism, of which the observed fact shall be the conclusion, the known conditions of the observation one premise, and the inductive or hypothetic inference the other. This statistical syllogism may be conveniently termed the explanatory syllogism.

In order that an induction or hypothesis should have any validity at all, it is requisite that the explanatory syllogism should be a valid statistical deduction. Its conclusion must not merely follow from the premises, but follow from them upon the principle of probability. The inversion of *ordinary* syllogism does not give rise to an induction or hypothesis. The statistical syllogism of Form IV is invertible, because it proceeds upon the principle of an approximate *equality* between the ratio of *P*'s in the whole class and the ratio in a well-drawn sample, and because equality is a convertible relation. But ordinary syllogism is based upon the property of the relation of containing and contained, and that is not a convertible relation. There is, however, a way in which ordinary syllogism may be inverted; namely, the conclusion and either of the premises may be interchanged by negativing each of them. This is the way in which the indirect, or apagogical,[5] figures of syllogism are derived from the first, and in which the *modus tollens* is derived from the *modus ponens.* The following schemes show this:—

5. From ἀπαγωγὴ εἰς τὸν ἀδύνατον, Aristotle's name for the *reductio ad absurdum.*

First Figure.

Rule.	All M is P;
Case.	S is M:
Result.	S is P.

Second Figure.

Rule.	All M is P;
Denial of Result.	S is not P:
Denial of Case.	S is not M.

Third Figure.

Denial of Result.	S is not P;
Case.	S is M:
Denial of Rule.	Some M is not P.

Modus Ponens.

Rule.	If A is true, C is true;
Case.	In a certain case A is true:
Result.	∴ In that case C is true.

Modus Tollens.

Rule. If A is true, C is true;
Denial of Result. In a certain case C is not true:
Denial of Case. ∴ In that case A is not true.

Modus Innominatus.

Case. In a certain case A is true;
Denial of Result. In that case C is not true:
Denial of Rule. ∴ If A is true, C is is not necessarily true.

Now suppose we ask ourselves what would be the result of thus apagogically inverting a statistical deduction. Let us take, for example, Form IV:—

> The S's are a numerous random sample of the M's;
> The proportion r of the M's are P's:
> Hence, probably about the proportion r of the S's are P's.

The ratio r, as we have already noticed, is not necessarily perfectly definite; it may be only known to have a certain maximum or minimum; in fact, it may have any kind of indeterminacy. Of all possible values between 0 and 1, it admits of some and excludes others. The logical negative of the ratio r is, therefore, itself a ratio, which we may name ρ; it admits of every value which r excludes, and excludes every value of which r admits. Transposing, then, the major premise and conclusion of our statistical deduction, and at the same time denying both, we obtain the following inverted form:—

The S's are a numerous random sample of the M's;
The proportion ρ of the S's are P's:
Hence, probably about the proportion ρ of the M's are P's.[6]

But this coincides with the formula of Induction. Again, let us apagogically invert the statistical deduction of Form IV(*bis*). This form is,—

Every M has, for example, the numerous marks P', P'', P''', etc.
S has an r-likeness to the M's:
Hence, probably and approximately, S has the proportion r
of the marks P', P'', P''', etc.

Transposing the minor premise and conclusion, at the same time denying both, we get the inverted form,—

Every M has, for example, the numerous marks P', P'', P''', etc.
S has the proportion ρ of the marks P', P'', P''', etc.:
Hence, probably and approximately, S has a ρ-likeness
to the class of M's.

This coincides with the formula of Hypothesis. Thus we see that Induction and Hypothesis are nothing but the apagogical inversions of statistical deductions. Accordingly, when r is taken as 1, so that ρ is "less than 1," or when r is taken as 0, so that ρ is "more than 0," the induction degenerates into a syllogism of the third figure and the hypothesis into a syllogism of the second figure. In these special cases, there is no very essential difference between the mode of reasoning in the direct and in the apagogical form. But, in general, while the probability of the two forms is precisely the same,—in this sense, that for any fixed proportion of P's among the M's (or of marks of S's among the marks of the M's) the probability of any given error in the concluded value is precisely the same in the indirect as it is in the direct form,—yet there is this striking difference, that a multiplication of instances will in the one case *confirm*, and in the other *modify*, the concluded value of the ratio.

We are thus led to another form for our rule of validity of ampliative inference; namely, instead of saying that the *explanatory* syllo-

6. The conclusion of the statistical deduction is here regarded as being "the proportion r of the S's are P's," and the words "probably about" as indicating the modality with which this conclusion is drawn and held for true. It would be equally true to consider the "probably about" as forming part of the contents of the conclusion; only from that point of view the inference ceases to be probable, and becomes rigidly necessary, and its apagogical inversion is also a necessary inference presenting no particular interest.

gism must be a good probable deduction, we may say that the syllogism of which the induction or hypothesis is the apagogical modification (in the traditional language of logic, the *reduction*) must be valid.

Probable inferences, though valid, may still differ in their strength. A probable deduction has a greater or less probable error in the concluded ratio. When r is a definite number the probable error is also definite; but as a general rule we can only assign maximum and minimum values of the probable error. The probable error is, in fact,—

$$0.477\sqrt{\frac{2r(1-r)}{n}}$$

where n is the number of independent instances. The same formula gives the probable error of an induction or hypothesis; only that in these cases, r being wholly indeterminate, the minimum value is *zero*, and the maximum is obtained by putting $r = \frac{1}{2}$.

VII.

Although the rule given above really contains all the conditions to which Inductions and Hypotheses need to conform, yet inasmuch as there are many delicate questions in regard to the application of it, and particularly since it is of that nature that a violation of it, if not too gross, may not absolutely destroy the virtue of the reasoning, a somewhat detailed study of its requirements in regard to each of the premises of the argument is still needed.

The first premise of a scientific inference is that certain things (in the case of induction) or certain characters (in the case of hypothesis) constitute a fairly chosen *sample* of the class of things or the run of characters from which they have been drawn.

The rule requires that the sample should be drawn at random and independently from the whole lot sampled. That is to say, the sample must be taken according to a precept or method which, being applied over and over again indefinitely, would in the long run result in the drawing of any one set of instances as often as any other set of the same number.

The needfulness of this rule is obvious; the difficulty is to know how we are to carry it out. The usual method is mentally to run over the lot of objects or characters to be sampled, abstracting our attention from their peculiarities and arresting ourselves at this one or that one from motives wholly unconnected with those peculiarities. But this abstention from a further determination of our choice often demands an effort of the will that is beyond our strength; and in that case a mechanical contrivance may be called to our aid. We may, for example, number all the objects of the lot, and then draw numbers by means of a roulette or other such instrument. We may even go so far as to say that this method is the type of all random drawing; for when we abstract our attention from the peculiarities of objects, the psychologists tell us that what we do is to substitute for the images of sense certain mental signs, and when we proceed to a random and arbitrary choice among these abstract objects we are governed by fortuitous determinations of the nervous system, which in this case serves the purpose of a roulette.

The drawing of objects at random is an act in which honesty is called for; and it is often hard enough to be sure that we have dealt honestly with ourselves in the matter, and still more hard to be satisfied of the honesty of another. Accordingly, one method of sampling has come to be preferred in argumentation; namely, to take of the class to be sampled all the objects of which we have a sufficient knowledge. Sampling is, however, a real art, well deserving an extended study by itself: to enlarge upon it here would lead us aside from our main purpose.

Let us rather ask what will be the effect upon inductive inference of an imperfection in the strictly random character of the sampling. Suppose that, instead of using such a precept of selection that any one M would in the long run be chosen as often as any other, we used a precept which would give a preference to a certain half of the M's, so that they would be drawn twice as often as the rest. If we were to draw a numerous sample by such a precept, and if we were to find that the proportion ρ of the sample consisted of P's, the inference that we should be regularly entitled to make would be, that among all the M's, counting the preferred half for two each, the proportion ρ would be P's. But this regular inductive inference being granted, from it we could deduce by arithmetic the further conclusion that, counting the M's for one each, the proportion of P's among them must (ρ being over $\frac{2}{3}$) lie between $\frac{3}{4}\rho + \frac{1}{4}$ and $\frac{3}{2}\rho - \frac{1}{2}$. Hence, if more than two-thirds of the instances drawn by the use of the false precept were found to be P's, we should be entitled to conclude that more than half of all the M's were P's. Thus, without allowing ourselves to be led away into a mathematical discussion, we can easily see that, in general, an imperfection of that kind in the random character of the sampling will only weaken the inductive conclusion, and render the concluded ratio less determinate, but will not necessarily destroy the force of the argument completely. In particular, when ρ approximates towards 1 or 0, the effect of the imperfect sampling will be but slight.

Nor must we lose sight of the constant tendency of the inductive process to correct itself. This is of its essence. This is the marvel of it. The probability of its conclusion only consists in the fact that if the true value of the ratio sought has not been reached, an extension of the inductive process will lead to a closer approximation. Thus, even though doubts may be entertained whether one selection of instances is a random one, yet a different selection, made by a different method, will be likely to vary from the normal in a different way, and if the ratios derived from such different selections are nearly equal, they may be presumed to be near the truth. This consideration makes it extremely advantageous in all ampliative reasoning to fortify one method of investigation by another.[7] Still we must not allow ourselves to trust so much to this virtue of induction as to relax our efforts towards making our drawings of instances as random and independent as we can. For if we infer a ratio from a number of different inductions, the magnitude of its probable error will depend very much more on the worst than on the best inductions used.

We have, thus far, supposed that although the selection of instances is not exactly regular, yet the precept followed is such that every unit of the lot would eventually get drawn. But very often it is impracticable so to draw our instances, for the reason that a part of the lot to be sampled is absolutely inaccessible to our powers of observation. If we want to know whether it will be profitable to open a mine, we sample the ore; but in advance of our mining operations, we can obtain only what ore lies near the surface. Then, simple

7. This I conceive to be all the truth there is in the doctrine of Bacon and Mill regarding different Methods of Experimental Inquiry. The main proposition of Bacon and Mill's doctrine is, that in order to prove that all *M*'s are *P*'s, we should not only take random instances of the *M*'s and examine them to see that they are *P*'s, but we should also take instances of not-*P*'s and examine them to see that they are not-*M*'s. This is an excellent way of fortifying one induction by another, when it is applicable; but it is entirely inapplicable when r has any other value than 1 or 0. For, in general, there is no connection between the proportion of *M*'s that are *P*'s and the proportion of non-*P*'s that are non-*M*'s. A very small proportion of calves may be monstrosities, and yet a very large proportion of monstrosities may be calves.

induction becomes worthless, and another method must be resorted to. Suppose we wish to make an induction regarding a series of events extending from the distant past to the distant future; only those events of the series which occur within the period of time over which available history extends can be taken as instances. Within this period we may find that the events of the class in question present some uniform character; yet how do we know but this uniformity was suddenly established a little while before the history commenced, or will suddenly break up a little while after it terminates? Now, whether the uniformity observed consists (1) in a mere resemblance between all the phenomena, or (2) in their consisting of a disorderly mixture of two kinds in a certain constant proportion, or (3) in the character of the events being a mathematical function of the time of occurrence,—in any of these cases we can make use of an apagoge from the following probable deduction:—

Within the period of time *M*, a certain event *P* occurs;
S is a period of time taken at random from *M*, and more than half as long:
Hence, probably the event *P* will occur within the time *S*.

Inverting this deduction, we have the following ampliative inference:—

S is a period of time taken at random from *M*, and more than half as long;
The event *P* does not happen in the time *S*:
Hence, probably the event *P* does not happen in the period *M*.

The probability of the conclusion consists in this, that we here follow a precept of inference, which, if it is very often applied, will more than half the time lead us right. Analogous reasoning would obviously apply to any portion of an unidimensional continuum, which might be similar to periods of time. This is a sort of logic which is often applied by physicists in what is called *extrapolation* of an empirical law. As compared with a typical induction, it is obviously an excessively weak kind of inference. Although indispensible in almost every branch of science, it can lead to no solid conclusions in regard to what is remote from the field of direct perception, unless it be bolstered up in certain ways to which we shall have occasion to refer further on.

Let us now consider another class of difficulties in regard to the rule that the samples must be drawn at random and independently. In the first place, what if the lot to be sampled be infinite in number? In what sense could a random sample be taken from a lot like that?

A random sample is one taken according to a method that would, in the long run, draw any one object as often as any other. In what sense can such drawing be made from an infinite class? The answer is not far to seek. Conceive a cardboard disk revolving in its own plane about its centre and pretty accurately balanced, so that when put into rotation it shall be about[8] as likely to come to rest in any one position as in any other; and let a fixed pointer indicate a position on the disk: the number of points on the circumference is infinite, and on rotating the disk repeatedly the pointer enables us to make a selection from this infinite number. This means merely that although the points are innumerable, yet there is a certain order among them that enables us to run them through and pick from them as from a very numerous collection. In such a case, and in no other, can an infinite lot be sampled. But it would be equally true to say that a finite lot can be sampled only on condition that it can be regarded as equivalent to an infinite lot. For the random sampling of a finite class supposes the possibility of drawing out an object, throwing it back, and continuing this process indefinitely; so that what is really sampled is not the finite collection of things, but the unlimited number of possible drawings.

But though there is thus no insuperable difficulty in sampling an infinite lot, yet it must be remembered that the conclusion of inductive reasoning only consists in the approximate evaluation of a *ratio,* so that it never can authorize us to conclude that in an infinite lot sampled there exists no single exception to a rule. Although all the planets are found to gravitate toward one another, this affords not the slightest direct reason for denying that among the innumerable orbs of heaven there may be some which exert no such force. Although at no point of space where we have yet been have we found any possibility of motion in a fourth dimension, yet this does not tend to show (by simple induction, at least) that space has absolutely but three dimensions. Although all the bodies we have had the opportunity of examining appear to obey the law of inertia, this does not prove that atoms and atomicules are subject to the same law. Such conclusions must be reached, if at all, in some other way than by simple induction. This latter may show that it is unlikely that, in my lifetime or yours, things so extraordinary should be found, but do not

8. I say *about,* because the doctrine of probability only deals with approximate evaluations.

warrant extending the prediction into the indefinite future. And experience shows it is not safe to predict that such and such a fact will *never* be met with.

If the different instances of the lot sampled are to be drawn independently, as the rule requires, then the fact that an instance has been drawn once must not prevent its being drawn again. It is true that if the objects remaining unchosen are very much more numerous than those selected, it makes practically no difference whether they have a chance of being drawn again or not, since that chance is in any case very small. Probability is wholly an affair of approximate, not at all of exact, measurement; so that when the class sampled is very large, there is no need of considering whether objects can be drawn more than once or not. But in what is known as "reasoning from analogy," the class sampled is small, and no instance is taken twice. For example: we know that of the major planets the Earth, Mars, Jupiter, and Saturn revolve on their axes, and we conclude that the remaining four, Mercury, Venus, Uranus, and Neptune, probably do the like. This is essentially different from an inference from what has been found in drawings made hitherto, to what will be found in indefinitely numerous drawings to be made hereafter. Our premises here are that the Earth, Mars, Jupiter, and Saturn are a random sample of a natural class of major planets,—a class which, though (so far as we know) it is very small, yet *may* be very extensive, comprizing whatever there may be that revolves in a circular orbit around a great sun, is nearly spherical, shines with reflected light, is very large, etc. Now the examples of major planets that we can examine all rotate on their axes; whence we suppose that Mercury, Venus, Uranus, and Neptune, since they possess, so far as we know, all the properties common to the natural class to which the Earth, Mars, Jupiter, and Saturn belong, possess this property likewise. The points to be observed are, first, that any small class of things may be regarded as a mere sample of an actual or possible large class having the same properties and subject to the same conditions; second, that while we do not know what all these properties and conditions are, we do know some of them, which some may be considered as a random sample of all; third, that a random selection without replacement from a small class may be regarded as a true random selection from that infinite class of which the finite class is a random selection. The formula of the analogical inference presents, therefore, three premises, thus:—

S', S'', S''' are a random sample of some undefined class X, of whose characters P', P'', P''' are samples.
Q is P', P'', P'''.
S', S'', S''', are R's.
Hence, Q is an R.

We have evidently here an induction and an hypothesis followed by a deduction; thus,—

Every X is, for example, P', P'', P''', etc.	S', S'', S''', etc., are samples of the X's.
Q is found to be P', P'', P''', etc.	S', S'', S''', etc., are found to be R's.
Hence, hypothetically, Q is an X.	Hence, inductively, every X is an R.

Hence, deductively, Q is an R.[9]

An argument from analogy may be strengthened by the addition of instance after instance to the premises, until it loses its ampliative character by the exhaustion of the class and becomes a mere deduction of that kind called *complete induction,* in which, however, some shadow of the inductive character remains, as this name implies.

VIII.

Take any human being, at random,—say Queen Elizabeth. Now a little more than half of all the human beings who have ever existed have been males; but it does not follow that it is a little more likely than not that Queen Elizabeth was a male, since we know she was a woman. Nor, if we had selected Julius Cæsar, would it be only a little more likely than not that he was a male. It is true that if we were to

9. That this is really a correct analysis of the reasoning can be shown by the theory of probabilities. For the expression

$$\frac{(p+q)!}{p!q!} \cdot \frac{(\pi+\rho)!}{\pi!\rho!} \cdot \frac{(p+\pi)!(q+\rho)!}{(p+\pi+q+\rho)!}$$

expresses at once the probability of two events; namely, it expresses first the probability that of $p+q$ objects drawn without replacement from a lot consisting of $p+\pi$ objects having the character R together with $q+\rho$ not having this character, the number of those drawn having this character will be p; and second, the same expression denotes the probability that if among $p+\pi+q+\rho$ objects drawn at random from an infinite class (containing no matter what proportion of R's to non-R's), it happens that $p+\pi$ have the character R, then among any $p+q$ of them, designated at random, p will have the same character. Thus we see that the chances in reference to drawing without replacement from a finite class are precisely the same as those in reference to a class which has been drawn at random from an infinite class.

go on drawing at random an indefinite number of instances of human beings, a slight excess over one-half would be males. But that which constitutes the probability of an inference is the proportion of true conclusions among all those which could be derived *from the same precept.* Now a precept of inference, being a rule which the mind is to follow, changes its character and becomes different when the case presented to the mind is essentially different. When, knowing that the proportion r of all M's are P's, I draw an instance, S, of an M, without any other knowledge of whether it is a P or not, and infer with probability, r, that it is P, the case presented to my mind is very different from what it is if I have such other knowledge. In short, I cannot make a valid probable inference without taking into account whatever knowledge I have (or, at least, whatever occurs to my mind) that bears upon the question.

The same principle may be applied to the statistical deduction of Form IV. If the major premise, that the proportion r of the M's are P's, be laid down first, before the instances of M's are drawn, we really draw our inference concerning those instances (that the proportion r of them will be P's) in advance of the drawing, and therefore before we know whether they are P's or not. But if we draw the instances of the M's first, and after the examination of them decide what we will select for the predicate of our major premise, the inference will generally be completely fallacious. In short, we have the rule that the major term P must be decided upon in advance of the examination of the sample; and in like manner in Form IV (*bis*) the minor term S must be decided upon in advance of the drawing.

The same rule follows us into the logic of induction and hypothesis. If in sampling any class, say the M's, we first decide what the character P is for which we propose to sample that class, and also how many instances we propose to draw, our inference is really made before these latter are drawn, that the proportion of P's in the whole class is probably about the same as among the instances that are to be drawn, and the only thing we have to do is to draw them and observe the ratio. But suppose we were to draw our inferences without the predesignation of the character P; then we might in every case find some recondite character in which those instances would all agree. That, by the exercise of sufficient ingenuity, we should be sure to be able to do this, even if not a single other object of the class M possessed that character, is a matter of demonstration. For in geometry a curve may be drawn through any given series of points, without passing through any one of another given series of points, and this irrespective of the number of dimensions. Now, all the qualities of objects may be conceived to result from variations of a number of continuous variables; hence any lot of objects possesses some character in common, not possessed by any other. It is true that if the universe of quality is limited, this is not altogether true; but it remains true that unless we have some special premise from which to infer the contrary, it always *may* be possible to assign some common character of the instances S', S'', S''', etc., drawn at random from among the M's, which does not belong to the M's generally. So that if the character P were not predesignate, the deduction of which our induction is the apagogical inversion would not be valid; that is to say, we could not reason that if the M's did not generally possess the character P, it would not be likely that the S's should all possess this character.

I take from a biographical dictionary the first five names of poets, with their ages at death. They are,

Aagard,	died	at	48.
Abeille,	"	"	70.
Abulola,	"	"	84.
Abunowas,	"	"	48.
Accords,	"	"	45.

These five ages have the following characters in common:—

1. The difference of the two digits composing the number, divided by three, leaves a remainder of *one.*

2. The first digit raised to the power indicated by the second, and then divided by three, leaves a remainder of *one.*

3. The sum of the prime factors of each age, including *one* as a prime factor, is divisible by *three.*

Yet there is not the smallest reason to believe that the next poet's age would possess these characters.

Here we have a *conditio sine quâ non* of valid induction which has been singularly overlooked by those who have treated of the logic of the subject, and is very frequently violated by those who draw inductions. So accomplished a reasoner as Dr. Lyon Playfair, for instance, has written a paper of which the following is an abstract. He first takes the specific gravities of the three allotropic forms of carbon, as follows:—

Diamond,	3.48
Graphite,	2.29
Charcoal,	1.88

He now seeks to find a uniformity connecting these three instances; and he discovers that the atomic weight of carbon, being 12,

$$
\begin{array}{llll}
\text{Sp. gr.} & \text{diamond} & \text{nearly} & = 3.46 = \sqrt[2]{12} \\
\text{"}\quad\text{"} & \text{graphite} & \text{"} & = 2.29 = \sqrt[3]{12} \\
\text{"}\quad\text{"} & \text{charcoal} & \text{"} & = 1.86 = \sqrt[4]{12}
\end{array}
$$

This, he thinks, renders it probable that the specific gravities of the allotropic forms of other elements would, if we knew them, be found to equal the different roots of their atomic weight. But so far, the character in which the instances agree not having been predesignated, the induction can serve only to suggest a question, and ought not to create any belief. To test the proposed law, he selects the instance of silicon, which like carbon exists in a diamond and in a graphitoidal condition. He finds for the specific gravities—

Diamond silicon,	2.47
Graphite silicon,	2.33.[10]

Now, the atomic weight of silicon, that of carbon being 12, can only be taken as 28. But 2.47 does not approximate to any root of 28. It is, however, nearly the cube root of 14, ($\sqrt[3]{\frac{1}{2} \times 28} = 2.41$), while 2.33 is nearly the fourth root of 28 ($\sqrt[4]{28} = 2.30$). Dr. Playfair claims that silicon is an instance satisfying his formula. But in fact this instance requires the formula to be modified; and the modification not being predesignate, the instance cannot count. Boron also exists in a diamond and a graphitoidal form; and accordingly Dr. Playfair takes this as his next example. Its atomic weight is 10.9, and its specific gravity is 2.68; which is the square root of $\frac{2}{3} \times 10.9$. There seems to be here a further modification of the formula not predesignated, and therefore this instance can hardly be reckoned as confirmatory. The next instances which would occur to the mind of any chemist would be phosphorus and sulphur, which exist in familiarly known allotropic forms. Dr. Playfair admits that the specific gravities of phosphorus have no relations to its atomic weight at all analogous to those of carbon. The different forms of sulphur have nearly the

10. The author ought to have noted that this number is open to some doubt, since the specific gravity of this form of silicon appears to vary largely. If a different value had suited the theory better, he might have been able to find reasons for preferring that other value. But I do not mean to imply that Dr. Playfair has not dealt with perfect fairness with his facts, except as to the fallacy which I point out.

same specific gravity, being approximately the fifth root of the atomic weight 32. Selenium also has two allotropic forms, whose specific gravities are 4.8 and 4.3; one of these follows the law, while the other does not. For tellurium the law fails altogether; but for bromine and iodine it holds. Thus the number of specific gravities for which the law was predesignate are 8; namely, 2 for phosphorus, 1 for sulphur, 2 for selenium, 1 for tellurium, 1 for bromine, and 1 for iodine. The law holds for 4 of these, and the proper inference is that about half the specific gravities of metalloids are roots of some simple ratio of their atomic weights.

Having thus determined this ratio, we proceed to inquire whether an agreement half the time with the formula constitutes any special connection between the specific gravity and the atomic weight of a metalloid. As a test of this, let us arrange the elements in the order of their atomic weights, and compare the specific gravity of the first with the atomic weight of the last, that of the second with the atomic weight of the last but one, and so on. The atomic weights are—

Boron,	10.9	Tellurium,	128.1
Carbon,	12.0	Iodine,	126.9
Silicon,	28.0	Bromine,	80.0
Phosphorus,	31.0	Selenium,	79.1

Sulphur, 32.

There are three specific gravities given for carbon, and two each for silicon, phosphorus, and selenium. The question, therefore, is, whether of the fourteen specific gravities as many as seven are in Playfair's relation with the atomic weights, not of the same element, but of the one paired with it. Now, taking the original formula of Playfair we find

Sp. gr. boron	= 2.68	$\sqrt[5]{\mathrm{Te}} = 2.64$
3rd Sp. gr. carbon	= 1.88	$\sqrt[5]{\mathrm{I}} = 1.84$
2nd Sp. gr. carbon	= 2.29	$\sqrt[6]{\mathrm{I}} = 2.24$
1st Sp. gr. phosphorus	= 1.83	$\sqrt[7]{\mathrm{Se}} = 1.87$
2nd Sp. gr. phosphorus	= 2.10	$\sqrt[6]{\mathrm{Se}} = 2.07$

or five such relations without counting that of sulphur to itself. Next, with the modification introduced by Playfair, we have

1st Sp. gr. silicon	= 2.47	$\sqrt[4]{\frac{1}{2} \times \mathrm{Br}} = 2.51$
2nd Sp. gr. silicon	= 2.33	$\sqrt[6]{2 \times \mathrm{Br}} = 2.33$
Sp. gr. iodine	= 4.95	$\sqrt[2]{2 \times \mathrm{C}} = 4.90$
1st Sp. gr. carbon	= 3.48	$\sqrt[3]{\frac{1}{3} \times \mathrm{I}} = 3.48$

It thus appears that there is no more frequent agreement with Playfair's proposed law than what is due to chance.[11]

Another example of this fallacy was "Bode's law" of the relative distances of the planets, which was shattered by the first discovery of a true planet after its enunciation. In fact, this false kind of induction is extremely common in science and in medicine.[12] In the case of hypothesis, the correct rule has often been laid down; namely, that a hypothesis can only be received upon the ground of its having been *verified* by successful *prediction.* The term *predesignation* used in this paper appears to be more exact, inasmuch as it is not at all requisite that the ratio ρ should be given in advance of the examination of the samples. Still, since ρ is equal to 1 in all ordinary hypotheses, there can be no doubt that the rule of prediction, so far as it goes, coincides with that here laid down.

We have now to consider an important modification of the rule. Suppose that, before sampling a class of objects, we have predesignated not a single character but n characters, for which we propose to examine the samples. This is equivalent to making n different inductions from the same instances. The probable error in this case is that error whose probability for a simple induction is only $(\frac{1}{2})^n$, and the theory of probabilities shows that it increases but slowly with n; in fact, for $n = 1000$ it is only about five times as great as for $n = 1$, so that with only 25 times as many instances the inference would be as secure for the former value of n as with the latter; with 100 times as many instances an induction in which $n = 10{,}000{,}000{,}000$ would be equally secure. Now the whole universe of characters will never contain such a number as the last; and the same may be said of the universe of objects in the case of hypothesis. So that, without any voluntary predesignation, the limitation of our imagination and experience amounts to a predesignation far within those limits; and we thus see that if the number of instances be very great indeed, the failure to predesignate is not an important fault. Of characters at all striking, or of objects at all familiar, the number will seldom reach 1,000; and of very striking characters or very familiar objects the

11. As the relations of the different powers of the specific gravity would be entirely different if any other substance than water were assumed as the standard, the law is antecedently in the highest degree improbable. This makes it likely that some fallacy was committed, but does not show what it was.

12. The physicians seem to use the maxim that you cannot reason from *post hoc* to *propter hoc* to mean (rather obscurely) that cases must not be used to prove a proposition that has only been suggested by these cases themselves.

number is still less. So that if a large number of samples of a class are found to have some very striking character in common, or if a large number of characters of one object are found to be possessed by a very familiar object, we need not hesitate to infer, in the first case, that the same characters belong to the whole class, or, in the second case, that the two objects are practically identical; remembering only that the inference is less to be relied upon than it would be had a deliberate predesignation been made. This is no doubt the precise significance of the rule sometimes laid down, that a hypothesis ought to be *simple,*—simple here being taken in the sense of familiar.

This modification of the rule shows that, even in the absence of voluntary predesignation, *some* slight weight is to be attached to an induction or hypothesis. And perhaps when the number of instances is not very small, it is enough to make it worth while to subject the inference to a regular test. But our natural tendency will be to attach too much importance to such suggestions, and we shall avoid waste of time in passing them by without notice until some stronger plausibility presents itself.

IX.

In almost every case in which we make an induction or a hypothesis, we have some knowledge which renders our conclusion antecedently likely or unlikely. The effect of such knowledge is very obvious, and needs no remark. But what also very often happens is that we have some knowledge, which, though not of itself bearing upon the conclusion of the scientific argument, yet serves to render our inference more or less probable, or even to alter the terms of it. Suppose, for example, that we antecedently know that all the M's strongly resemble one another in regard to characters of a certain order. Then, if we find that a moderate number of M's taken at random have a certain character, P, of that order, we shall attach a greater weight to the induction than we should do if we had not that antecedent knowledge. Thus, if we find that a certain sample of gold has a certain chemical character,—since we have very strong reason for thinking that all gold is alike in its chemical characters,—we shall have no hesitation in extending the proposition from the one sample to gold in general. Or if we know that among a certain people,—say the Icelanders,—an extreme uniformity prevails in regard to all their ideas, then, if we find that two or three individuals taken at random from among them have all any particular superstition, we shall be the more ready to infer that it belongs to the whole people from what we know of their uniformity. The influence of this sort of uniformity upon inductive conclusions was strongly insisted upon by Philodemus, and some very exact conceptions in regard to it may be gathered from the writings of Mr. Galton. Again, suppose we know of a certain character, P, that in whatever classes of a certain description it is found at all, to those it usually belongs as a universal character; then any induction which goes toward showing that all the M's are P will be greatly strengthened. Thus it is enough to find that two or three individuals taken at random from a genus of animals have three toes on each foot, to prove that the same is true of the whole genus; for we know that this is a *generic* character. On the other hand, we shall be slow to infer that all the animals of a genus have the same color, because color varies in almost every genus. This kind of uniformity seemed to J. S. Mill to have so controlling an influence upon inductions, that he has taken it as the centre of his whole theory of the subject.

Analogous considerations modify our hypothetic inferences. The sight of two or three words will be sufficient to convince me that a certain manuscript was written by myself, because I know a certain look is peculiar to it. So an analytical chemist, who wishes to know whether a solution contains gold, will be completely satisfied if it gives a precipitate of the purple of cassius with chloride of tin; because this proves that either gold or some hitherto unknown substance is present. These are examples of characteristic tests. Again, we may know of a certain person, that whatever opinions he holds he carries out with uncompromising rigor to their utmost logical consequences; then, if we find his views bear some of the marks of any ultra school of thought, we shall readily conclude that he fully adheres to that school.

There are thus four different kinds of uniformity and non-uniformity which may influence our ampliative inferences:—

1. The members of a class may present a greater or less general resemblance as regards a certain line of characters.

2. A character may have a greater or less tendency to be present or absent throughout the whole of whatever classes of certain kinds.

3. A certain set of characters may be more or less intimately connected, so as to be probably either present or absent together in certain kinds of objects.

4. An object may have more or less tendency to possess the whole of certain sets of characters when it possesses any of them.

A consideration of this sort may be so strong as to amount to demonstration of the conclusion. In this case, the inference is mere deduction,—that is, the application of a general rule already established. In other cases, the consideration of uniformities will not wholly destroy the inductive or hypothetic character of the inference, but will only strengthen or weaken it by the addition of a new argument of a deductive kind.

X.

We have thus seen how, in a general way, the processes of inductive and hypothetic inference are able to afford answers to our questions, though these may relate to matters beyond our immediate ken. In short, a theory of the logic of verification has been sketched out. This theory will have to meet the objections of two opposing schools of logic.

The first of these explains induction by what is called the doctrine of Inverse Probabilities, of which the following is an example: Suppose an ancient denizen of the Mediterranean coast, who had never heard of the tides, had wandered to the shore of the Atlantic Ocean, and there, on a certain number m of successive days had witnessed the rise of the sea. Then, says Quételet, he would have been entitled to conclude that there was a probability equal to $\frac{m+1}{m+2}$ that the sea would rise on the next following day.[13] Putting $m = 0$, it is seen that this view assumes that the probability of a totally unknown event is $\frac{1}{2}$; or that of all theories proposed for examination one-half are true. In point of fact, we know that although theories are not proposed unless they present some decided plausibility, nothing like one-half turn out to be true. But to apply correctly the doctrine of inverse probabilities, it is necessary to know the antecedent probability of the event whose probability is in question. Now, in pure hypothesis or induction, we know nothing of the conclusion antecedently to the inference in hand. Mere ignorance, however, cannot advance us toward any knowledge; therefore it is impossible that the theory of inverse probabilities should rightly give a value for the probability of a pure inductive or hypothetic conclusion. For it cannot do this

13. See Laplace, *Théorie Analytique des Probabilités*, livre ii. chap. vi.

without assigning an antecedent probability to this conclusion; so that if this antecedent probability represents mere ignorance (which never aids us), it cannot do it at all.

The principle which is usually assumed by those who seek to reduce inductive reasoning to a problem in inverse probabilities is, that if nothing whatever is known about the frequency of occurrence of an event, then any one frequency is as probable as any other. But Boole has shown that there is no reason whatever to prefer this assumption, to saying that any one "constitution of the universe" is as probable as any other. Suppose, for instance, there were four possible occasions upon which an event might occur. Then there would be 16 "constitutions of the universe," or possible distributions of occurrences and non-occurrences. They are shown in the following table, where *Y* stands for an occurrence and *N* for a non-occurrence.

4 *occurrences.*	3 *occurrences.*	2 *occurrences.*	1 *occurrence.*	0 *occurrence.*
YYYY	*YYYN*	*YYNN*	*YNNN*	*NNNN*
	YYNY	*YNYN*	*NYNN*	
	YNYY	*YNNY*	*NNYN*	
	NYYY	*NYYN*	*NNNY*	
		NYNY		
		NNYY		

It will be seen that different frequencies result some from more and some from fewer different "constitutions of the universe," so that it is a very different thing to assume that all frequencies are equally probable from what it is to assume that all constitutions of the universe are equally probable.

Boole says that one assumption is as good as the other. But I will go further, and say that the assumption that all constitutions of the universe are equally probable is far better than the assumption that all frequencies are equally probable. For the latter proposition, though it may be applied to any one unknown event, cannot be applied to all unknown events without inconsistency. Thus, suppose all frequencies of the event whose occurrence is represented by *Y* in the above table are equally probable. Then consider the event which consists in a *Y* following a *Y* or an *N* following an *N*. The possible ways in which *this* event may occur or not are shown in the following table:—

3 occurrences.	*2 occurrences.*	*1 occurrence.*	*0 occurrence.*
YYYY	*YYYN*	*YYNY*	*YNYN*
NNNN	*NNNY*	*NNYN*	*NYNY*
	YYNN	*YNNY*	
	NNYY	*NYYN*	
	NYYY	*YNYY*	
	YNNN	*NYNN*	

It will be found that assuming the different frequencies of the first event to be equally probable, those of this new event are not so,—the probability of three occurrences being half as large again as that of two, or one. On the other hand, if all constitutions of the universe are equally probable in the one case, they are so in the other; and this latter assumption, in regard to perfectly unknown events, never gives rise to any inconsistency.

Suppose, then, that we adopt the assumption that any one constitution of the universe is as probable as any other; how will the inductive inference then appear, considered as a problem in probabilities? The answer is extremely easy;[14] namely, the occurrences or non-occurrences of an event in the past in no way affect the probability of its occurrence in the future.

Boole frequently finds a problem in probabilities to be indeterminate. There are those to whom the idea of an unknown probability seems an absurdity. Probability, they say, measures the state of our knowledge, and ignorance is denoted by the probability $\frac{1}{2}$. But I apprehend that the expression "the probability of an event" is an incomplete one. A probability is a fraction whose numerator is the frequency of a specific kind of event, while its denominator is the frequency of a genus embracing that species. Now the expression in question names the numerator of the fraction, but omits to name the denominator. There is a sense in which it is true that the probability of a perfectly unknown event is one-half; namely, the assertion of its occurrence is the answer to a possible question answerable by "yes" or "no," and of all such questions just half the possible answers are true. But if attention be paid to the denominators of the fractions, it will be found that this value of $\frac{1}{2}$ is one of which no possible use can be made in the calculation of probabilities.

The theory here proposed does not assign any probability to the

14. See Boole, *Laws of Thought,* p. 370.

inductive or hypothetic conclusion, in the sense of undertaking to say how frequently *that conclusion* would be found true. It does not propose to look through all the possible universes, and say in what proportion of them a certain uniformity occurs; such a proceeding, were it possible, would be quite idle. The theory here presented only says how frequently, in this universe, the special form of induction or hypothesis would lead us right. The probability given by this theory is in every way different—in meaning, numerical value, and form—from that of those who would apply to ampliative inference the doctrine of inverse chances.

Other logicians hold that if inductive and hypothetic premises lead to true oftener than to false conclusions, it is only because the universe happens to have a certain constitution. Mill and his followers maintain that there is a general tendency toward uniformity in the universe, as well as special uniformities such as those which we have considered. The Abbé Gratry believes that the tendency toward the truth in induction is due to a miraculous intervention of Almighty God, whereby we are led to make such inductions as happen to be true, and are prevented from making those which are false. Others have supposed that there is a special adaptation of the mind to the universe, so that we are more apt to make true theories than we otherwise should be. Now, to say that a theory such as these is *necessary* to explaining the validity of induction and hypothesis is to say that these modes of inference are not in themselves valid, but that their conclusions are rendered probable by being probable deductive inferences from a suppressed (and originally unknown) premise. But I maintain that it has been shown that the modes of inference in question are necessarily valid, whatever the constitution of the universe, so long as it admits of the premises being true. Yet I am willing to concede, in order to concede as much as possible, that when a man draws instances at random, all that he knows is that he *tries* to follow a certain precept; so that the sampling process might be rendered generally fallacious by the existence of a mysterious and malign connection between the mind and the universe, such that the possession by an object of an *unperceived* character might influence the will toward choosing it or rejecting it. Such a circumstance would, however, be as fatal to deductive as to ampliative inference. Suppose, for example, that I were to enter a great hall where people were playing *rouge et noir* at many tables; and suppose that I knew that the red and black were turned up with equal frequency. Then, if I were to make a large number of mental bets with myself, at this table and at that, I might, by statistical deduction, expect to win about half of them,—precisely as I might expect, from the results of these samples, to infer by induction the probable ratio of frequency of the turnings of red and black in the long run, if I did not know it. But could some devil look at each card before it was turned, and then influence me mentally to bet upon it or to refrain therefrom, the observed ratio in the cases upon which I had bet might be quite different from the observed ratio in those cases upon which I had not bet. I grant, then, that even upon my theory some fact has to be supposed to make induction and hypothesis valid processes; namely, it is supposed that the supernal powers withhold their hands and let me alone, and that no mysterious uniformity or adaptation interferes with the action of chance. But then this negative fact supposed by my theory plays a totally different part from the facts supposed to be requisite by the logicians of whom I have been speaking. So far as facts like those they suppose can have any bearing, they serve as major premises from which the fact inferred by induction or hypothesis might be deduced; while the negative fact supposed by me is merely the denial of any major premise from which the falsity of the inductive or hypothetic conclusion could in general be deduced. Nor is it necessary to deny altogether the existence of mysterious influences adverse to the validity of the inductive and hypothetic processes. So long as their influence were not too overwhelming, the wonderful self-correcting nature of the ampliative inference would enable us, even if they did exist, to detect and make allowance for them.

Although the universe need have no peculiar constitution to render ampliative inference valid, yet it is worth while to inquire whether or not it has such a constitution; for if it has, that circumstance must have its effect upon all our inferences. It cannot any longer be denied that the human intellect is peculiarly adapted to the comprehension of the laws and facts of nature, or at least of some of them; and the effect of this adaptation upon our reasoning will be briefly considered in the next section. Of any miraculous interference by the higher powers, we know absolutely nothing; and it seems in the present state of science altogether improbable. The effect of a knowledge of special uniformities upon ampliative inferences has already been touched upon. That there is a general tendency toward uniformity in nature is not merely an unfounded, it is an absolutely absurd, idea in any other sense than that man is adapted to his surroundings. For the universe of marks is only limited by the limitation of human interests and powers of observation. Except for that limitation, every lot of objects in the universe would have (as I have elsewhere shown) some character in common and peculiar to it. Consequently, there is but one possible arrangement of characters among objects as they exist, and there is no room for a greater or less degree of uniformity in nature. If nature seems highly uniform to us, it is only because our powers are adapted to our desires.

XI.

The questions discussed in this essay relate to but a small part of the Logic of Scientific Investigation. Let us just glance at a few of the others.

Suppose a being from some remote part of the universe, where the conditions of existence are inconceivably different from ours, to be presented with a United States Census Report,—which is for us a mine of valuable inductions, so vast as almost to give that epithet a new signification. He begins, perhaps, by comparing the ratio of indebtedness to deaths by consumption in counties whose names begin with the different letters of the alphabet. It is safe to say that he would find the ratio everywhere the same, and thus his inquiry would lead to nothing. For an induction is wholly unimportant unless the proportions of P's among the M's and among the non-M's differ; and a hypothetic inference is unimportant unless it be found that S has either a greater or a less proportion of the characters of M than it has of other characters. The stranger to this planet might go on for some time asking inductive questions that the Census would faithfully answer, without learning anything except that certain conditions were independent of others. At length, it might occur to him to compare the January rain-fall with the illiteracy. What he would find is given in the following table[15]:—

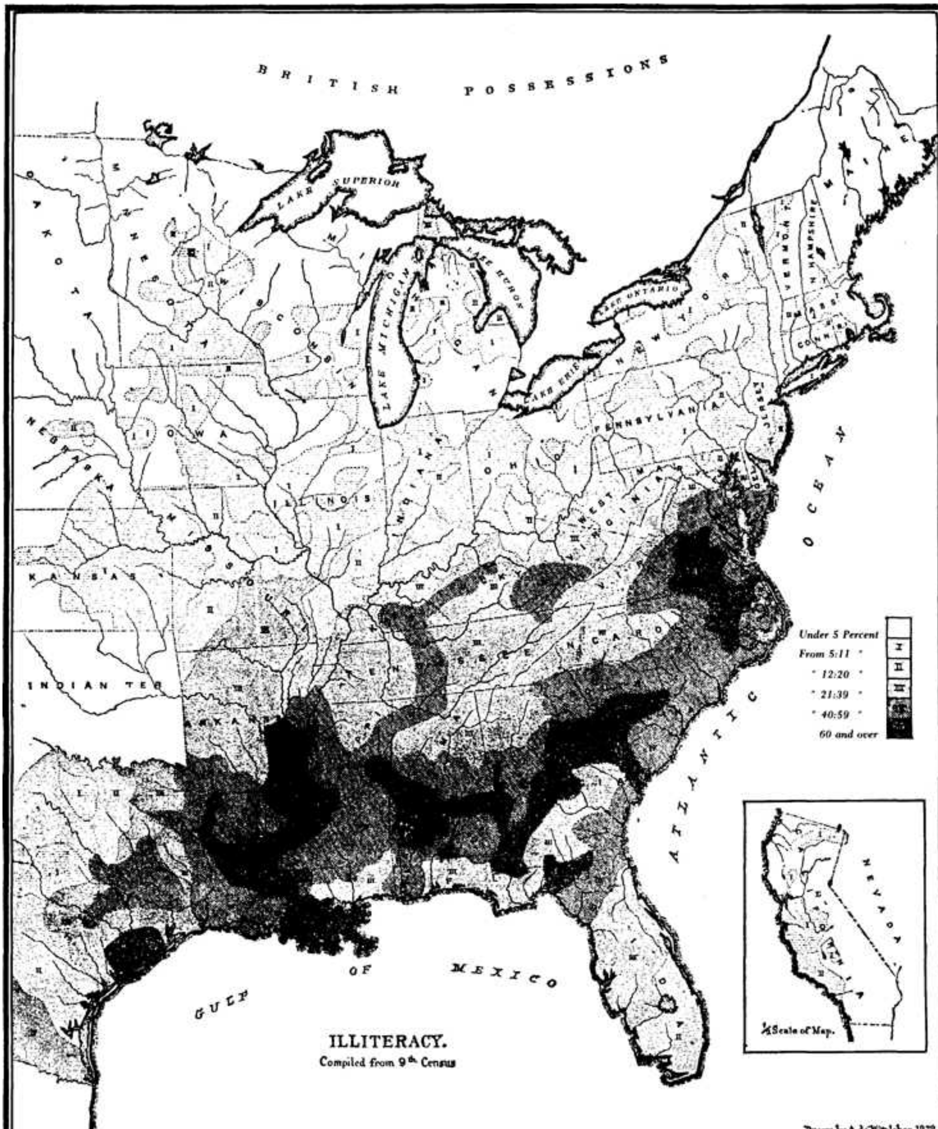
15. The different regions with the January rain-fall are taken from Mr. Schott's work. The percentage of illiteracy is roughly estimated from the numbers given in the Report of the 1870 Census.

REGION.	January Rain-fall.	Illiteracy.
	Inches.	Per cent.
Atlantic Sea-coast, Portland to Washington	0.92	11
Vermont, Northern and Western New York	0.78	7
Upper Mississippi River	0.52	3
Ohio River Valley	0.74	8
Lower Mississippi, Red River, and Kentucky	1.08	50
Mississippi Delta and Northern Gulf Coast	1.09	57
Southeastern Coast	0.68	40

He would infer that in places that are drier in January there is, not always but generally, less illiteracy than in wetter places. A detailed comparison between Mr. Schott's map of the winter rain-fall with the map of illiteracy in the general census, would confirm the result that these two conditions have a partial connection. This is a very good example of an induction in which the proportion of *P*'s among the *M*'s is different, but not very different, from the proportion among the non-*M*'s. It is unsatisfactory; it provokes further inquiry; we desire to replace the *M* by some different class, so that the two proportions may be more widely separated. Now we, knowing as much as we do of the effects of winter rain-fall upon agriculture, upon wealth, etc., and of the causes of illiteracy, should come to such an inquiry furnished with a large number of appropriate conceptions; so that we should be able to ask intelligent questions not unlikely to furnish the desired key to the problem. But the strange being we have imagined could only make his inquiries hap-hazard, and could hardly hope ever to find the induction of which he was in search.

Nature is a far vaster and less clearly arranged repertory of facts than a census report; and if men had not come to it with special aptitudes for guessing right, it may well be doubted whether in the ten or twenty thousand years that they may have existed their greatest mind would have attained the amount of knowledge which is actually possessed by the lowest idiot. But, in point of fact, not man merely, but all animals derive by inheritance (presumably by natural selection) two classes of ideas which adapt them to their environ-

WINTER RAIN FALL
C.A.Schott



ILLITERACY.
Compiled from 9th Census

ment. In the first place, they all have from birth some notions, however crude and concrete, of force, matter, space, and time; and, in the next place, they have some notion of what sort of objects their fellow-beings are, and of how they will act on given occasions. Our innate mechanical ideas were so nearly correct that they needed but slight correction. The fundamental principles of statics were made out by Archimedes. Centuries later Galileo began to understand the laws of dynamics, which in our times have been at length, perhaps, completely mastered. The other physical sciences are the results of inquiry based on guesses suggested by the ideas of mechanics. The moral sciences, so far as they can be called sciences, are equally developed out of our instinctive ideas about human nature. Man has thus far not attained to any knowledge that is not in a wide sense either mechanical or anthropological in its nature, and it may be reasonably presumed that he never will.

Side by side, then, with the well established proposition that all knowledge is based on experience, and that science is only advanced by the experimental verifications of theories, we have to place this other equally important truth, that all human knowledge, up to the highest flights of science, is but the development of our inborn animal instincts.

Note A: On A Limited Universe of Marks

P 268c: 182–86

Boole, De Morgan, and their followers, frequently speak of a "limited universe of discourse" in logic. An unlimited universe would comprise the whole realm of the logically possible. In such a universe, every universal proposition, not tautologous, is false; every particular proposition, not absurd, is true. Our discourse seldom relates to this universe: we are either thinking of the physically possible, or of the historically existent, or of the world of some romance, or of some other limited universe.

But besides its universe of objects, our discourse also refers to a universe of characters. Thus, we might naturally say that virtue and an orange have nothing in common. It is true that the English word for each is spelt with six letters, but this is not one of the marks of the universe of our discourse.

A universe of things is unlimited in which every combination of characters, short of the whole universe of characters, occurs in some object. In like manner, the universe of characters is unlimited in case every aggregate of things short of the whole universe of things possesses in common one of the characters of the universe of characters. The conception of ordinary syllogistic is so unclear that it would hardly be accurate to say that it supposes an unlimited universe of characters; but it comes nearer to that than to any other consistent view. The non-possession of any character is regarded as implying the possession of another character the negative of the first.

In our ordinary discourse, on the other hand, not only are both universes limited, but, further than that, we have nothing to do with individual objects nor simple marks; so that we have simply the two distinct universes of things and marks related to one another, in general, in a perfectly indeterminate manner. The consequence is, that a proposition concerning the relations of two groups of marks is not necessarily equivalent to any proposition concerning classes of things; so that the distinction between propositions in extension and propositions in comprehension is a real one, separating two kinds of facts, whereas in the view of ordinary syllogistic the distinction only relates to two modes of considering any fact. To say that every object of the class S is included among the class of P's, of course must imply that every common character of the P's is a common character of the S's. But the converse implication is by no means necessary, except with an unlimited universe of marks. The reasonings in depth of which I have spoken, suppose, of course, the absence of any general regularity about the relations of marks and things.

I may mention here another respect in which this view differs from that of ordinary logic, although it is a point which has, so far as I am aware, no bearing upon the theory of probable inference. It is that under this view there are propositions of which the subject is a class of things, while the predicate is a group of marks. Of such propositions there are twelve species, distinct from one another in the sense that any fact capable of being expressed by a proposition of one of these species cannot be expressed by any proposition of another species. The following are examples of six of the twelve species:—

1. Every object of the class S possesses every character of the group π.
2. Some object of the class S possesses all characters of the group π.
3. Every character of the group π is possessed by some object of the class S.
4. Some character of the group π is possessed by all the objects of the class S.
5. Every object of the class S possesses some character of the group π.
6. Some object of the class S possesses some character of the group π.

The remaining six species of propositions are like the above, except that they speak of objects *wanting* characters instead of *possessing* characters.

But the varieties of proposition do not end here; for we may have, for example, such a form as this: "Some object of the class S possesses every character not wanting to any object of the class P." In short, the relative term "possessing as a character," or its negative, may enter into the proposition any number of times. We may term this number the *order* of the proposition.

An important characteristic of this kind of logic is the part that immediate inference plays in it. Thus, the proposition numbered 3, above, follows from No. 2, and No. 5 from No. 4. It will be observed that in both cases a universal proposition (or one that states the non-existence of something) follows from a particular proposition (or one that states the existence of something). All the immediate inferences are essentially of that nature. A particular proposition is never immediately inferable from a universal one. (It is true that from "no A exists" we can infer that "something not A exists;" but this is not properly an immediate inference,—it really supposes the additional premise that "something exists.") There are also immediate inferences raising and reducing the *order* of propositions. Thus, the proposition of the second order given in the last paragraph follows from "some S is a P." On the other hand, the inference holds,—

Some common character of the S's is wanting to everything except P's;
∴ Every S is a P.

The necessary and sufficient condition of the existence of a syllogistic conclusion from two premises is simple enough. There is a conclusion if, and only if, there is a middle term distributed in one premise and undistributed in the other. But the conclusion is of the kind called *spurious*[1] by De Morgan if, and only if, the middle term is affected by a "some" in both premises. For example, let the two premises be,—

Every object of the class S wants some character of the group μ;
Every object of the class P possesses some character not of the group μ.

The middle term μ is distributed in the second premise, but not in the first; so that a conclusion can be drawn. But, though both propositions are universal, μ is under a "some" in both; hence only a spurious conclusion can be drawn, and in point of fact we can infer both of the following:—

Every object of the class S wants a character other than some character common to the class P;
Every object of the class P possesses a character other than some character wanting to every object of the class S.

The order of the conclusion is always the sum of the orders of the premises; but to draw up a rule to determine precisely what the conclusion is, would be difficult. It would at the same time be useless, because the problem is extremely simple when considered in the light of the logic of relatives.

Note B: The Logic of Relatives

P 268d: 187–203

A dual relative term, such as "lover," "benefactor," "servant," is a common name signifying a pair of objects. Of the two members of

1. On *spurious* propositions, see Mr. B. I. Gilman's paper in the *Johns Hopkins University Circular* for August, 1882. The number of such forms in any order is probably finite.

the pair, a determinate one is generally the first, and the other the second; so that if the order is reversed, the pair is not considered as remaining the same.

Let A, B, C, D, etc., be all the individual objects in the universe; then all the individual pairs may be arrayed in a block, thus:—

A : A	A : B	A : C	A : D	etc.
B : A	B : B	B : C	B : D	etc.
C : A	C : B	C : C	C : D	etc.
D : A	D : B	D : C	D : D	etc.
etc.	etc.	etc.	etc.	etc.

A general relative may be conceived as a logical aggregate of a number of such individual relatives. Let l denote "lover;" then we may write

$$l = \Sigma_i \Sigma_j (l)_{ij} (I:J)$$

where $(l)_{ij}$ is a numerical coëfficient, whose value is 1 in case I is a lover of J, and 0 in the opposite case, and where the sums are to be taken for all individuals in the universe.

Every relative term has a negative (like any other term) which may be represented by drawing a straight line over the sign for the relative itself. The negative of a relative includes every pair that the latter excludes, and *vice versa.* Every relative has also a *converse,* produced by reversing the order of the members of the pair. Thus, the converse of "lover" is "loved." The converse may be represented by drawing a curved line over the sign for the relative, thus: $\breve{l}$. It is defined by the equation

$$(\breve{l})_{ij} = (l)_{ji}.$$

The following formulæ are obvious, but important:—

$$\bar{\bar{l}} = l \qquad \breve{\breve{l}} = l$$

$$\bar{\breve{l}} = \breve{\bar{l}}$$

$$(l \prec b) = (\bar{b} \prec \bar{l}) \qquad (l \prec b) = (\breve{l} \prec \breve{b}).$$

Relative terms can be aggregated and compounded like others. Using $+$ for the sign of logical aggregation, and the comma for the sign of logical composition (Boole's multiplication, here to be called non-relative or internal multiplication), we have the definitions

$$(l + b)_{ij} = (l)_{ij} + (b)_{ij}$$

$$(l, b)_{ij} = (l)_{ij} \times (b)_{ij}.$$

The first of these equations, however, is to be understood in a peculiar way: namely, the $+$ in the second member is not strictly addition, but an operation by which

$$0+0=0 \qquad 0+1=1+0=1+1=1.$$

Instead of $(l)_{ij}+(b)_{ij}$, we might with more accuracy write

$$0^{0^{(l)ij+(b)ij}}.$$

The main formulæ of aggregation and composition are

$$\left\{\begin{array}{l}\text{If } l\prec s \text{ and } b\prec s, \text{ then } l+b\prec s.\\ \text{If } s\prec l \text{ and } s\prec b, \text{ then } s\prec l,b.\end{array}\right\}$$

$$\left\{\begin{array}{l}\text{If } l+b\prec s, \text{ then } l\prec s \text{ and } b\prec s.\\ \text{If } s\prec l,b, \text{ then } s\prec l \text{ and } s\prec b.\end{array}\right\}$$

$$\left\{\begin{array}{l}(l+b),s\prec l,s+b,s.\\ (l+s),(b+s)\prec l,b+s.\end{array}\right\}$$

The subsidiary formulæ need not be given, being the same as in non-relative logic.

We now come to the combination of relatives. Of these, we denote two by special symbols; namely, we write

lb for lover of a benefactor,

and

$l \dagger b$ for lover of everything but benefactors.

The former is called a particular combination, because it implies the *existence* of something *loved by* its relate and a *benefactor of* its correlate. The second combination is said to be *universal,* because it implies the *non-existence* of anything except what is either loved by its relate or a benefactor of its correlate. The combination lb is called a relative product, $l \dagger b$ a relative sum. The l and b are said to be undistributed in both, because if $l\prec s$, then $lb\prec sb$ and $l\dagger b\prec s\dagger b$; and if $b\prec s$, then $lb\prec ls$ and $l\dagger b\prec l\dagger s$.

The two combinations are defined by the equations

$$(lb)_{ij}=\Sigma_x(l)_{ix}(b)_{xj}$$

$$(l\dagger b)_{ij}=\Pi_x\{(l)_{ix}+(b)_{xj}\}.$$

The sign of addition in the last formula has the same signification as in the equation defining non-relative multiplication.

Relative addition and multiplication are subject to the associative law. That is,

$$l \dagger (b \dagger s) = (l \dagger b) \dagger s,$$
$$l(bs) = (lb)s.$$

Two formulæ so constantly used that hardly anything can be done without them are

$$l(b \dagger s) \prec lb \dagger s,$$
$$(l \dagger b)s \prec l \dagger bs.$$

The former asserts that whatever is lover of an object that is benefactor of everything but a servant, stands to everything but servants in the relation of lover of a benefactor. The latter asserts that whatever stands to any servant in the relation of lover of everything but its benefactors, is a lover of everything but benefactors of servants. The following formulæ are obvious and trivial:—

$$ls + bs \prec (l + b)s$$
$$l, b \dagger s \prec (l \dagger s), (b \dagger s).$$

Unobvious and important, however, are these:—

$$(l + b)s \prec ls + bs$$
$$(l \dagger s), (b \dagger s) \prec l, b \dagger s.$$

There are a number of curious development formulæ. Such are

$$(l, b)s = \Pi_p\{l(s, p) + b(s, \bar{p})\}$$
$$l(b, s) = \Pi_p\{(l, p)b + (l, \bar{p})s\}$$
$$(l + b) \dagger s = \Sigma_p\{[l \dagger (s + p)], [b \dagger (s + \bar{p})]\}$$
$$l \dagger (b + s) = \Sigma_p\{[(l + p) \dagger b], [(l + \bar{p}) \dagger s]\}.$$

The summations and multiplications denoted by Σ and Π are to be taken non-relatively, and all relative terms are to be successively substituted for p.

The negatives of the combinations follow these rules:

$$\overline{l + b} = \bar{l}, \bar{b} \qquad \overline{l, b} = \bar{l} + \bar{b}$$
$$\overline{l \dagger b} = \bar{l}\bar{b} \qquad \overline{lb} = \bar{l} \dagger \bar{b}.$$

The converses of combinations are as follows:—

$$\breve{\overline{l + b}} = \breve{l} + \breve{b} \qquad \breve{\overline{l, b}} = \breve{l}, \breve{b}$$
$$\breve{\overline{l \dagger b}} = \breve{b} \dagger \breve{l} \qquad \breve{\overline{lb}} = \breve{b}\breve{l}.$$

Individual dual relatives are of two types,—

$$\mathrm{A}:\mathrm{A} \qquad \text{and} \qquad \mathrm{A}:\mathrm{B}.$$

Relatives containing no pair of an object with itself are called *alio-relatives* as opposed to *self-relatives.* The negatives of alio-relatives pair every object with itself. Relatives containing no pair of an object with anything but itself are called *concurrents* as opposed to *opponents.* The negatives of concurrents pair every object with every other.

There is but one relative which pairs every object with itself and with every other. It is the aggregate of all pairs, and is denoted by ∞. It is translated into ordinary language by "coexistent with." Its negative is 0. There is but one relative which pairs every object with itself and none with any other. It is

$$(\mathrm{A}:\mathrm{A}) + (\mathrm{B}:\mathrm{B}) + (\mathrm{C}:\mathrm{C}) + \text{etc.};$$

is denoted by 1, and in ordinary language is "identical with ———." Its negative, denoted by $\mathfrak{n}$, is "other than ———," or "not."

No matter what relative term x may be, we have

$$0 \prec x \qquad x \prec \infty.$$

Hence, obviously

$$x + 0 = x \qquad x, \infty = x$$
$$x + \infty = \infty \qquad x, 0 = 0.$$

The last formulæ hold for the relative operations; thus,

$$x \dagger \infty = \infty \qquad x0 = 0.$$
$$\infty \dagger x = \infty \qquad 0x = 0.$$

The formulæ

$$x + 0 = x \qquad x, \infty = x$$

also hold if we substitute the relative operations, and also 1 for ∞, and $\mathfrak{n}$ for 0; thus,

$$x \dagger \mathfrak{n} = x \qquad x1 = x.$$
$$\mathfrak{n} \dagger x = x \qquad 1x = x.$$

We have also

$$l + \bar{l} = \infty \qquad l, \bar{l} = 0.$$

To these partially correspond the following pair of highly important formulæ:—

$$1 \prec l \dagger \breve{\bar{l}} \qquad l\breve{\bar{l}} \prec \mathrm{n}.$$

The logic of relatives is highly multiform; it is characterized by innumerable immediate inferences, and by various distinct conclusions from the same sets of premises. An example of the first character is afforded by Mr. Mitchell's F_{1v} following from $F_{1v'}$. As an instance of the second, take the premises,

Every man is a lover of an animal;

and

Every woman is a lover of a non-animal.

From these we can equally infer that

Every man is a lover of something which stands to each woman in the relation of not being the only thing loved by her,

and that

Every woman is a lover of something which stands to each man in the relation of not being the only thing loved by him.

The effect of these peculiarities is that this algebra cannot be subjected to hard and fast rules like those of the Boolian calculus; and all that can be done in this place is to give a general idea of the way of working with it. The student must at the outset disabuse himself of the notion that the chief instruments of algebra are the inverse operations. General algebra hardly knows any inverse operations. When an inverse operation is identical with a direct operation with an inverse quantity (as subtraction is the addition of the negative, and as division is multiplication by the reciprocal), it is useful; otherwise it is almost always useless. In ordinary algebra, we speak of the "principal value" of the logarithm, etc., which is a direct operation substituted for an indefinitely ambiguous inverse operation. The elimination and transposition in this algebra really does depend, however, upon formulæ quite analogous to the

$$x + (-x) = 0 \qquad x \times \frac{1}{x} = 1,$$

of arithmetical algebra. These formulæ are

$$l,\bar{l}=0 \qquad l\breve{\bar{l}}\prec\mathfrak{n}$$

$$l+\bar{l}=\infty \qquad 1\prec l\dagger\breve{\bar{l}}.$$

For example, to eliminate s from the two propositions

$$1\prec l\bar{s} \qquad 1\prec\breve{s}b,$$

we relatively multiply them in such an order as to bring the two s's together, and then apply the second of the above formulæ, thus:—

$$1\prec l\bar{s}\breve{s}b\prec l\mathfrak{n}b.$$

This example shows the use of the association formulæ in bringing letters together. Other formulæ of great importance for this purpose are

$$(b\dagger l)s\prec b\dagger ls \qquad b(l\dagger s)\prec bl\dagger s.$$

The distribution formulæ are also useful for this purpose.

When the letter to be eliminated has thus been replaced by one of the four relatives, — 0, ∞, 1, $\mathfrak{n}$, — the replacing relative can often be got rid of by means of one of the formulæ

$$l+0=l \qquad l,\infty=l$$

$$l\dagger\mathfrak{n}=\mathfrak{n}\dagger l=l \qquad l1=1l=l.$$

When we have only to deal with universal propositions, it will be found convenient so to transpose everything from subject to predicate as to make the subject 1. Thus, if we have given $l\prec b$, we may relatively add $\breve{\bar{l}}$ to both sides; whereupon we have

$$1\prec l\dagger\breve{\bar{l}}\prec b\dagger\breve{\bar{l}}.$$

Every proposition will then be in one of the forms

$$1\prec b\dagger l \qquad 1\prec bl.$$

With a proposition of the form $1\prec b\dagger l$, we have the right (1) to transpose the terms, and (2) to convert the terms. Thus, the following are equivalent:—

$$1\prec b\dagger l$$

$$1\prec l\dagger b \qquad 1\prec\breve{b}\dagger\breve{l}$$

$$1\prec\breve{l}\dagger\breve{b}.$$

With a proposition of the form $1\prec bl$, we have only the right to convert the predicate giving $1\prec\breve{l}\breve{b}$.

With three terms, there are four forms of universal propositions, namely:—

$$1 \prec l \dagger b \dagger s \quad 1 \prec l(b \dagger s) \quad 1 \prec lb \dagger s \quad 1 \prec lbs.$$

Of these, the third is an immediate inference from the second.

By way of illustration, we may work out the syllogisms whose premises are the propositions of the first order referred to in Note A. Let a and c be class terms, and let β be a group of characters. Let p be the relative "possessing as a character." The non-relative terms are to be treated as relatives,—a, for instance, being considered as "a coexistent with" and $\breve{a}$ as "coexistent with a that is." Then, the six forms of affirmative propositions of the first order are

$$1 \prec \breve{a} \dagger p \dagger \beta$$
$$1 \prec \breve{a}(p \dagger \beta) \qquad 1 \prec (\breve{a} \dagger p)\beta$$
$$1 \prec \breve{a}p \dagger \beta \qquad 1 \prec \breve{a} \dagger p\beta$$
$$1 \prec \breve{a}p\beta.$$

The various kinds of syllogism are as follows:—

1. *Premises:* $1 \prec \breve{a} \dagger p \dagger \beta \qquad 1 \prec \breve{c} \dagger p \dagger \bar{\beta}.$

Convert one of the premises and multiply,

$$1 \prec (\breve{a} \dagger p \dagger \beta)(\breve{\bar{\beta}} \dagger \breve{p} \dagger c) \prec \breve{a} \dagger p \dagger \beta\breve{\bar{\beta}} \dagger \breve{p} \dagger c \prec \breve{a} \dagger p \dagger n \dagger \breve{p} \dagger c \prec \breve{a} \dagger p \dagger \breve{p} \dagger c.$$

The treatment would be the same if one or both of the premises were negative; that is, contained $\bar{p}$ in place of p.

2. *Premises:* $1 \prec \breve{a} \dagger p \dagger \beta \qquad 1 \prec \breve{c}(p \dagger \bar{\beta}).$

We have

$$1 \prec (\breve{a} \dagger p \dagger \beta)(\breve{\bar{\beta}} \dagger \breve{p})c \prec (\breve{a} \dagger p \dagger \breve{p})c.$$

The same with negatives.

3. *Premises:* $1 \prec \breve{a}(p \dagger \beta) \qquad 1 \prec \breve{c}(p \dagger \bar{\beta}).$

$$1 \prec \breve{a}(p \dagger \beta)(\breve{\bar{\beta}} \dagger \breve{p})c \prec \breve{a}(p \dagger \breve{p})c.$$

The same with negatives.

4. *Premises:* $1 \prec \breve{a} \dagger p \dagger \beta \qquad 1 \prec (\breve{c} \dagger p)\bar{\beta}.$

$$1 \prec (\breve{a} \dagger p \dagger \beta)\breve{\bar{\beta}}(\breve{p} \dagger c) \prec (\breve{a} \dagger p \dagger \beta\breve{\bar{\beta}})(\breve{p} \dagger c) \prec (\breve{a} \dagger p)(\breve{p} \dagger c).$$

If one of the premises, say the first, were negative, we should obtain a similar conclusion,—

$$1 -\!\!< (\breve{a} \dagger \bar{p})(\breve{p} \dagger c);$$

but from this again p could be eliminated, giving

$$1 -\!\!< \breve{a} \dagger c, \qquad \text{or} \qquad \bar{a} -\!\!< c.$$

5. *Premises:* $1 -\!\!< \breve{a}(p \dagger \beta) \qquad 1 -\!\!< (\breve{c} \dagger p)\bar{\beta}.$

$$1 -\!\!< \breve{a}(p \dagger \beta)\breve{\bar{\bar{\beta}}}(\breve{p} \dagger c) -\!\!< \breve{a}p(\breve{p} \dagger c).$$

If either premise were negative, p could be eliminated, giving $1 -\!\!< \breve{a}c$, or some a is c.

6. *Premises:* $1 -\!\!< (\breve{a} \dagger p)\beta \qquad 1 -\!\!< (\breve{c} \dagger p)\bar{\beta}.$

$$1 -\!\!< (\breve{a} \dagger p)\beta\breve{\bar{\bar{\beta}}}(\breve{p} \dagger c) -\!\!< (\breve{a} \dagger p)\,\mathfrak{n}\,(\breve{p} \dagger c).$$

7. *Premises:* $1 -\!\!< \breve{a} \dagger p \dagger \beta \qquad 1 -\!\!< \breve{c}p \dagger \bar{\beta}.$

$$1 -\!\!< (\breve{a} \dagger p \dagger \beta)(\breve{\bar{\bar{\beta}}} \dagger \breve{p}c) -\!\!< \breve{a} \dagger p \dagger \breve{p}c.$$

8. *Premises:* $1 -\!\!< \breve{a}(p \dagger \beta) \qquad 1 -\!\!< \breve{c}p \dagger \bar{b}.$

$$1 -\!\!< \breve{a}(p \dagger \beta)(\breve{\bar{\bar{\beta}}} \dagger \breve{p}c) -\!\!< \breve{a}(p \dagger \breve{p}c).$$

9. *Premises:* $1 -\!\!< (\breve{a} \dagger p)\beta \qquad 1 -\!\!< \breve{c}p \dagger \bar{\beta}.$

$$1 -\!\!< (\breve{a} \dagger p)\beta(\breve{\bar{\bar{\beta}}} \dagger \breve{p}c) -\!\!< (\breve{a} \dagger p)\breve{p}c.$$

If one premise is negative, we have the further conclusion $1 -\!\!< \breve{a}c$.

10. *Premises:* $1 -\!\!< \breve{a}p \dagger \beta \qquad 1 -\!\!< \breve{c}p \dagger \bar{\beta}.$

$$1 -\!\!< (\breve{a}p \dagger \beta)(\breve{\bar{\bar{\beta}}} \dagger \breve{p}c) -\!\!< \breve{a}p \dagger \breve{p}c.$$

11. *Premises:* $1 -\!\!< \breve{a} \dagger p \dagger \beta \qquad 1 -\!\!< \breve{c} \dagger p\bar{\beta}.$

$$1 -\!\!< (\breve{a} \dagger p \dagger \beta)(\breve{\bar{\bar{\beta}}}\breve{p} \dagger c) -\!\!< (\breve{a} \dagger p)\breve{p} \dagger c.$$

We might also conclude

$$1 -\!\!< \breve{a} \dagger p \dagger \mathfrak{n}\breve{p} \dagger c;$$

but this conclusion is an immediate inference from the other; for

$$(\breve{a} \dagger p)\breve{p} \dagger c -\!\!< (\breve{a} \dagger p)(1 \dagger \mathfrak{n})\breve{p} \dagger c -\!\!< (\breve{a} \dagger p)1 \dagger \mathfrak{n}\breve{p} \dagger c -\!\!< \breve{a} \dagger p \dagger \mathfrak{n}\breve{p} \dagger c.$$

If one premise is negative, we have the further conclusion $1 -\!\!< \breve{a} \dagger c$.

12. *Premises:* $1 -\!\!< \breve{a}(p \dagger \beta) \qquad 1 -\!\!< \breve{c} \dagger p\bar{\beta}.$

$$1 -\!\!< \breve{a}(p \dagger \beta)(\breve{\bar{\bar{\beta}}}\breve{p} \dagger c) -\!\!< \breve{a}(p\breve{p} \dagger c).$$

If one premise is negative, we have the further inference $1 -\!\!< \breve{a}c$.

13. *Premises:* $1 \prec (\breve{a} \dagger p)\beta \qquad 1 \prec \breve{c} \dagger p\bar{\beta}$.

$$1 \prec (\breve{a} \dagger p)\beta(\breve{\bar{\beta}}\breve{p} \dagger c) \prec (\breve{a} \dagger p)(\mathfrak{n}\breve{p} \dagger c).$$

14. *Premises:* $1 \prec \breve{a}p \dagger \beta \qquad 1 \prec \breve{c} \dagger p\bar{\beta}$.

$$1 \prec (\breve{a}p \dagger \beta)(\breve{\bar{\beta}}\breve{p} \dagger c) \prec \breve{a}p\breve{p} \dagger c.$$

If one premise is negative, we have the further spurious inference $1 \prec \breve{a}\mathfrak{n} \dagger c$.

15. *Premises:* $1 \prec \breve{a} \dagger p\beta \qquad 1 \prec \breve{c} \dagger p\bar{\beta}$.

$$1 \prec (\breve{a} \dagger p\beta)(\breve{\bar{\beta}}\breve{p} \dagger c) \prec \breve{a} \dagger p(\mathfrak{n}\breve{p} \dagger c).$$

We can also infer $1 \prec (\breve{a} \dagger p\mathfrak{n})\breve{p} \dagger c$.

16. *Premises:* $1 \prec \breve{a} \dagger p \dagger \beta \qquad 1 \prec \breve{c}p\bar{\beta}$.

$$1 \prec (\breve{a} \dagger p \dagger \beta)\breve{\bar{\beta}}\breve{p}c \prec (\breve{a} \dagger p)\breve{p}c.$$

If one premise is negative, we can further infer $1 \prec \breve{a}c$.

17. *Premises:* $1 \prec \breve{a}(p \dagger \beta) \qquad 1 \prec \breve{c}p\bar{\beta}$.

$$1 \prec \breve{a}(p \dagger \beta)\breve{\bar{\beta}}\breve{p}c \prec \breve{a}p\breve{p}c.$$

If one premise is negative, we have the further spurious conclusion $1 \prec \breve{a}\mathfrak{n}c$.

18. *Premises:* $1 \prec (\breve{a} \dagger p)\beta \qquad 1 \prec \breve{c}p\bar{\beta}$.

$$1 \prec (\breve{a} \dagger p)\beta\breve{\bar{\beta}}\breve{p}c \prec (\breve{a} \dagger p)\mathfrak{n}\breve{p}c.$$

19. *Premises:* $1 \prec \breve{a}p \dagger \beta \qquad 1 \prec \breve{c}p\bar{\beta}$.

$$1 \prec (\breve{a}p \dagger \beta)\breve{\bar{\beta}}\breve{p}c \prec \breve{a}p\breve{p}c.$$

If one premise is negative, we further conclude $1 \prec \breve{a}\mathfrak{n}c$.

20. *Premises:* $1 \prec \breve{a} \dagger p\beta \qquad 1 \prec \breve{c}p\bar{\beta}$.

$$1 \prec (\breve{a} \dagger p\beta)\breve{\bar{\beta}}\breve{p}c \prec (\breve{a} \dagger p\mathfrak{n})\breve{p}c.$$

21. *Premises:* $1 \prec \breve{a}p\beta \qquad 1 \prec \breve{c}p\bar{\beta}$.

$$1 \prec \breve{a}p\beta\breve{\bar{\beta}}\breve{p}c \prec \breve{a}p\mathfrak{n}\breve{p}c.$$

When we have to do with particular propositions, we have the proposition $\infty \,\bar{\prec}\, 0$, or "something exists;" for every particular proposition implies this. Then every proposition can be put into one or other of the four forms

$$\infty \prec 0 \dagger l \dagger 0$$
$$\infty \prec (0 \dagger l)\infty$$
$$\infty \prec 0 \dagger l\infty$$
$$\infty \prec \infty l \infty.$$

Each of these propositions immediately follows from the one above it. The *enveloped* expressions which form the predicates have the remarkable property that each is either 0 or ∞. This fact gives extraordinary freedom in the use of the formulæ. In particular, since if anything not zero is included under such an expression, the whole universe is included, it will be quite unnecessary to write the $\infty \prec$ which begins every proposition.

Suppose that f and g are general relatives signifying relations of things to times. Then, Dr. Mitchell's six forms of two dimensional propositions appear thus:—

$$F_{11} = 0 \dagger f \dagger 0$$
$$F_{1v} = 0 \dagger f\infty$$
$$F_{u1} = \infty f \dagger 0$$
$$F_{1v'} = (0 \dagger f)\infty$$
$$F_{u'1} = \infty(f \dagger 0)$$
$$F_{uv} = \infty f \infty.$$

It is obvious that $l \dagger 0 \prec l$, for

$$l \dagger 0 \prec l \dagger \mathrm{n} \prec l.$$

If then we have $0 \dagger f \dagger 0$ as one premise, and the other contains g, we may substitute for g the product (f, g).

$$g \prec g, \infty \prec g, (0 \dagger f \dagger 0) \prec g, f.$$

From the two premises

$$\infty(f \dagger 0) \qquad \text{and} \qquad 0 \dagger g\infty,$$

by the application of the formulæ

$$ls, (b \dagger \bar{s}) \prec (l, b)s$$

$$sl, (\bar{s} \dagger b) \prec s(l, b),$$

we have

$$\{\infty(f \dagger 0)\}, (0 \dagger g\infty) \prec \infty\{(f \dagger 0), g\infty\} \prec \infty(f, g)\infty.$$

These formulæ give the first column of Dr. Mitchell's rule on page 90.

The following formulæ may also be applied:—

$$\begin{array}{rll}
1. & (0\dagger f\dagger 0),(0\dagger g\dagger 0) & = 0\dagger(f,g)\dagger 0. \\
2. & (0\dagger f)\infty\ \ (0\dagger\breve{g}\dagger 0) & \prec (0\dagger f)(\breve{g}\dagger 0). \\
3. & (0\dagger f)\infty\ \ \infty(\breve{g}\dagger 0) & = (0\dagger f)(\breve{g}\dagger 0)+(0\dagger f)\mathfrak{n}(\breve{g}\dagger 0). \\
4. & (0\dagger f)\infty\ \ (0\dagger\breve{g})\infty & \prec (0\dagger f)\breve{g}\,\infty. \\
5. & (0\dagger f\dagger 0)(0\dagger\breve{g}\,\infty) & = 0\dagger(\breve{g}f,f)\dagger 0. \\
6. & (0\dagger f)\infty\ \ (0\dagger\breve{g}\,\infty) & = (0\dagger\breve{g}f,f)\infty. \\
7. & (0\dagger f)\infty,(0\dagger g\,\infty) & = (0\dagger f,g\,\infty)\infty. \\
8. & (0\dagger f\infty)(0\dagger g\,\infty) & = 0\dagger(f\breve{g},\breve{g}f)\infty. \\
9. & (0\dagger f\infty),(0\dagger g\,\infty) & = 0\dagger f\infty,g\,\infty. \\
10. & (0\dagger f\dagger 0)\infty\breve{g}\,\infty & = 0\dagger(f\breve{g}f,f)\dagger 0. \\
11. & (0\dagger f)\infty\ \ \infty\breve{g}\,\infty & = (0\dagger f)\breve{g}\,\infty+(0\dagger f)\mathfrak{n}\breve{g}\,\infty. \\
12. & (0\dagger f\infty)\ \ \infty\breve{g}\,\infty & = (0\dagger f\breve{g}\,\infty)+(0\dagger f\mathfrak{n}\breve{g}\,\infty). \\
13. & \infty f\infty\ \ \infty\breve{g}\,\infty & = \infty f\breve{g}\,\infty+\infty f\mathfrak{n}\breve{g}\,\infty.
\end{array}$$

When the relative and non-relative operations occur together, the rules of the calculus become pretty complicated. In these cases, as well as in such as involve *plural* relations (subsisting between three or more objects), it is often advantageous to recur to the numerical coëfficients mentioned on page 454. Any proposition whatever is equivalent to saying that some complexus of aggregates[1] and products of such numerical coëfficients is greater than zero. Thus,

$$\Sigma_i\Sigma_j l_{ij} > 0$$

means that something is a lover of something; and

$$\Pi_i\Sigma_j l_{ij} > 0$$

means that everything is a lover of something. We shall, however, naturally omit, in writing the inequalities, the > 0 which terminates them all; and the above two propositions will appear as

$$\Sigma_i\Sigma_j l_{ij} \quad \text{and} \quad \Pi_i\Sigma_j l_{ij}.$$

The following are other examples:—

$$\Pi_i\Sigma_j(l)_{ij}(b)_{ij}$$

means that everything is at once a lover and a benefactor of something.

$$\Pi_i\Sigma_j(l)_{ij}(b)_{ji}$$

means that everything is a lover of a benefactor of itself.

$$\Sigma_i\Sigma_k\Pi_j(l_{ij}+b_{jk})$$

1. The sums of pages 454-55.

means that there is something which stands to something in the relation of loving everything except benefactors of it.

Let α denote the triple relative "accuser to —— of ——," and ϵ the triple relative "excuser to —— of ——." Then,

$$\Sigma_i \Pi_j \Sigma_k (\alpha)_{ijk} (\epsilon)_{jki}$$

means that an individual i can be found, such, that taking any individual whatever, j, it will always be possible so to select a third individual, k, that i is an accuser to j of k, and j an excuser to k of i.

Let π denote "preferrer to —— of ——." Then,

$$\Pi_i \Sigma_j \Sigma_k (\alpha)_{ijk} (\epsilon_{jki} + \pi_{kij})$$

means that, having taken any individual i whatever, it is always possible so to select two, j and k, that i is an accuser to j of k, and also is either excused by j to k or is something to which j is preferred by k.

When we have a number of premises expressed in this manner, the conclusion is readily deduced by the use of the following simple rules. In the first place, we have

$$\Sigma_i \Pi_j \prec \Pi_j \Sigma_i .$$

In the second place, we have the formulæ

$$\{\Pi_i \varphi(i)\}\{\Pi_j \psi(j)\} = \Pi_i \{\varphi(i) \cdot \psi(i)\}.$$
$$\{\Pi_i \varphi(i)\}\{\Sigma_j \psi(j)\} \prec \Sigma_i \{\varphi(i) \cdot \psi(i)\}.$$

In the third place, since the numerical coëfficients are all either *zero* or *unity*, the Boolian calculus is applicable to them.

The following is one of the simplest possible examples. Required to eliminate *servant* from these two premises:

First premise. There is somebody who accuses everybody to everybody, unless the latter is loved by some person that is servant of all not accused to him.

Second premise. There are two persons, the first of whom excuses everybody to everybody, unless the unexcused be benefited by, without the person to whom he is unexcused being a servant of, the second.

These premises may be written thus:

$$\Sigma_h \Pi_i \Sigma_j \Pi_k (\alpha_{hik} + s_{jk} l_{ji}).$$
$$\Sigma_u \Sigma_v \Pi_x \Pi_y (\epsilon_{uyx} + \bar{s}_{yv} b_{vx}).$$

The second yields the immediate inference,

$$\Pi_x \Sigma_u \Pi_y \Sigma_v (\epsilon_{uyx} + \bar{s}_{yv} b_{vx}).$$

Combining this with the first, we have

$$\Sigma_x \Sigma_u \Sigma_y \Sigma_v (\epsilon_{uyx} + \bar{s}_{yv} b_{vx})(a_{xuv} + s_{yv} l_{yu}).$$

Finally, applying the Boolian calculus, we deduce the desired conclusion

$$\Sigma_x \Sigma_u \Sigma_y \Sigma_v (\epsilon_{uyx} \alpha_{xuv} + \epsilon_{uyx} l_{yu} + \alpha_{xuv} b_{vx}).$$

The interpretation of this is that either there is somebody excused by a person to whom he accuses somebody, or somebody excuses somebody to his (the excuser's) lover, or somebody accuses his own benefactor.

The procedure may often be abbreviated by the use of operations intermediate between Π and Σ. Thus, we may use Π', Π'', etc. to mean the products for all individuals except one, except two, etc. Thus,

$$\Pi'_i \Pi''_j (l_{ij} + b_{ji})$$

will mean that every person except one is a lover of everybody except its benefactors, and at most two non-benefactors. In the same manner, Σ', Σ'', etc. will denote the sums of all products of two, of all products of three, etc. Thus,

$$\Sigma''_i (l_{ii})$$

will mean that there are at least three things in the universe that are lovers of themselves. It is plain that if $m < n$, we have

$$\Pi^m \prec \Pi^n \qquad \Sigma^n \prec \Sigma^m.$$
$$(\Pi_i^m \varphi i)(\Sigma_j^n \psi j) \prec \Sigma_i^{n-m} (\varphi i \cdot \psi i)$$
$$(\Pi_i^m \varphi i)(\Pi_j^n \psi j) \prec \Pi_i^{m+n} (\varphi i \cdot \psi i).$$

Mr. Schlötel has written to the London Mathematical Society, accusing me of having, in my "Algebra of Logic," plagiarized from his writings. He has also written to me to inform me that he has read that Memoir with "heitere Ironie," and that Professor Drobisch, the Berlin Academy, and I constitute a "liederliche Kleeblatt," with many other things of the same sort. Up to the time of publishing my Memoir, I had never seen any of Mr. Schlötel's writings; I have since procured his *Logik*, and he has been so obliging as to send me two cuttings from his papers, thinking, apparently, that I might be curious to see the passages that I had appropriated. But having examined these productions, I find no thought in them that I ever did, or ever should be likely to put forth as my own.

A Communication from Mr. Peirce

P 245: **Johns Hopkins University Circulars**
2:22 (April 1883): 86–88

Readers of Professor Sylvester's communication entitled *"Erratum"* in the last number of these Circulars have perhaps inferred that my conduct in the matter there referred to had been in fault. Professor Sylvester's *"Erratum"* relates to his "Word on Nonions," printed in the *Johns Hopkins University Circulars No.* 17, *p.* 242. In that article appears this sentence: "These forms [*i.e.* a certain group of nine Forms belonging to the algebra of Nonions] can be derived from an algebra given by Mr. Charles S. Peirce, (*Logic of Relatives,* 1870)." The object of Professor Sylvester's *"Erratum"* would seem to be to say that this sentence was inserted by me in his proof-sheet without his knowledge or authority on the occasion of the proof being submitted to me to supply a reference, and to repudiate the sentence, because he "knows nothing whatever" of the fact stated.

But I think this view of Professor Sylvester's meaning is refuted by simply citing the following testimony of Professor Sylvester himself, printed in the *Johns Hopkins University Circulars, No.* 15, *p.* 203.

"Mr. Sylvester mentioned . . . that . . . he had come upon a system of Nonions, the exact analogues of the Hamiltonian Quaternions. . . . Mr. Charles S. Peirce, it should be stated, had to the certain knowledge of Mr. Sylvester arrived at the same result many years ago in connection with his theory of the *logic of relatives;* but whether the result had been published by Mr. Peirce, he was unable to say."

This being so, I think that on the occasion of Professor Sylvester's publishing these forms I was entitled to some mention, if I had already published them, and *a fortiori* if I had not. When the proof-sheet was put into my hands, the request made to me, by an oral message, was not simply to supply a reference but to correct a statement relating to my work in the body of the text. And I had no reason to suppose that having thus submitted his text to me, Professor Sylvester would omit to look at his proof-sheet after it left my hands to see whether or not he approved of such alteration as I might have proposed. At any rate, when from these causes Professor Sylvester's "Word upon Nonions" had been published with the above statement concerning me, would it have been too much to expect that he should take the trouble to refer to my memoir in order to see whether the statement was not after all true, before publicly protesting against it?

I will now explain what the system of Nonions consists in and how I have been concerned with it.

The calculus of Quaternions, one of the greatest of all mathematical discoveries, is a certain system of algebra applied to geometry. A quaternion is a four-dimensional quantity; that is to say, its value cannot be precisely expressed without the use of a set of four numbers. It is much as if a geographical position should be expressed by a single algebraical letter; the value of this letter could only be defined by the use of two numbers, say the Latitude and Longitude. There are various ways in which a quaternion may be conceived to be measured and various different sets of four numbers by which its value may be defined. Of all these modes, Hamilton, the author of the algebra, selected one as the standard. Namely, he conceived the general quaternion q to be put into the form

$$q = xi + yj + zk + w,$$

where x, y, z, w are four ordinary numbers, while i, j, k are peculiar units, subject to singular laws of multiplication. For $ij = -ji$, the order of the factors being material, as shown in this multiplication table, where the first factor is entered at the side, the second at the top, and the product is found in the body of the table.

	1	i	j	k
1	1	i	j	k
i	i	-1	k	$-j$
j	j	$-k$	-1	i
k	k	j	$-i$	-1

As long as x, y, z, and w in Hamilton's standard tetranomial form are confined to being *real* numbers, as he usually supposed them to be, no simpler or more advantageous form of conceiving the measurement of a quaternion can be found. But my father, Benjamin Peirce, made the profound, original, and pregnant discovery that when x, y, z, w are permitted to be imaginaries, there is another very different and preferable system of measurement of a quaternion. Namely, he showed that the general quaternion, q, can be put into the form

$$q = xi + yj + zk + wl,$$

where x, y, z, w are real or imaginary numbers, while i, j, k, l are peculiar units whose multiplication obeys this table.

	i	j	k	l
i	i	j	0	0
j	0	0	i	j
k	k	l	0	0
l	0	0	k	l

A quaternion does not cease to be a quaternion by being measured upon one system rather than another. Any quantity belonging to the algebra is a quaternion; the algebra itself is "quaternions." The usual formulae of the calculus have no reference to any tetranomial form, and such a form might even be dispensed with altogether.

While my father was making his investigations in multiple algebra I was, in my humble way, studying the logic of relatives and an algebraic notation for it; and in the ninth volume of the *Memoirs of the American Academy of Arts and Sciences,* appeared my first paper upon the subject. In this memoir, I was led, from logical considerations that are patent to those who read it, to endeavor to put the general expression of any linear associative algebra into a certain form; namely as a linear expression in certain units which I wrote thus:

$$\begin{array}{llll}
(u_1 : u_1) & (u_1 : u_2) & (u_1 : u_3), & \text{etc.,} \\
(u_2 : u_1) & (u_2 : u_2) & (u_2 : u_3), & \text{etc.,} \\
(u_3 : u_1) & (u_3 : u_2) & (u_3 : u_3), & \text{etc.,} \\
\text{etc.} & \text{etc.} & \text{etc.} &
\end{array}$$

These forms, in their multiplication, follow these rules:

$$(u_a : u_b)(u_b : u_c) = (u_a : u_c) \quad (u_a : u_b)(u_c : u_d) = 0.$$

I said, "I can assert, upon reasonable inductive evidence, that all such algebras can be interpreted on the principles of the present notation in the same way," and consequently can be put into this form. I afterwards published a proof of this. I added that this amounted to saying that "all such algebras are complications and modifications of the algebra of . . . Hamilton's quaternions." What I meant by this appears plainly in the memoir. It is that any algebra that can be put into the form proposed by me is thereby referred to an algebra of a certain class (afterwards named *quadrates* by Professor Clifford) which present so close an analogy with quaternions that they may all be considered as mere complications of that algebra. Of these algebras, I gave as an example, the multiplication table of that one which Professor Clifford afterward named *nonions*.[1] This is the passage: "For example, if we have three classes of individuals, u_1, u_2, u_3, which are related to one another in pairs, we may put

$$\begin{array}{lll} u_1 : u_1 = i & u_1 : u_2 = j & u_1 : u_3 = k \\ u_2 : u_1 = l & u_2 : u_2 = m & u_2 : u_3 = n \\ u_3 : u_1 = o & u_3 : u_2 = p & u_3 : u_3 = q \end{array}$$

and by (155) we get the multiplication table

	i	j	k	l	m	n	o	p	q
i	i	j	k	0	0	0	0	0	0
j	0	0	0	i	j	k	0	0	0
k	0	0	0	0	0	0	i	j	k
l	l	m	n	0	0	0	0	0	0
m	0	0	0	l	m	n	0	0	0
n	0	0	0	0	0	0	l	m	n
o	o	p	q	0	0	0	0	0	0
p	0	0	0	o	p	q	0	0	0
q	0	0	0	0	0	0	o	p	q

It will be seen that the system of nonions is not a group but an algebra; that just as the word "quaternion" is not restricted to the three perpendicular vectors and unity, so a nonion is any quantity of this nine-fold algebra.

1. It would have been more accurately analogical, perhaps, to call it *novenions*.

So much was published by me in 1870; and it then occurred either to my father or to me (probably in conversing together) that since this algebra was thus shown (through his form of quaternions) to be the strict analogue of quaternions, there ought to be a form of it analogous to Hamilton's standard tetranomial form of quaternions. That form, either he or I certainly found. I cannot remember, after so many years, which first looked for it; whichever did must have found it at once. I cannot tell what his method of search would have been, but I can show what my own must have been. The following course of reasoning was so obtrusive that I could not have missed it.

Hamilton's form of quaternions presents a group of four square-roots of unity. Are there, then, in nonions, nine independent cube-roots of unity, forming a group? Now, unity upon my system of notation was written thus:

$$(u_1 : u_1) + (u_2 : u_2) + (u_3 : u_3).$$

Two independent cube-roots of this suggest themselves at once, they are

$$(u_1 : u_2) + (u_2 : u_3) + (u_3 : u_1)$$
$$(u_3 : u_2) + (u_2 : u_1) + (u_1 : u_3).$$

In fact these are hinted at in my memoir, p. 53. Then, it must have immediately occurred to me, from the most familiar properties of the imaginary roots of unity, that instead of the coëfficients

$$1, \quad 1, \quad 1,$$

I might substitute

$$1, \quad g, \quad g^2,$$

or

$$1, \quad g^2, \quad g,$$

where g is an imaginary cube-root of unity. The nine cube-roots of unity thus obtained are obviously independent and obviously form a group. Thus the problem is solved by a method applicable to any other quadrate.

My father, with his strong partiality for my performances, talked a good deal about the algebra of nonions in general and these forms in particular; and they became rather widely known as mine. Yet it is clear that the only real merit in the discovery lay in my father's transformation of quaternions. In 1875, when I was in Germany, my father wrote to me that he was going to print a miscellaneous paper on multiple algebra and he wished to have it accompanied by a paper by me, giving an account of what I had found out. I wrote such a paper, and sent it to him; but somehow all but the first few pages of the manuscript were lost, a circumstance I never discovered till I saw the part that had reached him (and which he took for the whole) in print. I did not afterward publish the matter, because I did not attach much importance to it, and because I thought that too much had been made, already, of the very simple things I had done.

I here close the narrative. The priority of publication of the particular group referred to belongs to Professor Sylvester. But most readers will agree that he could not have desired to print it without making any allusion to my work, and that to say the group could be derived from my algebra was not too much.

A Problem relating to the Construction of a reversible pendulum

MS 450: Spring–Summer 1883

Datum. A pendulum consists of two rigid parts, one above the knife-edge, the other below. These two parts are joined at the knife-edge and there is a finite resistance to bending at the joint.

Quaesitum. How stiff must this joint be in order that the period of oscillation shall not be affected by the bending by more than one two-millionth part?

Solution. Adopt this notation

$\left.\begin{matrix}\varphi_1\\ \varphi_2\end{matrix}\right\}$	angle of displacement of the $\left\{\begin{matrix}\text{lower}\\ \text{upper}\end{matrix}\right\}$ part from the vertical
$\left.\begin{matrix}I_1\\ I_2\end{matrix}\right\}$	moments of inertia
$\gamma_1\ \gamma_2$	Moments of gravity
ϵ	Elasticity of the joint

Then the equations are

$$I_1 D_t^2 \varphi_1 = -\gamma_1 \varphi_1 - \epsilon(\varphi_1 - \varphi_2)$$

$$I_2 D_t^2 \varphi_2 = +\gamma_2 \varphi_2 + \epsilon(\varphi_1 - \varphi_2)$$

The first equation gives

$$\varphi_2 = \frac{I_1}{\epsilon} D_t^2 \varphi_1 + \frac{\epsilon + \gamma_1}{\epsilon} \varphi_1$$

$$D_t^2 \varphi_2 = \frac{I_1}{\epsilon} D_t^4 \varphi_1 + \frac{\epsilon + \gamma_1}{\epsilon} D_t^2 \varphi_1$$

Substituting these values in the second equation we get

$$0 = \frac{I_1 I_2}{\epsilon} D_t^4 \varphi_1 + \left(I_2 \frac{\epsilon + \gamma_1}{\epsilon} + I_1 \frac{\epsilon - \gamma_2}{\epsilon} \right) D_t^2 \varphi_1 - \left(\epsilon - \frac{(\epsilon + \gamma_1)(\epsilon - \gamma_2)}{\epsilon} \right) \varphi_1$$

or, separating the operator of φ_1 into factors

$$\Bigg\{ I_1 I_2 D_t^2 + \frac{1}{2}[I_2(\epsilon + \gamma_1) + I_1(\epsilon - \gamma_2)] + \sqrt{I_1 I_2 \epsilon^2 + \left(\frac{I_2(\epsilon + \gamma_1) - I_1(\epsilon - \gamma_2)}{2} \right)^2} \Bigg\}$$

$$\times \Bigg\{ I_1 I_2 D_t^2 + \frac{1}{2}[I_2(\epsilon + \gamma_1) + I_1(\epsilon - \gamma_2)] - \sqrt{I_1 I_2 \epsilon^2 + \left(\frac{I_2(\epsilon + \gamma_1) - I_1(\epsilon - \gamma_2)}{2} \right)^2} \Bigg\} \varphi_1 = 0$$

The quantity under the radical sign may be transformed as follows

$$I_1 I_2 \epsilon^2 + \left(\frac{I_2(\epsilon + \gamma_1) - I_1(\epsilon - \gamma_2)}{2} \right)^2 = I_1 I_2 \epsilon^2 + \left(\frac{(I_2 - I_1)\epsilon + (I_2 \gamma_1 + I_1 \gamma_2)}{2} \right)^2$$

$$= \frac{1}{4}(I_2 + I_1)^2 \epsilon^2 + \frac{1}{2}(I_2 - I_1)(I_2 \gamma_1 + I_1 \gamma_2)\epsilon + \frac{1}{4}(I_2 \gamma_1 + I_1 \gamma_2)^2$$

Since ϵ is very large, the square root of this quantity is approximately (neglecting ϵ^{-3})

$$\frac{1}{2}(I_2 + I_1)\epsilon \left(1 + \frac{(I_2 - I_1)(I_2 \gamma_1 + I_1 \gamma_2)}{(I_2 + I_1)^2 \epsilon} + \frac{1}{2} \left(\frac{I_2 \gamma_1 + I_1 \gamma_2}{(I_2 + I_1)\epsilon} \right)^2 - \frac{1}{2} \frac{(I_2 - I_1)^2 (I_2 \gamma_1 + I_1 \gamma_2)^2}{(I_2 + I_1)^4 \epsilon^2} \right)$$

$$= \frac{1}{2}(I_2 + I_1)\epsilon + \frac{1}{2} \frac{I_2 - I_1}{I_2 + I_1}(I_2 \gamma_1 + I_1 \gamma_2) + \frac{I_1 I_2}{(I_2 + I_1)^2}(I_2 \gamma_1 + I_1 \gamma_2)^2 \frac{1}{(I_2 + I_1)\epsilon}$$

Consequently the differential operator becomes

$$\left\{ I_1 I_2 D_t^2 + (I_2 + I_1)\epsilon + \frac{I_2^2 \gamma_1 - I_1^2 \gamma_2}{I_2 + I_1} + \frac{I_1 I_2}{(I_2 + I_1)^2}(I_2 \gamma_1 + I_1 \gamma_2)^2 \frac{1}{(I_2 + I_1)\epsilon} \right\}$$

$$\times \left\{ I_1 I_2 D_t^2 + \frac{I_1 I_2}{I_1 + I_2}(\gamma_1 - \gamma_2) - \frac{I_1 I_2}{(I_2 + I_1)^2}(I_2 \gamma_1 + I_1 \gamma_2)^2 \frac{1}{(I_1 + I_2)\epsilon} \right\}$$

The first factor relates merely to a vibration in the joint. The second alone need be considered. It shows that the square of the period of oscillation is lengthened in the ratio

$$1+\frac{(I_2\gamma_1+I_1\gamma_2)^2}{(I_1+I_2)^2(\gamma_1-\gamma_2)\epsilon}$$

The value of ϵ will be experimentally determined by holding the upper part of the pendulum in a vise in the horizontal position and observing the depression of the lower part produced by adding a known load at a known distance.

In order to get a general conception of the mode of motion of the pendulum, suppress the first factor of the differential equation and write

$$I_1D_t^2\varphi_1+\left\{\frac{I_1}{I_1+I_2}(\gamma_1-\gamma_2)-\frac{I_1}{(I_1+I_2)^3}\frac{(I_2\gamma_1+I_1\gamma_2)^2}{\epsilon}\right\}\varphi_1=0$$

Subtract from this the first equation

$$I_1D_t^2\varphi_1=-\gamma_1\varphi_1-\epsilon(\varphi_1-\varphi_2)$$

and we have

$$\frac{I_1}{I_1+I_2}\left\{(\gamma_1-\gamma_2)-\frac{(I_2\gamma_1+I_1\gamma_2)^2}{(I_1+I_2)^2\epsilon}\right\}\varphi_1=(\epsilon+\gamma_1)\varphi_1-\epsilon\varphi_2$$

or

$$\varphi_2-\varphi_1=\left\{\frac{I_2\gamma_1+I_1\gamma_2}{(I_1+I_2)\epsilon}+\frac{I_1(I_2\gamma_1+I_1\gamma_2)^2}{(I_1+I_2)^3\epsilon^2}\right\}\varphi_1$$

This shows that, except for a vibratory motion which may be set up, the upper part will always be a little more displaced than the lower in a fixed ratio.

[Syllabus of Sixty Lectures on Logic]

MS 459: Summer 1883

LECTURE I.

Definition of Logic. A practical science. What is to be expected from the study of it.

LECTURE II.

Physiological and psychological basis of logic. Thinking, as cerebration, subject to the laws of nervous action. Five properties of nerves. 1. Irritability. 2. Conveyance of irritation. 3. Spreading of irritation. 4. Fatigue. 5. Habit. Hypothesis to account, by these five principles, for the direction of action toward an end. Illustration by cards.

Direction of discharge. Essential intricacy of connections. Inhibition. Spontaneous explosions. Visceral excitation.

Imagination. Not sensation. Action in an unforeseen emergency. Fancied action with inhibition of external volition.

Attention and self-direction.

LECTURE III.

The fixation of belief. Four methods: 1, Tenacity; 2, Authority; 3, Natural Inclination; 4, Scientific Investigation. Close connection between Logic and Ethics.

Nature of reality. Three grades of clearness of apprehension: 1, Familiarity; 2, Formal distinctness; 3, Apprehension of the practical or sensible issue. Rule for attaining the third grade. Illustrations.

Lecture IV.

Continuation of the same subject. Reality defined. Historical sketch of different forms of idealism.

Lecture V.

The flow of time and inference. The parts of inference: 1, Premise; 2, Conclusion; 3, Leading principle.

$$\begin{array}{c} \mathrm{P} \\ \therefore \mathrm{C}. \end{array}$$

The universe of discourse; definition of possibility. Nature of logical principles.

Propositions are either, 1, *Universal,* affirming leading principles, asserting non-existence, or 2, *Particular,* denying leading principles, asserting existence.

$$\begin{array}{ll} \mathrm{A} \prec \mathrm{B} & \mathrm{A} \overline{\prec} \mathrm{B} \\ \Pi_i u_i & \Sigma_i u_i. \end{array}$$

Distinction of categoricals and hypotheticals, unimportant. Syllogism and dialogism.

Three properties of the copula:

1$^{\text{st}}$, If $\mathrm{A} \prec \mathrm{B}$ and $\mathrm{B} \prec \mathrm{C}$, then $\mathrm{A} \prec \mathrm{C}$.
2$^{\text{nd}}$ $\mathrm{A} \prec \mathrm{A}$.
3$^{\text{rd}}$ If $\mathrm{A} \prec \mathrm{B}$ and $\mathrm{B} \prec \mathrm{A}$, then $\mathrm{A} = \mathrm{B}$.

Lecture VI.

Syllogistic, or the algebra of the copula.

$$a \prec (b \prec c) = b \prec (a \prec c).$$

Indirect syllogism.

Properties of the negative. Principles of contraposition, contradiction, and excluded middle. Syllogism depends on principle of contradiction, dialogism on principle of excluded middle.

Canons of syllogism. The middle term must be distributed once and only once. The order of particularity of the conclusion is the sum of those of the premises. Spurious propositions, of the second and higher orders of particularity. Anti-spurious propositions, of negative orders of particularity.

LECTURE VII.

Introduction to the Boolian algebra. Let a variable's coming to one fixed value, v, mean that a certain proposition is true, and its coming to another fixed value, f, mean that the same proposition is false. The principle of contradiction expressed by the fundamental equation

$$(x-\mathrm{f})(\mathrm{v}-x)=0.$$

The proposition 'if X then Y' is expressed by

$$(x-\mathrm{f})(\mathrm{v}-y)=0.$$

The negative of x is expressed by any function of x which equals f when $x=\mathrm{v}$ and equals v, when $x=\mathrm{f}$. The simplest such function is

$$\mathrm{f}+\mathrm{v}-x.$$

The logical aggregate of $x,y,z,$ is expressed most simply by

$$\mathrm{v}-(\mathrm{v}-\mathrm{f})\frac{(\mathrm{v}-x)(\mathrm{v}-y)(\mathrm{v}-z)}{(\mathrm{v}-\mathrm{f})(\mathrm{v}-\mathrm{f})(\mathrm{v}-\mathrm{f})}.$$

The logical compound of x, y, z, is most simply expressed by

$$\mathrm{f}+(\mathrm{v}-\mathrm{f})\frac{(x-\mathrm{f})(y-\mathrm{f})(z-\mathrm{f})}{(\mathrm{v}-\mathrm{f})(\mathrm{v}-\mathrm{f})(\mathrm{v}-\mathrm{f})}.$$

Different values for v and f.

The two terms of second intention, *being* and *nothing.* Definitions of logical addition and multiplication.

$$\left\{\begin{array}{l}\text{If } a \prec x \text{ and } b \prec x \text{ then } a+b \prec x.\\ \text{If } x \prec a \text{ and } x \prec b \text{ then } x \prec a \times b.\end{array}\right\}$$

$$\left\{\begin{array}{l}\text{If } a+b \prec x\text{, then } a \prec x \text{ and } b \prec x.\\ \text{If } x \prec a \times b\text{, then } x \prec a \text{ and } x \prec b.\end{array}\right\}$$

$$\left\{\begin{array}{l}(a+b)\times c \prec (a\times c)+(b\times c)\\ (a+c)\times(b+c)\prec(a\times b)+c\end{array}\right\}$$

Other formulae.

LECTURE VIII.

Solution of problems in non-relative deductive logic. Various methods. Rules of lecturer's method.

First process. Analysis of the premises.

$$\{(a+b) \prec c\} \prec (a \prec c) \times (b \prec c)$$
$$\{a \prec (b \times c)\} \prec (a \prec b) \times (a \prec c)$$
$$\{(a+b) \overline{\prec} c\} \prec (a \overline{\prec} c) + (b \overline{\prec} c)$$
$$\{a \overline{\prec} (b \times c)\} \prec (a \overline{\prec} b) + (a \overline{\prec} c).$$

Second process. Elimination of middle terms by means of the principles of syllogistic, combined with the following formulae of transposition:

$$(x \prec y + z) = (x \times \overline{y} \prec z)$$
$$(x \times y \prec z) = (x \prec \overline{y} + z).$$

Third process. Recomposition of results to find antecedents or consequents of any term.

Examples.

LECTURE IX.

Application of the Boolian algebra to the inversion of the order of integration and summation.

LECTURE X.

Introduction to the logic of relatives. Individuals. Nominalism and realism. The principle of individuation. Historical sketch of the controversy. Application to this question of the principle of continuity.

LECTURE XI.

The logic of relatives, continued. The proposition that something exists considered as a principle of logic. Universal and particular propositions represented by the two types

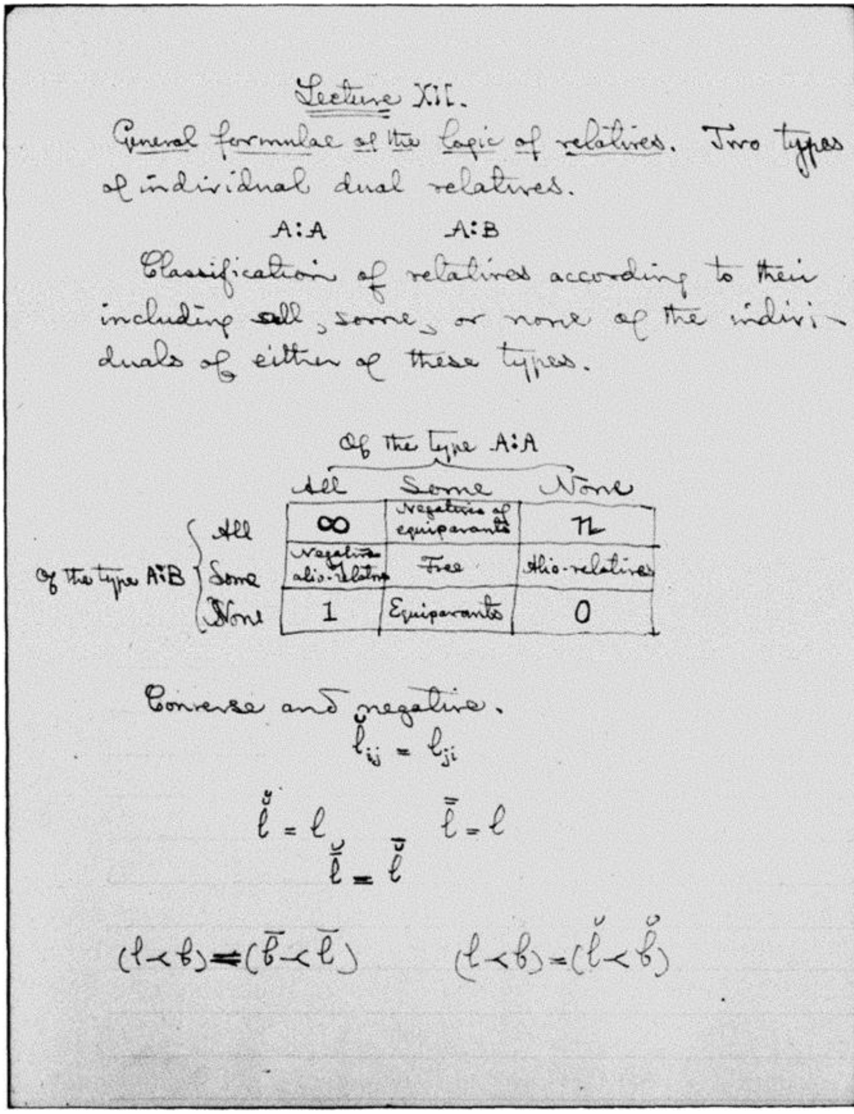
$$\Pi_i u_i \qquad \Sigma_i u_i.$$

Conception of a relative term. Limited universe in relative logic. The universal block,

A:A	A:B	A:C	etc.
B:A	B:B	B:C	etc.
C:A	C:B	C:C	etc.
etc.	etc.	etc.	etc.

Representation of a relative in the form

$$l = \Sigma_i \Sigma_j l_{ij} (\mathrm{I}:\mathrm{J}).$$



Lecture XII.

General formulae of the logic of relatives. Two types of individual dual relatives.

A:A A:B

Classification of relatives according to their including all, some, or none of the individuals of either of these types.

		Of the type A:A		
		All	Some	None
Of the type A:B	All	∞	Negatives of equiparants	n
	Some	Negative alio-relatives	Free	Alio-relatives
	None	1	Equiparants	0

Converse and negative.

$$\breve{l}_{ij} = l_{ji}$$

$$\breve{\breve{l}} = l \qquad \bar{\bar{l}} = l$$

$$\bar{\breve{l}} = \breve{\bar{l}}$$

$$(l \prec b) = (\bar{b} \prec \bar{l}) \qquad (l \prec b) = (\breve{l} \prec \breve{b})$$

Holograph page from MS 459
(Harvard Peirce Papers MS 745)

By means of the numerical coëfficients, l_{ij}, every problem in the logic of relatives is reduced to a problem in the Boolian algebra.

In dual relatives, there are four species of propositions:

$$\Pi_i \Pi_j u_{ij} \qquad \Sigma_i \Pi_j u_{ij} \qquad \Pi_j \Sigma_i u_{ij} \qquad \Sigma_i \Sigma_j u_{ij}.$$

We have, in general,

$$\begin{gathered} \Pi_i \Pi_j = \Pi_j \Pi_i \\ \Sigma_i \Sigma_j = \Sigma_j \Sigma_i \\ \Sigma_i \Pi_j \prec \Pi_j \Sigma_i . \end{gathered}$$

The last is the most important formula of logic. But we have

$$\begin{gathered} \Sigma_i \Pi_j u_i v_j = \Pi_j \Sigma_i u_i v_j \\ \Sigma_i \Pi_j (u_i + v_j) = \Pi_j \Sigma_i (u_i + v_j). \end{gathered}$$

Lecture XII.

General formulae of the logic of relatives. Two types of individual dual relatives.

A:A A:B

Classification of relatives according to their including all, some, or none of the individuals of either of these types.

		Of the type A : A		
		All	Some	None
Of the type A : B	All	∞	Negatives of equiparants	n
	Some	Negatives of alio-relatives	Free	Alio-relatives
	None	1	Equiparants	0

Converse and negative.

$$\breve{l}_{ij} = l_{ji}$$

$$\breve{\breve{l}} = l \qquad \bar{\bar{l}} = l$$

$$\breve{\bar{l}} = \bar{\breve{l}}$$

$$(l \prec b) = (\bar{b} \prec \bar{l}) \qquad (l \prec b) = (\breve{l} \prec \breve{b})$$

Relative addition and multiplication defined by the formulae

$$(lb)_{ij} = \Sigma_x l_{ix} b_{xj}$$

$$(l \dagger b)_{ij} = \Pi_x (l_{ix} + b_{xj})$$

Multiplication a *particular,* addition a *universal* mode of combination. The parts *undistributed* in both. Associative principle:

$$l \dagger (b \dagger s) = (l \dagger b) \dagger s$$
$$l(bs) = (lb)s.$$

Two important formulae:

$$l(b \dagger s) \prec lb \dagger s$$
$$(l \dagger b)s \prec l \dagger bs.$$

Formulae of transposition:

$$(l \prec b \dagger s) = (\breve{\bar{b}} \prec s \dagger \breve{\bar{l}}) = (\breve{\bar{s}} \prec \breve{\bar{l}} \dagger b)$$
$$(lb \prec s) = (b\breve{\bar{s}} \prec \breve{\bar{l}}) = (\breve{\bar{s}}l \prec \breve{\bar{b}}).$$

Distributive principle:

$$(l + b)s = ls + bs$$
$$s(l + b) = sl + sb$$
$$l,b \dagger s = (l \dagger s),(b \dagger s)$$
$$s \dagger l,b = (s \dagger l),(s \dagger b).$$

Negatives of combinations:

$$\overline{l + b} = \bar{l},\bar{b} \qquad \overline{l,b} = \bar{l} + \bar{b}$$
$$\overline{l \dagger b} = \bar{l}\,\bar{b} \qquad \overline{lb} = \bar{l} \dagger \bar{b}.$$

Converses of combinations:

$$\wideparen{l + b} = \breve{b} + \breve{l} \qquad \wideparen{l,b} = \breve{b},\breve{l}$$
$$\wideparen{l \dagger b} = \breve{b} \dagger \breve{l} \qquad \wideparen{lb} = \breve{b}\breve{l}.$$

Formulae relating to the relatives of second intention, $\infty, 0, 1, \mathfrak{n}$.

$$\begin{array}{ll}
0 \prec x & x \prec \infty \\
x + 0 = x & x, \infty = x \\
x + \infty = \infty & x,0 = 0. \\
& \\
x \dagger \infty = \infty & x0 = 0 \\
\infty \dagger x = \infty & 0x = 0. \\
& \\
x \dagger \mathfrak{n} = x & x1 = x \\
\mathfrak{n} \dagger x = x & 1x = x.
\end{array}$$

$$x + \bar{x} = \infty \qquad x , \bar{x} = 0.$$

$$1 \prec x \dagger \breve{\bar{x}} \qquad x\breve{\bar{x}} \prec \mathfrak{n}.$$

$$1 + \mathfrak{n} = \infty \qquad 1 , \mathfrak{n} = 0.$$

Method of using these formulae illustrated by examples.

LECTURE XIII.

Logical extension and comprehension. Germs of the doctrine among the Stoics, and in Porphyry. John of Salisbury. Durandus and Scotus. The Port Royal Logic. Kant and his followers.

Various ways in which it has been understood. Objections which have been made to it.

Proof that the doctrine should be symmetrical. Difficulty of rendering it perfectly so.

Application of the logic of relatives and in particular the principles of the last lecture solves every difficulty. Limited and unlimited universes of marks. New theory of names springing from this method of treatment. Names are *universal* or *particular*, like propositions. Perfected theory of categorical propositions.

LECTURE XIV.

General Method with the Logic of Relatives. The premises are first multiplied together and the operators Σ and Π are all brought to the left of all the operands by the formulae

$$\Pi_i u_i \cdot \Pi_j v_j = \Pi_i \Pi_j u_i v_j.$$
$$\Pi_i u_i \cdot \Sigma_j v_j = \Pi_i \Sigma_j u_i v_j.$$
$$\Sigma_i u_i \cdot \Sigma_j v_j = \Sigma_i \Sigma_j u_i v_j.$$

To these formulae may be added the following:

$$\Pi_i u_i + \Pi_j v_j = \Pi_i \Pi_j (u_i + v_j).$$
$$\Pi_i u_i + \Sigma_j v_j = \Pi_i \Sigma_j (u_i + v_j).$$
$$\Sigma_i u_i + \Sigma_j v_j = \Sigma_i \Sigma_j (u_i + v_j).$$

We may also often simplify the expression by means of the following:

$$\Pi_i \Pi_j u_i v_j = \Pi_k u_k v_k.$$
$$\Sigma_i \Sigma_j (u_i + v_j) = \Sigma_k (u_k + v_k).$$

It will then be found that the number to be eliminated occurs in the two forms u_i and $\bar{u}_j$, and supposing the operator relative to i to be to the left of that relative to j, it becomes necessary to substitute j for i.

The most perfect method of doing this, consists in finding a system of relatives

$$w,\ w',\ w'',\ w''',\ \text{etc.}$$

such that

$$\begin{array}{llll} \Pi_i\Sigma_j w_{ij} & \Pi_i\Sigma_j w'_{ij} & \Pi_i\Sigma_j w''_{ij} & \text{etc.} \\ \Pi_i\Pi_j\{\overline{w}_{ij} + (\overline{wn})_{ij}\} & \Pi_i\Pi_j\{\overline{w}'_{ij} + (\overline{w'n})_{ij}\} & & \text{etc.} \\ \Pi_i\Pi_j(w_{ij} + w'_{ij} + w''_{ij} + \text{etc.}) & & & \end{array}$$

Then, we have

$$\Sigma_j u_j = u_i + (wu)_i + (w'u)_i + (w''u)_i + (w'''u)_i + \text{etc.}$$
$$\Pi_j u_j = u_j \times (wu)_j \times (w'u)_j \times (w''u)_j \times (w'''u)_j \times \text{etc.}$$

After this, we shall use the process of elimination of the Boolian algebra.

Required, for instance, to eliminate s from the premises

$$\Sigma_h\Pi_i\Sigma_j\Pi_k(\alpha_{hik} + s_{jk}l_{ji})$$
$$\Sigma_u\Sigma_v\Pi_x\Pi_y(\epsilon_{uyx} + \bar{s}_{yv}b_{vx}).$$

Multiplying, we have

$$\Sigma_u\Sigma_v\Pi_x\Pi_y\Sigma_h\Pi_i\Sigma_j\Pi_k(\alpha_{hik} + s_{jk}l_{ji})(\epsilon_{uyx} + \bar{s}_{yv}b_{vx}).$$

This is the same as

$$\Sigma_u\Sigma_v\Pi_x\Pi_y\Sigma_j\Pi_k(\alpha_{uxk} + \mathrm{s}_{jk}\mathrm{l}_{jx})(\epsilon_{uyx} + \bar{s}_{yv}b_{vx}).$$

LECTURE XV.

Logic of arithmetic; its true nature made manifest by the application of the logic of relatives. The reason of the important part played by relations of correspondence in general and by counting in particular, made plain from the principles of the last lecture. Conception of quantity, in general. Let g be 'as great as'. Then,

$$1 \prec g$$

$$gg \prec g.$$

In the universe of quantity

$$g,\breve{g} \prec 1.$$

In linear quantity

$$\infty \prec g + \breve{g}.$$

In continuous quantity

$$g \dagger g \prec g;$$

in discrete quantity, we have the conception of the *next.*

$$l,(\bar{l} \dagger \bar{l}).$$

Conception of finite and super-infinite quantity. Quantity may be *limited,* in the sense of having an absolute maximum without being finite. The logic of relatives first shows us this, which we are then able to make intuitively evident. Unlimited quantity is defined by the formula

$$g \dagger 0 \prec 0.$$

Finite quantity, how distinguished from the super-infinite. Infinite quantity is that which is at once unlimited and finite. Mathematical induction generally fallacious; when valid.

Counting is a relative of correspondence. Defining formulae of such a relative

$$\breve{c}c \prec 1 \qquad c\breve{c} \prec 1.$$

If c be a number used in counting, an s an object of the lot counted; we have

$$s \prec \breve{c}$$
$$\breve{g}c \prec c.$$

If the lot counted is a finite and limited lot there is a maximum number reached in the counting. Calling this q, we have

$$q \prec c$$
$$\bar{g}q \prec \bar{c}.$$

Nature of mathematical reasoning shown by the application of the principles of the last lecture. Syllogisms of transposed quantity generally fallacious; when valid.

Lecture XVI.

Logic of the differential calculus. Application of the rule for attaining clear ideas to the conception of continuous quantity. The doctrine of limits. The value at the limit is the sum of an infinite convergent series, and this was explained in the last lecture. The conception of continuity involves the principles of the logic of probability. Evaluation of indeterminate expressions. Cases of ambiguity. The solution sometimes depends on the number of dimensions. Real indeterminacy in the path of a moving body beyond a certain point shows that the time occupied in reaching that point is infinite.

LECTURE XVII.

Continuity continued. The sophism of Achilles and the tortoise and its congeners. The part played by these sophisms in philosophy. Herbart and Hegel. Analysis by means of the logic of relatives. The principle of continuity.

LECTURE XVIII.

The so-called absurd quantities. Application of the rule for attaining clear ideas to the question how far such quantities are real and how far fictitious. The square-root of the negative. Multiple algebra. Sketch of the history of these conceptions. The non-Euclidean geometry in its two aspects. Every system of finite or super-infinite quantity reducible to a system of simply infinite quantity. The doctrine of infinitesimals. Intuitional representation by means of a spiral. In regard to the *zero* and the *infinite* orders we have still to distinguish the order of the orders, and so on *ad infinitum.* Nothing false or really absurd about infinitesimals.

LECTURE XIX.

Enumeration and logical examination of the methods of geometry.

Nature of geometrical axioms.

LECTURE XX.

Logic of Mechanics. Methods of establishing the first principles. The chief principles of mechanics. Methods of mechanics and of geometry compared. Imaginaries in mechanics.

LECTURE XXI.

Traditional Logic. General sketch of its history. The doctrine of the *predicables.*

The Categories. Trendelenburg's view. Kantian categories. Other lists, including the lecturer's.

LECTURE XXII.

The *traditional syllogistic* set forth. Aristotle. Ancient development of hypotheticals. Petrus Hispanus. Sketch of the doctrine of modals. Lambert's *Neues Organon.* De Morgan. Criticism in the light of the logic of relatives.

LECTURE XXIII.

The *Posterior Analytics.* Principal contents of the books.

LECTURE XXIV.

The *Topics.* Aristotle. Later developments. The *ars magna* of Raymund Lully. Possible future of this part of logic.

LECTURE XXV.

Fallacies. The traditional list. Different classifications.

Utility of the doctrine. But is there, after all, any fallacious thinking? Harmfulness of logic, too narrowly studied. Drink deep,—etc.

LECTURE XXVI.

The *parva logicalia.* Suppositiones. Distributio. Exponibilia. Insolubilia. Obligationes.

The formalitates of Scotus. Terministic views. Paulus Venetus and his *Sophismata aurea.*

Traditional scraps of logic. Ass of Buridanus. Argumentum ad hominem. *Exceptio probat regulam.*

LECTURE XXVII.

Introduction to the *theory of probabilities.* Crude conception of probability as a reduced mode of existence. Conception of probability as the measure of just belief unduly emphasizes a secondary circumstance. Application of the rule for attaining clear ideas. Probability attaches primarily to modes of inference and denotes the frequency with which they carry truth with them. Probability of an event an abridged expression. Probability a matter of fact. The feeling of probability.

LECTURE XXVIII.

Relative numbers, in general. Rules for combining them. Independent relatives. Probabilities a species of relative numbers.

Connection with logical algebra, as especially with the Boolian calculus. Effect of assigning different values to f and v. Probabilities and odds. The feeling of probability and the psychophysical law of Fechner.

LECTURE XXIX.

Elementary problems in probabilities. The binomial development.

Boole's method in probabilities reformed, with examples. Extension of this method to problems involving other relative numbers.

LECTURE XXX.

The law of high numbers. Important consequences of certain numbers being large in different branches of science; such as political economy, theory of gases, physiology, doctrine of natural selection, and wherever there is a tendency toward an end.

LECTURE XXXI.

The law of error. Natural classes. Mr. Galton's methods.

The principles of least squares. Ferrero's theory.

LECTURE XXXII.

Examples in *least squares.*

LECTURES XXXIII–XXXVI.

Equations of finite differences, treated mainly as in Boole.

A function in finite differences has not generally a differential coëfficient.

The algebraic properties of the symbol E.

Linear equations, simultaneous equations, partial equations, applied to problems in probability.

Equations of higher degrees.

The problem of the duration of play, applied to the theory of natural selection and to philosophy.

LECTURES XXXVII to XLVI.

The theory of *Induction*, treated as in the lecturer's essay in the *Studies in Logic.*

LECTURE XLVII.

Pure *induction* illustrated by chemical theories. Mendelejeff's law.

LECTURE XLVIII.

Hypothetic reasoning illustrated by the attempts to discover the identity of Junius.

LECTURE XLIX.

The *à priori* element of science illustrated by Galileo's dialogue, and other dynamical speculations.

LECTURES L to LV.

The history of *Astronomy.* Ptolemaic astronomy, expounded. Copernicus. Tycho Brahe. History of Kepler's work. The Newtonian discovery and its consequences. Hegel a pretended rival of Newton.

LECTURES LVI–LVIII.

The *kinetical theory of gases* and speculations on the constitution of matter. Stallo's objections.

LECTURE LIX.

Logical principles of political economy.

LECTURE LX.

Anthropomorphic science. Judgment of men. Physiognomy. Art. Natural theology. One-sidedness of physical science.

[Lecture on Propositions]

MS 462: Summer–Fall 1883

At this point you will do well to read the Note by Mrs. Franklin on the Constitution of the Universe. (*Studies in Logic* p. 61.)

The main points are these. Every proposition must be either true or false.—This is the principle of excluded middle which is represented by the fundamental quadratic of logical algebra

$$(x - \mathrm{f})(\mathrm{v} - x) = 0$$

and upon which, therefore, logical algebra as far as it has been developed in the previous lectures has been represented as turning. I have already intimated that this gives a very poor kind of logic,—one which is true indeed but which goes very little way. Because in all mathematical reasoning we do not merely distinguish the true & the false, lumping all that is not true in one indistinguishable mass as though it were equally valueless, but we recognize that though a proposition be false it may have a certain value if it is not *very* false, —and indeed wherever continuity comes in, and here alone the mathematical logic is fully developed, no real proposition is exactly true,—so that the question is *how* false a proposition is. This remark I repeat, in season and out, that there may be no mistake about my view of the paucity of the old logic which however is absolutely true as far as it pretends to go.

To return then to the principle of Excluded Middle, or as we may write it

$$\mathrm{A} + \overline{\mathrm{A}}.$$

Let x be any general term, and i be *"this"*,—any indicated subject. We have

$$x_i + \bar{x}_i$$

this is x or this is not x. And for the universe of i

$$\Sigma_i x_i + \Pi_i \bar{x}_i$$

some i is x or every i is non-x.

We also have

$$\Sigma_i \bar{x}_i + \Pi_i x_i$$

some i is non-x or else every i is x.

Thus in reference to the single term x; there are four conditions in which the universe can be viz.

$$\begin{gathered}(\Sigma_i x_i)(\Sigma_i \bar{x}_i)\\ (\Sigma_i x_i)(\Pi_i x_i)\\ (\Pi_i \bar{x}_i)(\Sigma_i \bar{x}_i)\\ (\Pi_i \bar{x}_i)(\Pi_i x_i)\end{gathered}$$

In the first case among the universe of i's there are x's and there are non-x's. In the second case, there are x's, and all there are are x's.

In the third case, there are non-x's and all there are are non-x's.

In the fourth case, all there are are non-x's and all there are are x's; that is to say, there are no i's at all; the universe is void.

Here are these four possible states of things, which by way of abbreviation we may write

$$\Sigma\Sigma \qquad \Sigma\Pi \qquad \Pi\Sigma \qquad \Pi\Pi.$$

Now our opinion may be such as to admit any of these as possible and to exclude the rest as impossible. Since there are four and each may be either admitted as possible or excluded, independently of the rest, 2^4 or 16 will be the number of our possible opinions. These will be

$$\begin{gathered}\Sigma\Sigma + \Sigma\Pi + \Pi\Sigma + \Pi\Pi\\ \Sigma\Sigma + \Sigma\Pi + \Pi\Sigma\\ \Sigma\Sigma + \Sigma\Pi + \Pi\Pi\\ \Sigma\Sigma + \Pi\Sigma + \Pi\Pi\\ \Sigma\Pi + \Pi\Sigma + \Pi\Pi\\ \Sigma\Sigma + \Sigma\Pi\\ \Sigma\Sigma + \Pi\Sigma\\ \Sigma\Sigma + \Pi\Pi\\ \Sigma\Pi + \Pi\Sigma\\ \Sigma\Pi + \Pi\Pi\\ \Pi\Sigma + \Pi\Pi\end{gathered}$$

$$\Sigma\Sigma$$
$$\Sigma\Pi$$
$$\Pi\Sigma$$
$$\Pi\Pi$$
$$0$$

The last opinion symbolized by *zero* is the absurd opinion that holds everything to be false. The first is the empty opinion that something is possible. These opinions might be excluded from the number of opinions but they are included in the system of enumeration we have adopted.

What are the other fourteen opinions? They are

1	1st	Some i exists.
10	2nd	If there be any i, some i is x.
11	3rd	If there be any i, some i is non-x.
100	4th	All the i's are alike in respect to being x's or non-x's.
101	5th	Some i is x.
110	6th	Some i is non-x.
111	7th	If there be any i, the i's differ in respect to being x's and non-x's.
1000	8th	There are i's, and they are all alike in respect to being x's and non-x's.
1001	9th	Whatever i's there are are x's.
1010	10th	Whatever i's there are, are non-x's.
1011	11th	Some i's are x's and some i's are non-x's.
1100	12th	There are i's and all are x's.
1101	13th	There are i's and all are non-x's.
1110	14th	There are no i's.

[Lecture on Types of Propositions]

MS 463: Summer–Fall 1883

Formal logic, so far as it has thus far been expounded, has been represented as all turning on the principle of excluded middle. This principle it is which is embodied in the fundamental quadratic equation of the Boolian algebra

$$(x - \mathrm{f})(\mathrm{v} - x) = 0$$

This it is which at once cuts down the algebra of logic to an algebra of two quantities and which makes its poverty on the mathematical or purely formal side,—its poverty as viewed from within.

This is the principle that "Being only is and nothing is altogether not," that thinks it sufficient to lay hold of the truth & cast away all error as being equally and absolutely valueless. But this is not so. The principle of excluded middle says nothing which is not true. But a logic, erected on that principle which cares not how little a statement errs so long as it is not exact truth, must be characterized by great poverty, in that it is inadequate to explaining any reasoning about quantity, and inasmuch as it adopts a point of view which must make almost every one of our judgments,—all those that refer to continuity —false.

In the *Studies in Logic,* p. 61, you will find a note of Mrs. Franklin entitled "On the Constitution of the Universe." This is a systematic development of the principle of excluded middle. It should be attentively read.

If we consider any single general term, as *black,* each single thing in the universe may be either *black* or non-black. Or the universe consists of two parts, the black things and the non-black things. But either of these parts may be wanting. So that there are four possible states of the universe with reference to blackness

1st Some things are black and some not.
2nd All things are black.
3rd All things are non-black.
4th Neither part of the universe exists, that is the universe is altogether void.

Our *opinion* may be such as to admit the possibility of any of these states of things and to exclude the rest. Thus, there will be 2^4 or 16 possible opinions on the subject.

They are as follows:—

The perfectly empty opinion that any one of these states of things may be the actual one.

The absurd opinion contrary to this that none of these states of things is the actual one.

Four opinions each excluding only *one* of the 4 states of things; together with 4 other opinions contrary to these each admitting only one of the four states of things.

These are as follows:—

Something exists
Nothing exists

If anything exists a black thing exists
Something exists but nothing black exists

If anything exists a non-black thing exists
Something exists but everything is black

Whatever things may exist are all alike in respect to being black or not.
Black things and non-black things both exist.

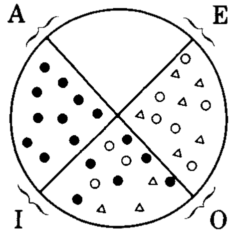
Finally we have 6 opinions which each admit the possibility of two states of things and exclude the possibility of two. These are

Something black exists
Nothing black exists

Something non-black exists
Nothing non-black exists

If anything exists, both black and non-black things exist
Something exists but it is all alike in respect to blackness or non-blackness

Let us represent the four possible states of the universe by the four quadrants of this circle



In the lower quadrant some things are black & some not. In the left hand one there are only black things in the right hand one only non-black things. In the upper quadrant nothing at all.

Let us denote by A the opinion that whatever there may be is black. This admits the possibility of the upper or left hand quadrant & excludes the lower and right hand ones.

By E, the opinion that whatever there may be is non-black. This admits the upper and right hand quadrants and excludes the lower and left hand ones.

By I the opinion that something black exists. This admits the possibility of the states of things represented by the lower and left hand quadrants & excludes the others.

By O, the opinion that something non-black exists; which admits the lower and right hand & excludes the upper and left hand.

Then our 16 possible opinions will be

Something exists $I + O$
Nothing exists AE
If anything exists, a black thing exists $A + I$
Something exists, but nothing black EO
If anything exists, a non-black thing exists $E + O$
Something exists, but only black things AI
Whatever things exist are all alike in respect to being black or not
$A + E$
Black things and non-black things both exist IO
If anything exists both black and non-black things exist
$AE + IO = (A + I)(E + O)$
Something exists but all is alike in being black or not
$(A + E)(I + O) = AI + EO$

In case two terms instead of one are admitted, Mrs. Franklin shows that there are 16 possible states of things and 65,536 different possible opinions, which can all be expressed by De Morgan's eight forms of propositions referred to by Mr. Mitchell and reducible to 6 different forms.

These six types of propositions consist of three pairs. In each pair one proposition asserts that something exists, the other that something does not exist. And of these 3 pairs, two are symmetrical, that is the two terms are similarly related to one another, in the third they are not.

In this third type it is either asserted or denied that everything denoted by one term embraces everything denoted by the other, as

All griffins breathe fire
Some griffins do not breathe fire.

Of the other two pairs, one asserts or denies, that two terms denote anything in common

Some crows are white
No crows are white.

The other asserts that two terms do or do not make up the universe together as

Every man is either a knave or a fool
Some men are neither knaves nor fools.

The theory of combinations of two terms is highly important. But the whole thing reduces to this.

There is an essential difference between asserting that something exists & saying that something doesn't exist. That is between propositions of the types

$$\Sigma x \qquad \Pi x$$

If the x is itself a sum or product we have 6 types

$$\begin{array}{cccc} \Sigma_i \Sigma_j & \Sigma_i \Pi_j & \Sigma_j \Pi_i & \\ & \Pi_j \Sigma_i & \Pi_i \Sigma_j & \Pi_i \Pi_j \end{array}$$

With three successive operators we have 20 types on the same principle.

But now I wish to point out that we have a superfluous element in our algebra. Let b be black. Then

$$\Sigma b$$

asserts that something is black. To deny this we may at pleasure use either of two forms

$$\overline{\Sigma b} = \Pi \bar{b}$$

That is we may directly apply the sign of negation to the whole or we may say whatever there is is non-black.

We have therefore three distinct signs Σ Π and the line denoting negation, when only two are requisite. But we cannot get along without a sign of negation and we have no reason for choosing one rather than the other of the other two signs Σ Π.

Some crows are white is written $\Sigma cw = \overline{\Pi(\bar{c} + \bar{w})}$
No crows are white is written $\overline{\Sigma cw} = \Pi(\bar{c} + \bar{w})$
Every man is either a knave or a fool is written $\Pi(k + f) = \overline{\Sigma \bar{k} \bar{f}}$
Some man is neither a fool nor a knave $\overline{\Pi(k + f)} = \Sigma \bar{k} \bar{f}$

Each proposition is written two ways.

I have fully shown in the *American Journal of Mathematics* Vol. iii "On the Algebra of Logic" that if in place of the symmetrical operators Σ Π we put one unsymmetrical operator, we have no need of a superfluity of signs.

We have already made use of the form

$$b_i$$

to mean that i is black

$$l_{ij}$$

to mean that i is a lover of j.

Taking a hint from this let us write any letter whatever subscript to another to show the circumstances under which the other is applicable. Thus

$$b_c$$

shall mean every crow is black.

0_c shall mean every crow is non-existent or no crow exists.
$(0_b)_c$ that in the case of a crow blackness is non-existent or no crow is black.
$0_{(b_c)}$ means that every crow's being black is a non-existent case or some crow is not black.
$0_{((0_b)_c)}$ means that some crow is black.

We have then

$$b_c = \Pi_i(\bar{c}_i + b_i)$$
$$(0_b)_c = \Pi_i(\bar{c}_i + \bar{b}_i)$$

This is the same as writing

$$0_b = \bar{b}$$

Now in the formula $b_c = \Pi_i(\bar{c}_i + b_i)$ put $b = 0$ and we have

$$0_c = \Pi_i \bar{c}_i$$

Now if $c = \Sigma_i c_i$ (or $c = c_1 + c_2 + c_3 + c_4$, the logical sum of the individual c's) we necessarily have

$$\bar{c} = \bar{c}_1 \times \bar{c}_2 \times \bar{c}_3 \times \bar{c}_4$$

Or by the rule

$$b_c = \Pi_i(\bar{c}_i + b_i)$$

we have

$$(a_b)_c = \Pi_i(\bar{c}_i + (a_b)_i) = \Pi_i(\bar{c}_i + \Pi_j(\bar{b}_j + a_j)_i)$$

Put $a = 0$

$$(0_b)_c = \Pi_i\{\bar{c}_i + (\Pi_j \bar{b}_j)_i\}$$

On the contrary, we have

$$a_{(b_c)} = \Pi_i(a_i + \overline{(b_c)}_i)$$

$$0_{(b_c)} = \Pi_i \overline{(b_c)}_i = \overline{(b_c)} = \Sigma_i(c_i \times \bar{b}_i)$$

Again if we put

l for lover
m for man
w for woman

we shall naturally write

$$l_{m,w} = \Pi_i \Pi_j(\overline{m}_i + \overline{w}_j + l_{ij})$$

or every man loves every woman.

Then,

$(0_l)_{m,w}$ will be every man is a non-lover of every woman
$(0_{(l_m)})_w$ will be every woman is unloved by some man
$\{0_{((0_l)_m)}\}_w$ will be every woman is loved by some man
$0_{\{0_{((0_l)_m)}\}_w}$ some man does not love every woman.

These examples sufficiently prove that an algebra of logic can be constructed without any superfluous element. At the same time they show that an algebra so constructed would be a very unwieldy affair.

Still it is important to remember that there is such an algebra and questions as to the true nature of inferences may often be solved by considering them from the point of view of such an algebra.

All the inferences which we have thus far considered are but complications of this one

$$(a+b)(\bar{b}+c)=(a+c)(a+b+\bar{c})(\bar{a}+\bar{b}+c)$$

Or if we write $\bar{a}$ for a

$$(\bar{a}+b)(\bar{b}+c)=(\bar{a}+c)(\bar{a}+\bar{c}+b)(\bar{b}+a+c)$$

This in the new notation is partly said in writing

$$\left\{ (c_a)_{(b_a)} \quad (c_b) \right\}$$

This in fact says that if c follows from b and b from a then c follows from a.

We see that no 0 enters into this formula and consequently negation has really nothing to do with the inference but only appears to do so owing to the peculiar system of logic which we have been using.

Thus we see that it is a false point of view from which the principle of excluded middle appears to be essential to all inference.

When I say

> every man loves a woman and every woman possesses long hair,

I am able to draw a conclusion which eliminates the term *woman*, namely the conclusion that

> every man loves a possessor of long hair.

The reason I can do this obviously is that *every* woman is spoken of in one of the premisses.

Let l be lover of,

$$\Pi_m \Sigma_w l_{mw}$$

may mean that every man loves a woman.

Let p be possessor of

$$\Pi_w \Sigma_h p_{wh}$$

may mean that every woman possesses some long hair. Multiply the two together and we have

$$(l_{11}+l_{12}+l_{13}+\text{etc.})(l_{21}+l_{22}+l_{23}+\text{etc.})$$
$$\times(p_{11}+p_{12}+p_{13}+\text{etc.})(p_{21}+p_{22}+p_{23}+\text{etc.})$$

This clearly contains as a factor

$$\begin{aligned}&(l_{11}p_{11} + l_{11}p_{12} + l_{11}p_{13} + \text{etc.})\\ \times\,&(l_{12}p_{21} + l_{12}p_{22} + l_{12}p_{23} + \text{etc.})\\ \times\,&(l_{13}p_{31} + l_{13}p_{32} + l_{13}p_{33} + \text{etc.})\end{aligned}$$

and so contains

$$\begin{aligned}&(l_{11}p_{11} + l_{12}p_{21} + l_{13}p_{31} + \text{etc.})\\ \times\,&(l_{11}p_{12} + l_{12}p_{22} + l_{13}p_{32} + \text{etc.})\end{aligned}$$

and so we come to

$$\Pi_m \Sigma_h (lp)_{mh}$$

where by $(lp)_{mh}$ we mean

$$\Sigma_w (l_{mw} \times p_{wh}).$$

In fact the mode of inference is just the same as that

$$(m_1 + m_2 + m_3 + \text{etc.})b_1 b_2 b_3 \text{ etc.}$$

contains as a factor

$$m_1 b_1 + m_2 b_2 + m_3 b_3 + \text{etc.}$$

The secret of it is simply that one of the factors is a product according to a universe (no matter what universe). That is one of the premises speaks of *every* one of a class.

Now according to the Boolian algebra you do not speak of the whole of a class unless you use the negative. Thus if you write

$$\Pi b$$

everything is black, you speak of the whole universe but you do not say there are no black things outside that universe. But when you write

$$\Pi \bar{b}$$

[. . .]

[From a Lecture on the Logic of Relatives]

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[. . .]

$$\begin{aligned}&\Pi_i\Pi_j(a_i + p_{ij} + \beta_j)\Sigma_i\Pi_j c_i(p_{ij} + \bar{\beta}_j)\\&\Sigma_i\Pi_j(a_i + p_{ij} + \beta_j)c_i(p_{ij} + \bar{\beta}_j)\\&\Sigma_i\Pi_j c_i(a_i + p_{ij})\\&= \Sigma_i c_i a_i + \Sigma_i\Pi_j c_i p_{ij}\end{aligned}$$

Some c is a or else some c possesses every character.

Third syllogism

$$\begin{aligned}&\Sigma_i\Pi_j a_i(p_{ij} + \beta_j)\Sigma_i\Pi_j c_i(p_{ij} + \bar{\beta}_j)\\&\Sigma_i\Sigma_{i'}\Pi_j a_i c_{i'}(p_{ij} + p_{i'j})\end{aligned}$$

There are an A and a C so related that each possesses every mark that the other does not.

Fourth syllogism

$$\begin{aligned}&\Pi_i\Pi_j(a_i + p_{ij} + \beta_j)\Sigma_j\Pi_i(c_i + p_{ij})\bar{\beta}_j\\&\Sigma_j\Pi_i(a_i c_i + p_{ij})\end{aligned}$$

There is a character which is possessed by every object except such as is at once an A and a C.

Variant

$$\begin{aligned}&\Pi_i\Pi_j(a_i + \bar{p}_{ij} + \beta_j)\Sigma_j\Pi_i(c_i + p_{ij})\bar{\beta}_j\\&\Sigma_j\Pi_i(a_i + \bar{p}_{ij})(c_i + p_{ij})\\&\Sigma_j\Pi_i(a_i + c_i)(a_i + \bar{c}_i + \bar{p}_{ij})(\bar{a}_i + c_i + p_{ij})\end{aligned}$$

Everything is either an A or a C and there is a certain character which every A not C possesses while every C not A wants it.

Fifth syllogism

$$\Sigma_i \Pi_j a_i(p_{ij} + \beta_j)\, \Sigma_j \Pi_i (c_i + p_{ij})\bar{\beta}_j$$

The conclusion we may write thus:—

$$\begin{aligned}
&\Sigma_i \Sigma_j \Pi_{i'}\, a_i(p_{ij} + \beta_j)(c_{i'} + p_{i'j})\bar{\beta}_j \\
&= \Sigma_i \Sigma_j \Pi_{i'}\, a_i p_{ij}(c_{i'} + p_{i'j}) \\
&= \Sigma_i \Pi^j_{i'} \Sigma_j (a_i p_{ij} c_{i'} + \mathrm{a}_i p_{ij} \breve{p}_{i'j}) \\
&= \Sigma_i \Pi^j_{i'} a_i (p\infty)_i c_{i'} + a_i (p\breve{p})_{ii'}
\end{aligned}$$

That is, an object i can be found such that no matter what lot of objects you take (so long as this lot is not greater in number than the marks in the universe) it will be true that i is of the class A and possesses some mark and also that this lot of objects are either all C's or else all possess some character in common with i.

The above examples in the logic of relatives (the logic of dual and plural characters) serve merely as an introduction to the subject.

The subject is difficult but it is most important. It is the only road by which reasoning is destined to be explained and facilitated. I therefore beg you to bend your minds to the subject.

Let us now seek to obtain a general method of working. The premises being given; multiply them together, to show they are all true at once. And distinguish the $i\,j$ etc. of the different premises by accents.

You can now bring all the Π's and Σ's to the left. For by the distributive process

$$\begin{gathered}
\Sigma_i u_i \cdot \Pi_j v_j = (u_1 + u_2 + u_3 \ldots) v^1 v^2 v^3 \\
\Sigma_i \Pi_j (u_i \cdot v_j) = u_1 v^1 v^2 v^3 + u_2 v^1 v^2 v^3 + \text{etc.}
\end{gathered}$$

We wish to eliminate certain letters. For that their subscripts must be made the same, and for that again the corresponding Σ and Π must be brought together.

We have therefore to consider the transpositions of the operational signs. We have in general

$$\begin{gathered}
\Pi_i \Pi_j = \Pi_j \Pi_i \\
\Sigma_i \Sigma_j = \Sigma_j \Sigma_i \\
\Sigma_i \Pi_j = \Pi^i_j \Sigma_i \\
\Pi_j \Sigma_i = \Sigma^j_i \Pi_j
\end{gathered}$$

Let me remind you of the meaning of these last two formulae.

If

$$\Pi_i \Sigma_j u_{ij} = (u_{11} + u_{12})(u_{21} + u_{22})(u_{31} + u_{32})$$

means every city has a celebration on some year or other of this century,

$$\begin{aligned}\Sigma_j^i \Pi_i u_{ij} = u_{11}u_{21}u_{31} &+ u_{11}u_{21}u_{32} \\ + u_{11}u_{22}u_{31} &+ u_{11}u_{22}u_{32} \\ + u_{12}u_{21}u_{31} &+ u_{12}u_{21}u_{32} \\ + u_{12}u_{22}u_{31} &+ u_{12}u_{22}u_{32}\end{aligned}$$

means that in some collection of years, not more in number than the number of cities, every city has a celebration.

So

$$\Sigma_j \Pi_i u_{ij} = u_{11}u_{21}u_{31} + u_{12}u_{22}u_{32}$$

means that in some one year all the cities celebrate while its equivalent

$$\begin{aligned}\Pi_i^j \Sigma_i u_{ij} = (u_{11} + u_{12}) \times (u_{11} + u_{22}) \times (u_{11} + u_{32}) \\ \times (u_{21} + u_{12}) \times (u_{21} + u_{22}) \times (u_{21} + u_{32}) \\ \times (u_{31} + u_{12}) \times (u_{31} + u_{22}) \times (u_{31} + u_{32})\end{aligned}$$

is read that in every collection of cities, not larger in number than that of the years, there is a celebration in one year or another.

Thus we see that

Σ_i directs us to successively substitute the different particular i's throughout the operand and add the results.

Σ_i^j directs us to successively substitute for i in j different places the particular i's in every possible combination and add the results.

While these are the general formulae we also have—

$$\Sigma_i \Pi_j (u_i v_j) = \Pi_j \Sigma_i (u_i v_j)$$

For

$$\begin{aligned}\Sigma_i \Pi_j (u_i v_j) &= \Sigma_i u_i (\Pi_j v_j) \\ &= \Sigma_i u_i \cdot \Pi_j v_j \\ &= \Pi_j \{\Sigma_i u_i \cdot v_j\} \\ &= \Pi_j \Sigma_i (u_i \cdot v_j)\end{aligned}$$

And in a similar way

$$\Sigma_i \Pi_j (u_i + v_j) = \Pi_j \Sigma_i (u_i + v_j)$$

Having in this way got those operational symbols together which needed to be brought together, we have to make their subscripts the same.

We shall, for the present, suppose that the two universes are the same, though one letter is distinguished from the other by an accent.

We have then in the first place

$$\Pi_i \Pi_{i'} u_i v_{i'} = u_1 u_2 u_3 \ldots v_1 v_2 v_3 \ldots$$
$$\Pi_i u_i v_i = u_1 v_1 \cdot u_2 v_2 \cdot u_3 v_3 \ldots$$

and so

$$\Sigma_i \Sigma_{i'} (u_i + v_{i'}) = \Sigma_i (u_i + v_i).$$

In the case of $\Sigma_i \Pi_{i'} u_i v_{i'}$ we have, *as a factor,* $\Sigma_i u_i v_i$.

We may therefore write

$$\Sigma_i \Pi_{i'} u_i v_{i'} = \Sigma_i u_i v_i \{\Pi_i (\bar{u}_i + \bar{v}_i) + \Pi_i v_i\}$$

The negative of this gives

$$\Sigma_i \Pi_{i'} (u_i + v_{i'}) = \Pi_{i'} (u_i + v_i) + \Sigma_i (u_i v_i) \Sigma_i v_i$$

But when we come to the general form we find that we have to introduce a different conception, from any that has as yet appeared. Thus

$$\begin{aligned}\Pi_i \Pi_{i'} u_{ii'} = u_{11} &\cdot u_{12} \cdot u_{13}\\ \cdot u_{21} &\cdot u_{22} \cdot u_{23}\\ \cdot u_{31} &\cdot u_{32} \cdot u_{33}\end{aligned}$$

To deal with this we introduce the term 0 and we will write

$$\Pi_j (l_{ij} + 0_{jk}) = (l \dagger 0)_{ik}$$

meaning that i is a lover of everything coexistent with k. That this is the meaning is easily seen for if there be any thing coexistent with k of which i is not said to be the lover let j be this thing; then $l_{ij} = 0$. But $0_{jk} = 0$ hence the factor vanishes and the whole expression is absurd. We have then

$$\Pi_i \Pi_{i'} u_{ii'} = \Pi_i (u \dagger 0)_{ii}$$

In like manner

$$\begin{aligned}\Sigma_i \Sigma_{i'} u_{ii'} = u_{11} &+ u_{12} + u_{13}\\ + u_{21} &+ u_{22} + u_{23}\\ + u_{31} &+ u_{32} + u_{33}\end{aligned}$$

Write

$$(u\infty)_{ik} = \Sigma_j(u_{ij}\infty_{jk})$$

where

$$\Pi_j \Pi_k \infty_{jk}$$

Then

$$\Sigma_i\Sigma_{i'}u_{ii'} = \Sigma_i(u\infty)_{ii}$$

We have next to consider

$$\begin{aligned}\Sigma_i\Pi_{i'}u_{ii'} &= u_{11}u_{12}u_{13}\\ &+ u_{21}u_{22}u_{23}\\ &+ u_{31}u_{32}u_{33}\\ &= \Sigma_i(u \dagger 0)_{ii}\end{aligned}$$

So

$$\Pi_i\Sigma_{i'}u_{ii'} = \Pi_i(u\infty)_{ii}$$

But it will be found that these formulae are often unsatisfactory and do little for us.

We have

$$\begin{aligned}\infty &= 1 + \mathfrak{n}\\ 0 &= 1,\mathfrak{n}.\end{aligned}$$

So that we may write

$$\begin{aligned}\Pi_i\Pi_{i'}u_{ii'} &= \Pi_i u_{ii}\cdot\Pi_i(u \dagger 1)_{ii}\\ \Sigma_i\Sigma_{i'}u_{ii'} &= \Sigma_i u_{ii} + \Sigma_i(u\mathfrak{n})_{ii}\\ \Sigma_i\Pi_{i'}u_{ii'} &= \Sigma_i\{u_{ii}(u \dagger 1)_{ii}\}\\ \Pi_i\Sigma_{i'}u_{ii'} &= \Pi_i\{u_{ii} + (u\mathfrak{n})_{ii}\}\end{aligned}$$

But even so the formulae are unsatisfactory for a reason soon to be explained.

Having brought the formulae to this state we next proceed to apply them a little more systematically.

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$$\begin{aligned}&\Sigma_i\Pi_j\{a_i(p_{ij}+\beta_j)\}\Sigma_{j'}\Pi_{i'}\{(c_{i'}+p_{i'j'})\overline{\beta}_{j'}\}\\ &= \Sigma_i\Pi_j\Sigma_{j'}\Pi_{i'}\{a_i(p_{ij}+\beta_j)(c_{i'}+p_{i'j'})\overline{\beta}_{j'}\}\\ &= \Sigma_i[a_i\Pi_j\Sigma_{j'}\{(p_{ij}+\beta_j)\overline{\beta}_{j'}\cdot\Pi_{i'}(c_{i'}+p_{i'j'})\}]\end{aligned}$$

We now ought to apply the last of the formulae just given. But it will be found there is a difficulty in finding where to place the Π.

What is to conclude is however seen to be this

$$\Sigma_i[a_i\Sigma_j\{\bar{\beta}_j p_{ij}(p_i + \beta \dagger 1)_j \Pi_{i'}(c_{i'} + p_{i'j})\}]$$

That is some A is such that a mark can be found which is not in the group β while this A possesses it while in reference to every other mark we can say that unless this A possesses it, it belongs to the group β and at the same time every thing that is not a C possesses this mark.

[Introductory Lecture on Logic]

MS 491: Fall–Winter 1883

The first thing that you have to ask of me is what it is that I propose to teach in this course. Logic. But what is meant by logic? What is commonly meant and what do I mean? To get an idea of what is commonly meant look into the *Summulae* of Petrus Hispanus, the ordinary logical text-book of the middle ages. The ages of faith are also the ages of logic,—the ages when logic was taken *au sérieux* in the most undoubting manner. In Petrus Hispanus you find everything centres about the syllogism. A syllogism is a formula consisting of three propositions, two of them premises from which the third—the conclusion—necessarily follows. The traditional example is

All men are mortal
Sortes is a man
∴ Sortes is mortal.

Every variety of syllogism is treated in Petrus Hispanus. All kinds of sophisms or false syllogisms are carefully guarded against. Thus, we are told that we must not reason in this way

Man is a common noun.
Sortes is a man;
∴ Sortes is a common noun.

That is incorrect because *Man* has a different *suppositio,*—or acceptation,—in the two premises.[1]

Very well you will ask if this is what logic is *par excellence,* do you believe that the study of these *suppositiones* and the like? No, I do not. This was implicitly believed in the middle ages, and on this conviction were written the great folios of Duns Scotus. But I think and everybody agrees that this logic is obsolete.

1. See Eck's commentary for a clear account of *suppositiones.*

The theory of syllogism and its varieties is called syllogistic. The study of formulae, of argument, of propositions of combinations of terms, is called *formal logic.* The medieval logic is not purely formal logic, although formal logic constitutes its ground-work. The logic of Kant and his immediate followers,—say Krug, Esser, Hamilton,—is more purely formal logic. The purest formal logic is the modern algebraical logic of Boole and De Morgan.

But do I think that formal logic in its most modern and improved shape is of very much use? Yes, of considerable use. Yet I do not think that this is more than a part, and a somewhat undeveloped part of the subject.

Aristotle is called the father of logic; and he is the inventor of the syllogism. His logic is however less purely formal by far than that of the middle ages. Everybody knows that Aristotle was the pupil of Plato and Plato of Socrates, and that Socrates was the originator of a kind of *dialectic* or systematic method of carrying on a conversational discussion, which placed him in sharp opposition to the rhetorical teachers called *Sophists.* These sophists were the real inventors of logic. With them logic consisted merely in a number of tricks by which your opponents could be entrapped into saying something absurd.

For instance, there was the trick called the Heap,—which you may still hear used by cross-examining lawyers every day. You say you have seen the accused many times. How many? You don't know exactly. Would you call a dozen times many? No. Two dozen? No. Would you call 50 times many? Yes. Would you call 25 times many? Yes. Then twenty-four times is not many but twenty-five is many; can a difference of only one time make all the difference between few and many? Large sums of money were paid to the sophists for instruction in such tricks as these.

What a state of thought this betrays! What a confusion between what a man says & what he has in his mind!

In Aristotle, we find a more advanced mode of thinking than that but still a very great confusion between the *word* and the *idea.*

Consider, for instance, his ten categories which are put forward as so many modes of *being* though it is apparent enough that they are mainly founded on the distinctions of the parts of speech. But grammar had at that time never been studied, and the difference between a mode of being and a mode of speech would have seemed very subtile.

Indeed, as regards the whole of formal logic from its first beginnings in the *Elenchi* of the sophists down to its most refined aspect in the logic of relatives,—I must say I think it is mainly a manipulation of modes of expression. And when I see such writers as George Bruce Halsted speaking of the *real power* of the Boolian algebra, I laugh in my sleeve. Not but that it has its utility. Some difficult problems can be resolved by its help. I admit it is useful, just as aluminium is a useful metal, for very nice opera glasses can be made of it, though it is far from being useful like iron or copper, zinc or tin.

The great value of formal logic seems to me to be that it is *instructive,* rather than that it is *useful.* It shows us what reasoning is built out of,—it has *theoretical* more than *practical* value.

Throughout the middle ages one of the important questions was whether logic is an art or a science. You will find some good remarks on this controversy in Hamilton's *Discussions* and you can dip into the controversy itself by looking over the *Commentary* of the Conimbricenses. Petrus Hispanus evades the question by saying that logic is at once an art & a science.

Dialectica est ars artium, scientia scientiarum, ad omnium methodorum principia viam habens.

Still this makes it a practical affair an art or science of methods of research.

It is in this sense that I lecture upon logic.

From this point of view too, I attach a certain importance to formal logic as being really at the base of all methods. You have heard that modern formal logic is a kind of algebra; but I have to tell you that all algebra is a kind of formal logic, and when we consider the immense influence that algebra has had in developing first modern mathematical and physical conceptions, and through these upon every department of thought and of sentiment, we may say that Formal Logic is the kind of logic that has exercised the most important influence upon civilization. And as for formal logic in the narrower sense, algebra without quantity, the science of pure quality as Jevons called it, this is also important as lying at the base of the quantitative kind. But I must confess though I have studied it a good deal myself it is rather an infantine thing to speak of as possessing "real power."

But modern logicians generally, particularly in Germany, do not regard logic as an art but as a science. They do not conceive the logician as occupied in the study of methods of research, but only as describing what they call the *normative laws of thought,* or the essential maxims of all thinking. Now I have not a high respect for the Germans as logicians. I think them very unclear and obtuse. But I must admit that there is much to be said in favor of distinguishing Logic from Methodology. If I were asked to say what is the distinction between the cast of thought of the logician and of the mathematician, I should say that the latter was always seeking methods for the solution of difficult problems while the former was occupied in analyzing reasonings to see what their essential elements precisely were. The mathematician is often illogical in this sense that he does not know precisely what his premises really are nor precisely where the *nodus* of his proof lies, but he advances and creates powerful methods nevertheless.

Let us say then that logic is not the *art of method* but the science which analyzes method.

A New Rule for Division in Arithmetic

P 266:* Science *2 (21 December 1883): 788–89

The ordinary process of long division is rather difficult, owing to the necessity of guessing at the successive figures which form the divisor. In case the repeating decimal expressing the *exact* quotient is required, the following method will be found convenient.

Rule for division.

First, Treat the divisor as follows:—

If its last figure is a 0, strike this off, and treat what is left as the divisor.

If its last figure is a 5, multiply the whole by 2, and treat the product as the divisor.

If its last figure is an even number, multiply the whole by 5, and treat the product as a divisor.

Repeat this treatment until these precepts cease to be applicable. Call the result the *prepared divisor.*

Second, From the prepared divisor cut off the last figure; and, if this be a 9, change it to a 1, or, if it be a 1, change it to a 9: otherwise keep it unchanged. Call this figure the *extraneous multiplier.*

Multiply the extraneous multiplier into the divisor thus truncated, and increase the product by 1, unless the extraneous multiplier be 7, when increase the product by 5. Call the result the *current multiplier.*

Third, Multiply together the extraneous multiplier and all the multipliers used in the process of obtaining the prepared divisor. Use the product to multiply the dividend, calling the result the *prepared dividend.*

Fourth, From the prepared dividend cut off the last figure, multiply this by the current multiplier, and add the product to the truncated dividend. Call the sum the *modified dividend,* and treat this in the same way. Continue this process until a modified dividend is reached which equals the original prepared dividend or some previous modified dividend; so that, were the process continued, the same figures would recur.

Fifth, Consider the series of last figures which have been successively cut off from the prepared dividend and from the modified dividends as constituting a number, the figure first cut off being in the units' place, the next in the tens' place, and so on. Call this the *first infinite number,* because its left-hand portion consists of a series of figures repeating itself indefinitely toward the left. Imagine another infinite number, identical with the first in the repeating part of the latter, but differing from this in that the same series is repeated uninterruptedly and indefinitely toward the right, into the decimal places.

Subtract the first infinite number from the second, and shift the decimal point as many places to the left as there were zeros dropped in the process of obtaining the prepared divisor.

The result is the quotient sought.

Examples.

1. The following is taken at random. Divide 1883 by 365.

First, The divisor, since it ends in 5, must be multiplied by 2, giving 730. Dropping the 0, we have 73 for the prepared divisor.

Second, The last figure of the prepared divisor being 3, this is the extraneous multiplier. Multiplying the truncated divisor, 7, by the extraneous multiplier, 3, and adding 1, we have 22 for the current multiplier.

Third, The dividend, 1883, has now to be multiplied by the product of 3, the extraneous multiplier, and 2, the multiplier used in preparing the divisor. The product, 11298, is the prepared dividend.

Fourth, From the prepared dividend, 11298, we cut off the last figure, 8, and multiply this by the current multiplier, 22. The product, 176, is added to the truncated dividend, 1129, and gives 1305 for the first modified divisor. The whole operation is shown thus:—

$$\begin{array}{r}
1883\\
\underline{6}\\
1129\boxed{8}\\
\underline{176}\\
130\boxed{5}\\
\underline{110}\\
2\boxed{40}\\
\underline{88}\\
\boxed{90}\\
19\boxed{8}\\
\underline{176}\\
19\boxed{5}\\
\underline{110}\\
12\boxed{9}\\
\underline{198}\\
2\boxed{10}\\
\underline{22}\\
24
\end{array}$$

We stop at this point because 24 was a previous modified dividend, written under the form 240 above. Our two infinite numbers (which need not in practice be written down) are, with their difference,—

$$\begin{array}{r}
\dot{1}0,958,90\dot{4},058\\
\underline{\dot{1}0,958,90\dot{4},109.58904\dot{1}095890\dot{4}}\\
51.58904\dot{1}095890\dot{4}
\end{array}$$

Hence the quotient sought is $5.\dot{1}5890410\dot{9}$.

Example 2. Find the reciprocal of 333667.

The whole work is here given:—

$$\begin{array}{l}
33366\boxed{7}\\
233367
\end{array}
\qquad\qquad
\begin{array}{r}
\boxed{7}\\
163496\boxed{9}\\
\underline{2102103}\\
226559\boxed{9}\\
\underline{2102103}\\
232866\boxed{2}\\
\underline{467134}\\
700000
\end{array}$$

Answer, $.\dot{0}0000299\dot{7}$.

Example 3. Find the reciprocal of 41.

Solution.—

```
   4|1          |9
  37|9        33|3
             111
             14|4
            148
            16|2
            74
            90
```

Answer, $.\dot{0}243\dot{9}$.

On the flexure of Pendulum Supports

P 253: **Coast Survey Report 1881, *359–441***

HISTORICAL.

The fact that the rate of a pendulum might be largely influenced by the elastic yielding of its support was first pointed out by Dr. Thomas Young in his article on "Tides" in the *Encyclopædia Britannica,* where he gave a correct mathematical analysis of the problem. Kater made use of the *noddy,* or inverted pendulum of Hardy, to assure himself that its support was sufficiently steady.

Hardy's noddy is a pendulum turning with a reed spring and provided with an adjustable bob. It differs from an ordinary pendulum, first, in being upside down, that is, having its centre of mass above its point of support; and second, in having a spring so strong as to act a little more strongly than gravity. The force tending to bring the pendulum to the vertical is then the excess of the force of the spring over the moment of gravity. In this way the noddy is easily adjusted so as to have the same period of oscillation as the pendulum used to determine gravity, while its moment of inertia is very small. In a note at the end of this paper I give the mathematical analysis of this state of things, from which it will be seen that Kater might have constructed his noddy in such a manner as to detect any amount of flexure sufficient to have a serious effect upon the period of his pendulum.

Bessel, at the end of §3 of his great memoir on the length of the seconds pendulum at Königsberg, states that he also used Hardy's noddy, and that he swung his pendulum again after stiffening the support. He adds that the effect on the period would probably be the same for his long pendulum as for his short one—a very just remark —which made it less necessary for him to attend to the rigidity of the stand.

The construction of English pendulum supports, that of Basevi, for example, shows that in that country this source of error was never overlooked. It is noticed even in brief accounts in English of the process of measuring gravity. Thus, a writer in the *Encyclopædia Britannica* proposed to make use of two different reversible pendulums of the same form but of different weights, in order to take account of the flexure, an idea lately borrowed by M. Cellérier.

When the reversible pendulum came into use the study of the writings of the older observers seems to have been neglected,[1] and the grave errors due to flexure were never suspected until Albrecht found a value of gravity at Berlin differing by nearly 2 millimetres from that of Bessel. So little was the true cause of this discrepancy at first suspected that it was paradoxically attributed to the neglect of a buoyancy correction.

In 1875, however, General Baeyer gravely suspected that the period of a pendulum swinging upon a Repsold tripod was affected by the oscillation of the latter, and in a circular addressed to the members of the committee on the pendulum of the International Geodetic Congress, he wrote: "The necessity of suspending the pendulum from a stand is a source of error, since a pendulum swinging on a stand sets the latter into oscillation and so influences the rate of the former. The effect could be diminished by the use of a shorter pendulum and smaller stand; but whether it would be rendered entirely insensible is open to question."

It was at this time that I first received the Repsold apparatus from the makers, of whom it had been ordered two years before, on the occasion of my first being charged with the pendulum operations of the Coast Survey. Becoming acquainted with General Baeyer's doubts, I determined to settle the question by measuring the flexibility of the Repsold tripod at the earliest opportunity. This I did at Geneva, where, though I only made a rough measurement, I found that the flexure was fully sufficient to account for the discrepancy between the determinations of Bessel and of Albrecht.

On September 25 of the same year I communicated my result to the standing committee of the Geodetical Congress. At the same sitting the reports of the different members of the pendulum committee were read. Dr. Bruhns said: "The question whether the

1. Thus, Bessel's idea of directly measuring the position of the centre of mass was supposed by the Swiss *savans* to belong to M. Cellérier.

stand is set into oscillation, and whether the rate of the pendulum is influenced thereby is, in my opinion, well worth investigation. But I should suppose that the stand could be made so stiff as to eliminate this source of error for a pendulum used only as a relative instrument." The views of M. Hirsch, who is so much occupied with the going of time-keepers, are interesting. He said: "The fear that the tripod of suspension may also enter into oscillation, unless it be a fact established by direct observations, seems to me unfounded. Indeed, it cannot be supposed that there are any true oscillations of a body of such a form resting on three points. Besides, the movement of the pendulum whose mechanical moment (*moment mécanique*) is slight on account of its small velocity, could only be communicated to the tripod by the friction of the knife on the supporting plane. Now, this friction is insignificant, as the slowness of the decrement of the amplitude shows, this being almost entirely due to the resistance of the air." It may be observed that the rolling friction of the knife-edge is, in truth, very slight, but the amount of the sliding friction is sufficient to hold the knife in place on the supporting plane. Dr. von Oppolzer, the designer of the Repsold tripod in its definitive form, said that the construction of the stand rendered any serious flexure *a priori* improbable; but he did not support this opinion by any calculations.

During the spring of 1876, having already measured the flexibility of the tripod in Paris, I remeasured it in Berlin, where my experiments were witnessed by General Baeyer and a party of gentlemen attached to the Prussian Survey.

In October, 1876, at the meeting of the standing committee of the International Geodetical Union at Brussels, the result of my experiments was announced by General Baeyer. M. Hirsch described certain experimental researches undertaken by him to ascertain whether there was any such flexure in the case of the Swiss tripod. He had, in the first place, employed an extremely sensitive level, which had not entered into oscillation while the pendulum was swinging upon it. It is not clear why M. Hirsch employed a very sensitive level, the natural time of oscillation of which would differ much more from the period of the pendulum than that of a less sensitive level would do. He also used an artificial horizon in the same way. M. Hirsch's conclusion is that "there remains no doubt that the Swiss stand is free from every trace of such oscillations." Dr. von Oppolzer entirely agreed with the views of M. Hirsch.

In the following summer I addressed to M. Plantamour a paper upon the subject, to be submitted to the next meeting of the Geodetical Congress. In this note, which is reprinted at the end of the present report, I first give a mathematical analysis of the problem. I next show experimentally that the motion of the knife-edge support is not a translation, but is a rotation, so that different parts of the head of the tripod, only a few centimetres distant from one another, move through very different distances. Consequently, measures of the flexure made anywhere except at the centre of the knife-edge plane require an important correction before they can be used to correct the periods. This is confirmed by experiments with a mirror while the pendulum is in motion. I next give a brief *résumé* of my statical measures of the flexure. I then give measures of the actual flexure under the oscillation of the pendulum, and show that the statical and dynamical flexibilities are approximately equal. Finally, I swing the same pendulum upon the Repsold support and upon another having seven times the rigidity of that one, and I show that the difference of the periods of oscillation agrees with the theory.

Immediately upon the reception of my manuscript, MM. Hirsch and Plantamour commenced new researches, designed to form an "étude approfondie de ce phénomène." These were embodied in a paper by M. Plantamour, which was read to the Geodetical Congress, and which has since been expanded into a memoir entitled "Recherches expérimentales sur le mouvement simultané d'un pendule et de ses supports." M. Plantamour finds fault with me, first, for having measured the flexure with a force five or ten times that of the deflecting force of the pendulum; and second, for measuring the elasticity statically instead of dynamically. The reply to the first objection is that the properties of metals are known to a great extent, that elasticity is not "une force capricieuse," and that no fact is better established than that an elastic strain is proportional to the stress up to near the limit of elasticity, which limit was not approached in the author's experiments. As to the second objection, I had shown by experiment that the statical and dynamical flexures are nearly equal; and I am willing to leave it to time to show whether this will not be assumed in future measures of the flexure of future pendulum supports. M. Plantamour caused a fine point fixed into the head of the tripod to press against a little mirror, mounted on an axis; and then observed the reflection of a scale in a telescope. The length of the path of light from the scale to the telescope divided by the distance of the bearing point from the axis of the mirror he calls the *grossissement;* so that had he used a fixed star in place of his scale, the *grossissement* would have been virtually infinite. From the given length of the lever it would appear that a movement of $0^{\mu}\!.03$ in the point would turn the mirror 4″. The aperture of the mirror is not stated, but it cannot be supposed that the error of observation would be less than this. It does not seem to me that the use of this mode of measurement, which magnifies the motion but little more than my method, is conducive to accuracy, especially in investigating the difference between statical and dynamical flexure. A certain finite force presses together the point and the lever. Dividing this force by the minute area of pressure, we find the pressure upon the metal is very great, approaching the crushing pressure. Now, the behavior of metals under great pressure is greatly influenced by the time. But my objection is not merely theoretical; I have myself made experiments upon this method, and, making them as skillfully as I could, I still found great uncertainty in the results.

The following table exhibits M. Plantamour's results:

M. Plantamour's flexure experiments.

	Flexure under swinging pendulum.	Flexure when weight is raised and lowered.	Statical flexure.
	μ	μ	μ
Support on floor, comparator removed	3.26 ± .05	3.17 ± .09	3.27 ± .04
On Geneva pier, comparator removed	3.17 ± .03	3.29 ± .08	3.48 ± .04
On Geneva pier, comparator in place	2.41 ± .06	2.50 ± .05	2.76 ± .04
On Berlin pier, comparator in place	2.51 ± .05	2.90 ± .04	3.24 ± .03
On wooden table, comparator in place	3.19 ± .03	3.26 ± .04	3.67 ± .02
On wooden table, comparator removed	4.42 ± .13	4.53 ± .04	4.98 ± .05
Excess:			
Geneva pier over floor	− .09 ± .06	+ .12 ± .12	+ .21 ± .06
Berlin over Geneva pier	+ .10 ± .08	+ .40 ± .06	+ .48 ± .05
Table over Geneva pier, comparator in place	+ .78 ± .07	+ .76 ± .06	± .91 ± .04
Table over Geneva pier, comparator removed	+1.16 ± .14	+1.36 ± .10	+1.71 ± .06
Effect of comparator:			
Geneva pier	− .76 ± .07	− .79 ± .09	− .72 ± .06
Table	−1.23 ± .14	−1.27 ± .06	−1.31 ± .05
Excess table over pier	− .47 ± .16	− .48 ± .11	− .59 ± .08

The table used is the same one shown in Fig. 26 of the *Coast Survey Report* for 1877. The numbers in the last line above should show the effect of the weight of 3 kilogrammes in diminishing the flexure of this table under a horizontal force of 100 grammes. The weights used in obtaining the first two numbers were about 100 grammes; but the last column is one-tenth the deflection produced by 1,000 grammes. It seems quite incredible that 3 kilogrammes, laid on the table, should really have an effect of this magnitude, so closely proportionate, too, to the deflecting force. It is highly desirable that this result should be confirmed by purely optical experiments; and until this is done, we must suspect that these large numbers indicate some error to which the method of observation is liable. It is certain that the comparator did not act as a brace to stiffen the instrument, and equally so that its weight is not sufficient to alter the modulus of elasticity of the brass of the support. It would seem, however, that the effect might be due to a film of some semi-elastic substance under the feet of the tripod. When the tripod is on the floor, no such effect is observed; when it rests on the Geneva pier the dynamical flexure is the same as when it is on the floor, but the statical flexure is much larger. On the Berlin pier the excess of the statical flexure over that on the Geneva pier is five times the dynamical excess. On the other hand, the excess of the dynamical flexure on the table over that at Berlin is half as great again as the statical excess.

MEASURES OF FLEXURE.

My own measures form two series, those made previous to, and those made subsequent to the publication of M. Plantamour's memoir.

In the first series, I was simply occupied in measuring the flexure of the Repsold tripod, as well when properly put up as when the nuts of the bolts were not tightened, of the Geneva support as mounted at Hoboken, and of my "stiffest" support. All the precise measures are statical, and, being made with a filar micrometer, are superior in accuracy to the subsequent ones.

In the second series, the flexures are always measured dynamically as well as statically, and the statical flexure is always found to be the greatest. On the excessively flexible Repsold tripod the difference is sufficient to affect the length of the seconds pendulum by 10^{μ}. Nevertheless, as the axis of motion is different for the two kinds of flexure, there are points at which the motion is *less* for dynamical than for statical flexure. And in point of fact, when the Geneva support rests on the Geneva tripod, the dynamical flexure of the centre of the knife-edge is *greater* than the statical flexure.

Experiments were also made upon the effect of leaving the nuts of the Repsold tripod entirely loose, of tightening them as much as possible by the hand, and of tightening them by a wrench. It is found that there is little difference between leaving them loose and tightening them by hand, but the effect of the wrench is to produce a stiffening equivalent to a shortening of the pendulum by 20 microns.

Experiments were also made upon the effect of placing a weight of 6 pounds, and afterwards of 25 pounds, upon the head of the Repsold support. The first weight produced absolutely no effect; the second moved the axis of motion a little, and thus caused a slight difference of flexure at some points.

Experiments were also made upon the effect of resting the Repsold support upon blotting-paper, upon blocks of oak, and upon blocks of India-rubber. In every case the difference between the statical and dynamical flexure was much increased.

The pendulum has also been *swung* on all these different supports and the period of oscillation determined with a view of ascertaining whether the statical or dynamical flexure should be used in calculating the corrections to the periods. The result, as might have been predicted from the mathematical theory, shows that a value intermediate between the two is to be taken. But the best way is to make the support so solid that the difference of the two kinds of flexure must be inconsiderable.

EXPERIMENTS TO DETERMINE THE FLEXURE-CORRECTION.

A.—*Flexure of the Repsold stand.*

To determine the flexure, a known force was applied statically to the stand, and the resulting deflection was measured. The principal experiments were made in the cellar of the Stevens Institute at Hoboken. The floor of the cellar is of brick laid down in cement directly on the solid ledge. The floor having been carefully cleaned, the three brass pieces which support the screw-feet of the Repsold tripod were laid down upon it, and the tripod itself was set up. The binding-screws of the feet were screwed up very tight. The pendulum, comparator, and metre were not placed on the tripod, but a mass of iron about equal to them in weight was placed on blocks on the lower part of the tripod in order to ballast it. To apply the force, a silken cord was wound round the tongue upon which the pendulum usually rests, just in the slot over which is the middle of the knife-edge, in such a manner that the cord when stretched horizontally was exactly at the level of the knife-edge. The cord passed horizontally and perpendicular to the knife-edge to a pulley-wheel over which it passed, and from which it hung down vertically; and to its extremity was attached a kilogramme. The pulley-wheel was one which belonged to an Atwood's machine; it turned with very little friction and its rim was accurately plane and perpendicular to the axis. This wheel rested on a stout wooden tripod; its axis was carefully adjusted to be parallel to the knife-edge and the upper part of the rim was brought to the level of the knife-edge. The usual position of the knife-edge is here referred to; but the pendulum was not actually in position. In the measurements of flexure, one person gently raised and lowered this weight alternately. The measurement of the deflection was made by another person, as follows: A micrometer scale on glass was fixed, either to the tongue or to an arm solidly fixed to the tongue, in such a way that the direction of measurement was parallel to the force applied to the tripod. This micrometer scale was observed by a microscope magnifying about fifty diameters and provided with a filar micrometer. This microscope was mounted on a separate, very stiff, iron stand resting on the floor, and carrying at its head a brass apparatus for holding the microscope. The optical axis of the microscope was made exactly parallel to the knife-edge and the filar micrometer screw was made parallel to the force applied to the stand, and the microscope was focused on the micrometer scale. Each division of the scale usually employed was about 12^{μ}. The filar micrometer wire (which was vertical) was made to bisect one division of the scale and the micrometer was read; it was then made to bisect another division, by turning the screw through about one revolution, and the micrometer was read again. Thus, the value of the revolution was obtained. The weight was then put on, and pointings were made upon the same two divisions. Then, the whole process was repeated until the weight had been put on five times. This made one set of experiments.

The following experiments were made to determine the position of the axis of rotation of the knife-edge support during flexure.

HOBOKEN, *March* 10, 1877. Ther. 13° C.—The micrometer scale, attached to an arm, was placed on the line of the knife-edge 53^{mm} in front of the anterior extremity of the tongue. The following were the readings of the filar micrometer on one of the lines of the scale with the weight alternately on and off (ρ throughout signifies a revolution of the micrometer screw):

	Weight off.	Weight on.
	ρ	ρ
	10.955	11.324
	.968	.320
	.978	.324
Means......	10.967	11.323

Difference, $+0^{\rho}.356$.

The arm was now lengthened so that the scale was 318^{mm} in front of the end of the tongue. The following readings of the filar micrometer were now made:

	Weight off.	Weight on.
	ρ	ρ
	10.344	10.762
	.350	.776
	.341	.793
	.335	.778
	.330	.772
Means......	10.340	10.776

Difference, $+0^{\rho}.436$.

The micrometer scale was next carried over to the other side of the instrument so as to be 496^{mm} behind the front end of the tongue. The following readings were now made:

	Weight off.	Weight on.
	ρ	ρ
	10.106	10.324
	.120	.334
	.141	.334
	.124	.346
	.136	.340
Means......	10.125	10.336

Difference, $+0^{\rho}.211$.

It will be understood that in all these experiments the arm to which the scale was fixed was attached to the tongue on which the pendulum rests, and that this arm was subjected to no force.

The above results are satisfied by supposing that the axis of rotation cuts the level of the knife-edge $1^{m}258$ behind the end of the tongue. The following table shows the agreement of the observations with this supposition.

Distance forward of end of tongue.	Flexure. Obs.	Calc.
m	ρ	ρ
+0.318	0.436	0.433
+0.053	0.356	0.361
−0.496	0.211	0.212

The scale was next (March 12, 1877, observer, Edwin Smith) fixed at 395^{mm} vertically below the end of the tongue. The following measures were then made:

	Weight off.	Weight on.
	ρ	μ
	13.739	13.260
	.700	.247
	.710	.261
	.700	.260
	.702	.243
	.710	....
Means......	13.710	13.254

Flexure, $+0^{\rho}446$.

The filar micrometer was here in the reverse position from its usual one, and hence the reading with weight off is greater than with weight on.

The scale was next placed 44^{cm} above the point of support and the following measures were made:

	Weight off.	Weight on.
	ρ	ρ
	10.523	10.737
	.453	.645
	.400	.578
Means...........	10.459	10.653

Deflection, $-0^{\rho}196$.

The filar micrometer was so shaky in this position that accurate measures could not be obtained, but the above answers the purpose.

The scale was next fixed on the end of the tongue and the three measures given below (series 18, 19, 20) were made. The mean of these gives a flexure of $0^{\rho}340$. These measures show that the axis of rotation cuts a vertical from the end of the tongue at a height of 1.07 metres above the level of the knife-edge. Thus we have on this hypothesis:

Distance below knife-edge.	Flexure. Obs.	Calc.
m	ρ	ρ
-0.44	0.196	0.196
0.00	0.340	0.332
$+0.395$	0.446	0.452

A large series of experiments were made at Hoboken to determine the amount of flexure.

*[*365.8 . . . 425.4*]*

General conclusions.

1. The flexibility of almost any pendulum support has an important effect on the time of oscillation, and should be measured.

2. The flexure rotates the knife-edge about an axis, sometimes not over 60^{cm} distant. It is, therefore, altogether erroneous to measure the flexure at any other point than the middle of the knife-edge, unless it be measured at a number of points and reduced to that point.

3. On a properly constructed support the difference between the statical and dynamical flexure should be immaterial. The dynamical flexure is less than the statical, owing to the time required for the transmission of the wave of strain to the more distant parts of the apparatus. The true correction seems to be intermediate between that calculated from the statical and the dynamical flexures, but pretty decidedly nearer to the latter.

4. A support like the Repsold tripod will grow more flexible with time, owing probably to the slight loosening of some parts.

5. Any dirt, cement, or other elastic film under the feet of such a tripod may greatly increase the flexure, as well as the difference between the two kinds.

6. If the flexure is considerable, it is likely to vary from day to day, or even during the course of an experiment.

7. The tightening of the parts may or may not greatly affect the flexure.

8. The loading of the support has no sensible effect.

9. Experiments made with weight and pulley give a larger value for the flexure than those made with the pendulum drawn to one side.

NOTE ON HARDY'S NODDY.

The theory of Hardy's noddy is very simple. When two pendulums oscillate on the same support in parallel planes, I have shown (*Am. Jour. Sci.*, third series, xviii, 113) that one of the differential equations is

$$\lambda D_t^2\varphi + D_t^2 s = -\gamma\varphi,$$

where

t is the time;
φ, the instantaneous angle of inclination of one pendulum;
s, the instantaneous linear displacement of its knife-edge from the position of repose;
λ, the virtual length of the pendulum;
γ, the vertical acceleration of each particle, or the constant of force of restoration of the pendulum.

The Hardy's noddy is a pendulum placed on the support of another pendulum so as to oscillate in a parallel plane. Its natural period $\tau = \eth\sqrt{\frac{\lambda}{\gamma}}$ is as nearly as possible equal to T, the period of the main pendulum; but γ, instead of being gravity, is the excess of the force of a spring over gravity and is made to be as small as possible, λ being correspondingly small, so as to give τ the right value. The noddy being very light, the value and changes of s are determined entirely by the main pendulum. We may, therefore, write

$$s = S\cos\frac{t}{T}\eth.$$

Substituting this value in the differential equation, the solution of the latter is

$$\varphi = \Phi\cos\frac{t-t_0}{\tau}\eth - \frac{1}{\gamma}\cdot\frac{S\eth^2}{\tau^2 - T^2}\cos\frac{t}{T}\eth.$$

But the noddy has no oscillation to begin with. This fact is represented by the equations

$$t_0 = 0 \qquad \Phi = \frac{1}{\gamma}\cdot\frac{\mathrm{S}\Game^2}{\tau^2 - \mathrm{T}^2}.$$

We thus have

$$\varphi = \frac{1}{\gamma}\cdot\frac{\mathrm{S}\Game^2}{\tau^2 - \mathrm{T}^2}\left(\cos\frac{t}{\tau}\Game - \cos\frac{t}{\mathrm{T}}\Game\right) = \frac{2}{\gamma}\cdot\frac{\mathrm{S}^2\Game^2}{\tau^2 - \mathrm{T}^2}\cdot\sin\frac{\tau - \mathrm{T}}{2\tau\mathrm{T}}t\,\Game\cdot\sin\frac{\tau + \mathrm{T}}{2\tau\mathrm{T}}t\,\Game.$$

This equation shows that the noddy oscillates with a period that is a sort of mean between its natural period and that of the large pendulum. The amplitude of oscillation increases from nothing at an initial rate equal to

$$\frac{\mathrm{S}\Game^3}{\gamma(\tau + \mathrm{T})\tau\mathrm{T}};$$

a rate not much affected by the value of $(\tau - \mathrm{T})$. But the amplitude increases more and more slowly, and reaches its maximum when

$$t = \frac{\tau\mathrm{T}}{\tau - \mathrm{T}},$$

after which it again diminishes and after the lapse of an equal time vanishes. At the beginning, the phase of motion of the support is $\frac{1}{2}\Game$, and that of the noddy is 0, so that the support is one quadrant ahead. At the time of the first maximum the phase of the support is

$$\left(\frac{1}{2} + \frac{\tau}{\tau - \mathrm{T}}\right)\Game$$

and that of the noddy is

$$\frac{1}{2}\frac{\tau + \mathrm{T}}{\tau - \mathrm{T}}\Game.$$

Subtracting the second from the first we see that the two motions are in opposition. When the motion of the noddy vanishes its phase is a quadrant in advance of that of the support. The motion immediately recommences, but

$$\sin\frac{\tau - \mathrm{T}}{2\tau\mathrm{T}}t\,\Game$$

is now negative, and this shows that the difference of phase changes to the opposite quadrant, and that the two oscillations again proceed toward opposition.

We have thus far not taken account of the resistance to the motion of the noddy, although this must evidently be large. In consequence of it, the natural motion of the noddy would be of the form

$$\varphi = \Phi \mathfrak{G}^{-\frac{t}{\theta}\mathfrak{O}} \cos\frac{t}{\tau}\mathfrak{O}.$$

From this we easily infer that the differential equation is

$$\mathrm{D}_t^2\varphi + 2\frac{\mathfrak{O}}{\theta}\mathrm{D}_t^2\varphi + \frac{\mathfrak{O}^2}{\tau^2}\varphi = \frac{\mathrm{S}}{\lambda}\frac{\mathfrak{O}^2}{\mathrm{T}^2}\cos\frac{t}{\mathrm{T}}\mathfrak{O}.$$

The solution of this is

$$\varphi = \frac{\mathrm{S}}{\lambda}\,\frac{1}{\dfrac{4\tau^2\mathrm{T}^2}{\theta^2(\tau^2-\mathrm{T}^2)} + \dfrac{\tau^2-\mathrm{T}^2}{\tau^2}}\left(\mathfrak{G}^{-\frac{t}{\theta}\mathfrak{O}}\cos\frac{t}{\tau}\mathfrak{O} - \cos\frac{t}{\mathrm{T}}\mathfrak{O}\right)$$

$$+\frac{\mathrm{S}}{\lambda}\,\frac{\dfrac{2\tau^2\mathrm{T}}{\theta(\tau^2-\mathrm{T}^2)}}{\dfrac{4\tau^2\mathrm{T}^2}{\theta^2(\tau^2-\mathrm{T}^2)} + \dfrac{\tau^2-\mathrm{T}^2}{\tau^2}}\sin\frac{t}{\mathrm{T}}\mathfrak{O}.$$

The signification of this is that the noddy approaches indefinitely toward settling down to an oscillation strictly synchronous with that of the support. Its ultimate amplitude is very little less than half what the maximum amplitude would be without resistance. But the phase may differ very much from that of the motion of the support. Namely, if the noddy is in precise adjustment to the period of the large pendulum, its phase will be one quadrant behind that of the support. If the noddy naturally oscillates slower than the large pendulum, its phase may be anywhere from one quadrant in arrear to opposition; if the noddy naturally oscillates faster than the pendulum, it may be anywhere from one quadrant behind, to coincidence.

If, then, γ is one-tenth of gravity, $\tau^2 - \mathrm{T}^2$ one-thousandth of a second, and S one-tenth of a micron, the amplitude of movement of the noddy will be one-thousandth of the radius, a quantity easily measured with a microscope.

[427 . . . 441]

On the Deduction of the Ellipticity of the Earth from Pendulum Experiments

P 254: **Coast Survey Report 1881,** ***442–56***

Any correction to pendulum experiments whose magnitude changes progressively with the latitude needs to be very accurately determined, lest an error be thereby introduced into the resulting value of the ellipticity of the earth. The atmospheric pressure and the temperature both give rise to corrections of this class, and the coëfficients of these corrections have been, in general, very inadequately determined. The experiments which have recently been published in the fifth volume of the India Survey, however, sufficiently determine these coëfficients for the Kater invariable pendulums. For all other forms of pendulums which have been used, excepting the large-sized Repsold reversible pendulum, their values are quite uncertain. But by far the greater part of the valuable data at hand in reference to the relative force of gravity in different latitudes consist of the times of oscillation of Kater invariable pendulums. So that even were we able properly to correct the others for temperature and pressure they would have very little influence upon the resulting ellipticity, for their absolute number is small, and they are not usefully distributed in latitude. Subject to uncertainties of reduction as they are, the only appreciable effect of admitting them into our calculations would be to increase the apparent probable error without altering the result. Under these circumstances, it has seemed best to me in making a new calculation of the ellipticity, as derived from this class of experiments, to restrict myself exclusively to those made with the Kater invariable pendulums.[1] The coëfficient of the effect

1. The present computation was begun before Major Herschel's work was received. The principles of my procedure being different from his, I have thought it worth while to complete my work and see how far Major Herschel's and my results would agree.

of atmospheric pressure upon pendulums of this class was determined in 1829, by Sabine, and the later researches under the auspices of the Indian Survey have not seriously invalidated his determinations. Baily, in his report on Foster's pendulum experiments, undertook to correct all the former results for this effect, but his work is erroneous in several particulars. Thus, he applies the correction to Lütke's numbers, to which it had already been rightly applied; he omits to reduce Sabine's values to the level of the sea, and commits various other errors which cannot be so easily explained in a few words. For the coëfficient of temperature effect a value has usually been assumed which was first obtained by Kater, and which is entirely incompatible with the known coëfficient of the expansion of brass and with the experiments upon the periods of oscillation at different temperatures. The value adopted in the reductions of the India Survey for pendulum No. 4, which was one of Sabine's pendulums, coincides precisely with that found by Lütke; differs very little from that obtained by the Indian Survey for their other pendulum, satisfies well the experiments by Sabine at different temperatures, agrees with our general knowledge of the coëfficient of expansion of brass, and has consequently been made use of by me in the reduction of all the experiments made with the Kater invariable pendulums. I have, therefore, made the necessary corrections to all the results with these pendulums, with the exception of those of the Indian series and of Lütke, which I have supposed to have been rightly reduced.

The corrections for elevation of the station above the level of the sea has hitherto been made by Dr. Young's rule, which is based on the assumption that all the earth and rock rising above the level of the sea is to be considered as attracting the pendulum as if it was so much additional matter beneath the pendulum in excess of what is found at the level of the sea. That this assumption, so foreign to the facts of the case, does not accord with the determinations of gravity made at the sea shore, and at great elevations in the neighborhood, has often been pointed out; in particular by M. Faye in the *Comptes Rendus,* June 21, 1880.

Let us first consider the question *à priori.*

The only geological cause of vast horizontal displacements of solid matter, such as alone could sensibly affect the pendulum, is denudation. This has always been at work to remove matter from the continents and deposit it on the bed of the ocean; and it seems probable that the general result of it must have been to diminish the amount of matter in those cones having their vertices at the centre of the earth and their bases on our present land, and to increase the amount of matter in those cones having their bases on our present sea bed. But it would seem that the effect of such transfer is one of those of which we cannot at present take account. We must, then, regard continental elevations, as produced by the elevation of a certain thickness of matter, equivalent to a crust.

If we use the following notation:

g = gravity,
δ = density of the continent,
Δ = mean density of the earth,
c = radius of the earth,
u = length of the chord from the centre to the circumference of a small circle,
h = elevation above the level of the sea,

then the vertical attraction of a cap of matter having the thickness h all over this small circle upon the point at its centre is, according to Pratt,

$$\frac{3}{2}g\frac{\delta 1}{\Delta c}\left(u+h-\sqrt{u^2+h^2}+\frac{uh}{2c}\right).$$

It has always been assumed that the second term of the polynomial in the parenthesis would alone be sensible except in extreme cases. If, now, we conceive that the elevation, instead of being produced by the addition of new matter, has been produced by the upheaval of the crust of thickness t, then the quantity in the parenthesis becomes

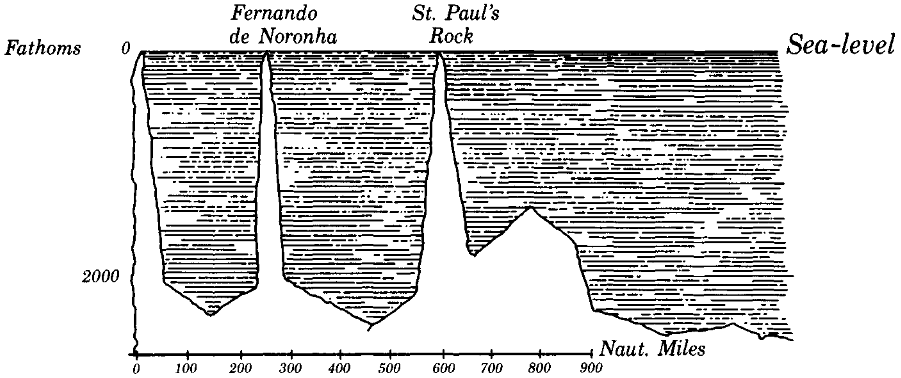
$$u+\sqrt{u^2+(t+h)^2}-\sqrt{u^2+t^2}-\sqrt{u^2+h^2}.$$

Here we perceive that that term which has generally been considered as alone sensible here disappears altogether; but a new term enters in of the first degree in h, the value of which is

$$\frac{th}{\sqrt{u^2+t^2}}.$$

It will be seen that this term, which is the principal part of the whole expression, is nearly inversely proportional to the radius of the cap, so that a large cap will have less downward attraction than a small one. The reason of this is as follows:

Through any point, p, let a line be drawn in a vertical direction; through p let a sphere be described having its centre upon this line. Taking p as the origin of polar co-ordinates, let a new surface be described having the radius vector in any direction equal to that of the sphere. Then this surface will be the surface upon which a particle of matter anywhere placed will exert a constant vertical attraction upon the point p. As the centre of the sphere varies its position along the vertical line through p, a succession of these inclosed surfaces of equal vertical attraction will be formed all in contact with one another at the point p. It will be seen then that the nearly spherical surface of the earth cuts all these surfaces in such a manner that the uplifting of the particles anywhere on its surface other than the immediate neighborhood of p will diminish the vertical attraction upon p. The exact calculation of the effect of the elevation of a crust being extremely troublesome, and the quantity being small, I have thought it sufficient to assume that the downward attraction would be one-tenth of the correction for elevation. This quantity which was adopted as satisfying moderately well the experiments on neighboring high and low levels is equivalent to assuming that the thickness of the crust is about one-eighth of the diameter of the space arched over; and the assumption has the same effect as if it were supposed that the continental elevations were produced by additional matter having three-fourths the density of water. We have so far only considered the solid material of the crust. In addition to this there is the water, which simply runs down to the lowest levels; so that its whole attraction is to be considered and allowed for. Let us consider a small island lying in the midst of the sea. In the first place, if there were no water about it, the gravity would be in excess upon such an island in consequence of the depression of the sea bed around it. This excess may, as we have seen, be roughly taken as equal to what would be produced by the downward attraction of an excess of matter sufficient to build up the island and having three-fourths the density of water. In the case of a small island the effect will be greater than this. In the case of a large island, especially one lying near a continent, the effect will be increased by the surrounding deposits of matter resulting from denudation. On the whole, then, we may roughly take the excess of gravity on the island to what it would be if the island was built up of extra matter having the density of water. Then when we take into account the attraction of the ocean itself, we may say that the excess of gravity upon an island will be about equal to what would be produced by an ocean having the general depth of the ocean bed quite outside the island and extending completely over the space occupied by the island. What is meant will be understood the moment that we look at a rough profile of the equatorial Atlantic through the islands Fernando de Noronha and Saint Paul's Island.



Vertical scale in fathoms. Horizontal scale in nautical miles.

Coast stations are generally to be considered as really continental, since the true boundary of the continent at a depth of some 100 fathoms is considerably outside the coast. Stations near this boundary may be considered to be like deep-water islands surrounded by water of half the depth of that which is upon one side of the station. Taking the contour chart of the ocean bed given in Mr. Wild's book called *Thalassa,* I find that the following pendulum stations have to be corrected for the depth of the ocean about them:

Stations at which corrections are to be applied for attraction of water.

	Fathoms.
Hare Island	1,000
Melville Island	1,000
Galapagos	2,000
St. Thomas	3,000
Ascension	2,000
Sierra Leone	500
Bahia	1,000
Jamaica	700
Spitzbergen	500
Point Bowen	500

	Fathoms.
Valparaiso	250
Port La Coquille	3,000
Guahan	3,000
Port Lloyd	2,000
St. Helena	2,000
Montevideo	200
Staten Island	500
South Shetland	500
Cape Horn	500
Fernando de Noronha	3,000
Minicoy Island	500

Rough as these corrections are, the application of them will show what we have to expect from a more exact treatment according to the principles just laid down. I have calculated the ellipticity with these corrections, and find it to be (taking $\eta = .0052375$)

$$\frac{1}{291.5 \pm 0.9}.$$

This probable error is rather smaller than that obtained by the treatment of the latest and best geodetical operations and is much smaller than that obtained hitherto from pendulum observations. Upon comparing my residuals with those of Clarke (*Geodesy,* p. 349), it will be observed that, as a general rule, the residuals that now remain large had previously been much larger, and are generally marine stations, from whence I infer that a more careful estimate of the attraction of the ocean would produce a still further improvement of the result. That which is most needed to improve the state of our knowledge of gravity are additional experiments in the Arctic Circle. It is desirable that these experiments should be made with the Indian apparatus, and it is also to be desired that the same apparatus should be used at Trinidad, Ascension, and Maranham, the three stations which are common to the great expeditions of Sabine and Foster.

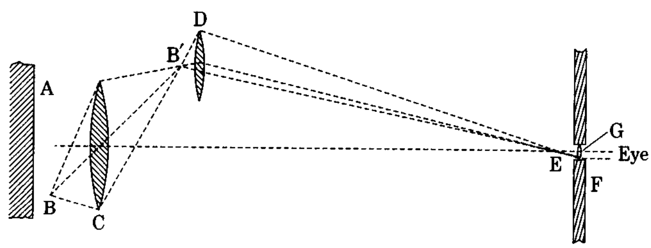
*[*445.21 . . . 456*]*

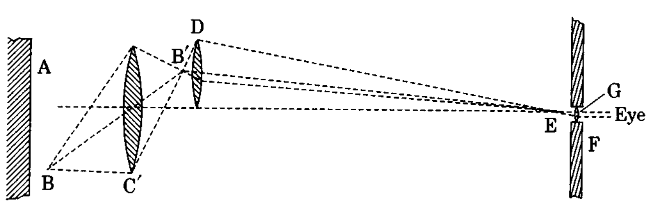
On a Method of Observing the Coincidence of Vibration of Two Pendulums

P 255: **Coast Survey Report 1881,** ***457–60***

NEW YORK, *August* 2, 1878.

DEAR SIR: I have made a full set of experiments with different methods of observing the coincidences of two pendulums. By far the most accurate method is the following:





To the wall A of a small chamber is fixed a clock which carries on its pendulum a brilliantly illuminated horizontal scale, say of half millimetres. B represents the middle point of this scale. C or C′ is a large achromatic lens placed so that an image of scale will be formed at B′ at a fixed distance from the wall. There are two positions, C and

C′, which the lens may have to effect this. In one position the amplitude of vibration of B is multiplied in a certain ratio, say *r;* in the other position it is diminished in the same ratio. This is a well-known optical principle. The lens moves in a slide, by means of strings, and up to stops, so that it can be drawn at any time from one of these positions to the other. At D, D is the plane of oscillation of the pendulum on knife-edges, which measures the force of gravity. The plane of motion of D is parallel to that of B. This pendulum carries a lens which brings the image at B′ at focus at E close to the opposite wall F of the room. When the amplitude of D is to the amplitude of B′ as E D is to E B′, the image remains stationary at E, provided the pendulums are in coincidence. The image E is observed by means of an eye-piece, G, fixed in the wall.

The effect is this: The lens C being in the position C (the nearer to B), and the pendulum D oscillating at nearly the right amplitude, the image of the scale will generally flash across the field of vision so rapidly that it can only be seen at the instant of reversing its direction. But as the pendulums approach coincidence it moves less and less, and if the two amplitudes are precisely in the right proportion it finally comes absolutely to rest with the middle of the scale just on the cross-wire of the eye-piece (*i.e.,* just where it would be with the pendulums both at rest). As a general rule, however, it does not come absolutely to rest, but finally gets over, say a millimetre in a second, after which it begins to move faster. The approach to and departure from the minimum amplitude is not very gradual but rather sudden, so that there is no difficulty at all in deciding which is the minimum oscillation. The observer has to note at what second the minimum oscillation occurs, and also what part of the scale is on the cross-wire at the turning points before and after this oscillation; then, by the application of a formula, the time can readily be determined to near $\frac{1}{1000}$th of a second. The lens C is then pulled forward to the position C′, and the observation is repeated when the pendulum has diminished its arc of oscillation sufficiently.

The formula which applies is as follows: Let s be the apparent oscillation of the scale; then,

$$s = a_1 \cos(b_1 t + c_1) - a_2 \cos(b_2 + c_2)$$

$$= -(a_1 + a_2) \sin\left(\frac{b_1 + b_2}{2} t + \frac{c_1 + c_2}{2}\right) \sin\left(\frac{b_1 - b_2}{2} t + \frac{c_1 - c_2}{2}\right)$$

$$+(a_1 - a_2)\cos\left(\frac{b_1 + b_2}{2}t + \frac{c_1 + c_2}{2}\right)\cos\left(\frac{b_1 - b_2}{2}t + \frac{c_1 - c_2}{2}\right).$$

Here a_1 and a_2 are the amplitudes of the knife-edge pendulum and of the image of the other formed at B′ and reduced in the ratio $\frac{\mathrm{ED}}{\mathrm{EB}'}$. b_1 and b_2 are the reciprocals of the periods of the two oscillations multiplied by 180°; c_1 and c_2 depend upon the initial conditions. Since b_1 and b_2 differ very little in value (about $\frac{1}{150}$), it follows that sin and cos $\left(\frac{b_1 + b_2}{2}t + \frac{c_1 + c_2}{2}\right)$ go through all their values in about two seconds, while sin and cos $\left(\frac{b_1 - b_2}{2}t + \frac{c_1 - c_2}{2}\right)$ go through their values in about five minutes. We thus see why the scale should appear to oscillate back and forward in a second with a changing amplitude. If a_1 did not change, the amplitude would go through its cycle of changes in five minutes.

Let us see what the amplitude of oscillation of s is for a particular value of $\frac{b_1 - b_2}{2}t + \frac{c_1 - c_2}{2}$. Considering this as fixed, the turning takes place when

$$(a_1 + a_2)\cos\left(\frac{b_1 + b_2}{2}t + \frac{c_1 + c_2}{2}\right)\sin\left(\frac{b_1 - b_2}{2}t + \frac{c_1 - c_2}{2}\right)$$

$$+(a_1 - a_2)\sin\left(\frac{b_1 + b_2}{2}t + \frac{c_1 + c_2}{2}\right)\cos\left(\frac{b_1 - b_2}{2}t + \frac{c_1 - c_2}{2}\right) = 0.$$

Or when

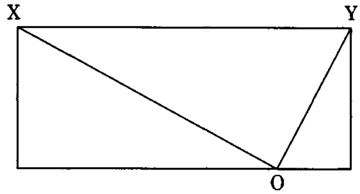
$$-\frac{a_1 + a_2}{a_1 - a_2}\tan\left(\frac{b_1 - b_2}{2}t + \frac{c_1 - c_2}{2}\right) = \tan\left(\frac{b_1 + b_2}{2}t + \frac{c_1 + c_2}{2}\right).$$

Putting in the figure below,

$$\frac{a_1 + a_2}{2}\sin\left(\frac{b_1 - b_2}{2}t + \frac{c_1 - c_2}{2}\right) = \mathrm{OX}$$

and

$$\frac{a_1 - a_2}{2}\cos\left(\frac{b_1 - b_2}{2}t + \frac{c_1 - c_2}{2}\right) = \mathrm{OY}$$



we see that the maximum value of s, that is, the value at the turning point, is

$$\sqrt{(\mathrm{OX})^2 + (\mathrm{OY})^2}$$

which is

$$\sqrt{(a_1^2 + 2a_1a_2 + a_2^2)\sin^2\left(\frac{b_1 - b_2}{2}t + \frac{c_1 - c_2}{2}\right) + (a_1^2 - 2a_1a_2 + a_2^2)\cos^2\left(\frac{b_1 - b_2}{2}t + \frac{c_1 - c_2}{2}\right)}$$

$$= \sqrt{a_1^2 + a_2^2 - 2a_1a_2\cos\big((b_1 - b_2)t + c_1 - c_2\big)}.$$

This is the amplitude of the apparent oscillation of the scale. Its greatest value is $a_1 + a_2$ and its least value is $a_1 - a_2$. A little calculation will show that supposing a_1 to be one twenty-fifth part larger than a_2, the oscillation next to the smallest has double the amplitude of the smallest. If, therefore, we only sought to know the coincidence within one second (giving the time to $\frac{1}{150}$th of a second) no calculation would be necessary; but we can find the time of coincidence nearer than a second.

For this purpose we require the precise condition which defines the moment of turning. It is

$$\left(-(a_1 + a_2)\frac{b_1 + b_2}{2} - (a_1 - a_2)\frac{b_1 - b_2}{2}\right)\cos\left(\frac{b_1 + b_2}{2}t + \frac{c_1 + c_2}{2}\right)$$

$$\cdot\sin\left(\frac{b_1 - b_2}{2}t + \frac{c_1 - c_2}{2}\right) + \left(-(a_1 + a_2)\frac{b_1 - b_2}{2} - (a_1 - a_2)\frac{b_1 + b_2}{2}\right)$$

$$\cdot\sin\left(\frac{b_1 + b_2}{2}t + \frac{c_1 + c_2}{2}\right)\cos\left(\frac{b_1 - b_2}{2}t + \frac{c_1 - c_2}{2}\right)$$

$$= -(a_1b_1 + a_2b_2)\cos\left(\frac{b_1 + b_2}{2}t + \frac{c_1 + c_2}{2}\right)\sin\left(\frac{b_1 - b_2}{2}t + \frac{c_1 - c_2}{2}\right)$$

$$-(a_1b_1 - a_2b_2)\sin\left(\frac{b_1 + b_2}{2}t + \frac{c_1 + c_2}{2}\right)\cos\left(\frac{b_1 - b_2}{2}t + \frac{c_1 - c_2}{2}\right) = 0.$$

Hence

$$\tan\left(\frac{b_1+b_2}{2}t+\frac{c_1+c_2}{2}\right)$$

$$= -\frac{a_1b_1+a_2b_2}{a_1b_1-a_2b_2}\tan\left(\frac{b_1-b_2}{2}t+\frac{c_1-c_2}{2}\right)\sin\left(\frac{b_1+b_2}{2}t+\frac{c_1+c_2}{2}\right)$$

$$= \frac{-\dfrac{a_1b_1+a_2b_2}{a_1b_1-a_2b_2}\tan\left(\dfrac{b_1-b_2}{2}t+\dfrac{c_1-c_2}{2}\right)}{\sqrt{1+\left(\dfrac{a_1b_1+a_2b_2}{a_1b_1-a_2b_2}\right)^2\tan^2\left(\dfrac{b_1-b_2}{2}t+\dfrac{c_1-c_2}{2}\right)}}$$

$$= \frac{\mp(a_1b_1+a_2b_2)\sin\left(\dfrac{b_1-b_2}{2}t+\dfrac{c_1-c_2}{2}\right)}{\sqrt{(a_1b_1-a_2b_2)^2\cos^2\left(\dfrac{b_1-b_2}{2}t+\dfrac{c_1-c_2}{2}\right)+(a_1b_1+a_2b_2)^2\sin^2\left(\dfrac{b_1-b_2}{2}t+\dfrac{c_1-c_2}{2}\right)}}$$

$$\cos\left(\frac{b_1+b_2}{2}t+\frac{c_1+c_2}{2}\right)$$

$$= \frac{\pm(a_1b_1-a_1b_2)\cos\left(\dfrac{b_1-b_2}{2}t+\dfrac{c_1-c_2}{2}\right)}{\sqrt{(a_1b_1-a_2b_2)^2\cos^2\left(\dfrac{b_1-b_2}{2}t+\dfrac{c_1-c_2}{2}\right)+(a_1b_1+a_2b_2)^2\sin^2\left(\dfrac{b_1-b_2}{2}t+\dfrac{c_1-c_2}{2}\right)}}.$$

Hence

$$s = \pm\frac{(a_1+a_2)(a_1b_1+a_2b_2)\sin^2\left(\dfrac{b_1-b_2}{2}t+\dfrac{c_1-c_2}{2}\right)\mp(a_1-a_2)(a_1b_1-a_2b_2)\cos^2\left(\dfrac{b_1-b_2}{2}t+\dfrac{c_1-c_2}{2}\right)}{\sqrt{(a_1b_1+a_2b_2)^2\sin^2\left(\dfrac{b_1-b_2}{2}t+\dfrac{c_1-c_2}{2}\right)+(a_1b_1-a_2b_2)^2\cos^2\left(\dfrac{b_1-b_2}{2}t+\dfrac{c_1-c_2}{2}\right)}}.$$

The coincidence occurs when

$$\frac{b_1-b_2}{2}t+\frac{c_1-c_2}{2}=0.$$

When the turning is near coincidence, so that this is a very small quantity,

$$\frac{b_1-b_2}{2}t+\frac{c_1-c_2}{2}=\frac{b_1-b_2}{2}dt$$

$$s = \pm \frac{(a_1 - a_2)(a_1 b_1 - a_2 b_2) + \left\{(a_1 + a_2)(a_1 b_1 + a_2 b_2) - (a_1 - a_2)(a_1 b_1 - a_2 b_2)\right\}\frac{(b_1 - b_2)^2}{4}(dt)^2}{\sqrt{(a_1 b_1 - a_2 b_2)^2 + \left\{(a_1 b_1 + a_2 b_2)^2 - (a_1 b_1 - a_2 b_2)^2\right\}\frac{(b_1 - b_2)^2}{4}(dt)^2}}$$

$$= \pm \frac{(a_1 - a_2) + \frac{1}{2}\frac{a_1 a_2 (b_1 + b_2)}{a_1 b_1 - a_2 b_2}(b_1 - b_2)^2 (dt)^2}{\sqrt{1 + \frac{a_1 a_2 b_1 b_2}{(a_1 b_1 - a_2 b_2)^2}(b_1 - b_2)^2 (dt)^2}} = \pm \left\{(a_1 - a_2) + \frac{a_1 a_2 (a_1 b_1^2 - a_2 b_2^2)(b_1 - b_2)^2}{(a_1 b_1 - a_2 b_2)^2}(dt)^2\right\}.$$

By observing the value of s on the turning points of the smallest oscillation the amplitude will give $(a_1 - a_2)$, and the difference of amplitudes on the two sides will give dt to about a sixth of a second on substituting in the last equation the known values of $a_2 b_1$ and b_2 and the value of a_1 determined from the amplitude. This will determine the time to about $\frac{1}{1000}$ th of a second.

My opinion, however, is, that the best way of making pendulum observations is with my relay described in my printed paper.

Yours, very respectfully,

C. S. PEIRCE, *Assistant.*

C. P. PATTERSON,
Superintendent Coast and Geodetic Survey.

NOTE.

Since witnessing Major Herschel's experiments, I have done some additional work with the method of coincidences. I have used a scale of half millimetres pasted on the clock pendulum, and brought to focus by a good lens, on the plane of oscillation of the point of a fine cambric needle placed vertically on the gravity pendulum. The correction for decrement of arc—an effect Major Herschel detected and for which Mr. Farquhar has obtained a formula—is considerable in the case of the reversible pendulums. I have read off its value from a diagram constructed for the purpose.

FEBRUARY 20, 1883.

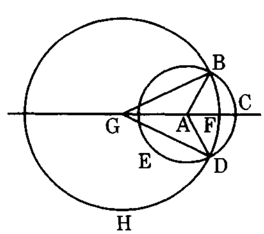
Additional Note on the Method of Coincidences

MS 445: February 1883

The old method of coincidences employed a single disk fixed to the clock pendulum.

The following will make the theory plain. Taking a system of rectangular coördinates, let the abscissas represent positions on the horizontal line of motion of a point of the gravity pendulum and of the image on the same line of a point of the clock pendulum. Let the ordinates represent velocities. Then draw a circle having its centre on the axis of abscissas, and a uniform motion around the circumference of this circle will represent the motion of the point on either pendulum.

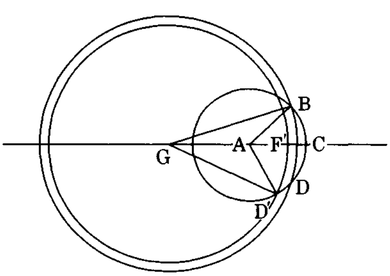
Fig. 1



Let the circle BCDE (Fig. 1), having its centre at A represent the motion of the point on the image of the clock pendulum, while the circle BFDH having its centre at G represents the motion of the point on the gravity pendulum. The two pendulums are supposed to be in coincidence (that is, in the same phase of motion) when one point is at C and the other at F. Of course, they might not be exactly in coincidence at the extremity of their motions, but we suppose that a possible error of one second in the time of coincidence to be neglected. What is observed is the times at which the points on the two pendulums have the same position and the same velocity. These states are represented by the points B and D on the diagram. Then, since the arcs are described with uniform velocity, the mean of the two times gives the time at which the point on one pendulum is in the state C and that on the other in the state F, that is, the time of coincidence.

Major Herschel has improved the old method by using many disks instead of one and thus gets many different pairs of times from which the time of coincidence is deduced. Now he finds that these different points do not give the same time of coincidence; and Mr. Farquhar has shown that this is owing to the decrement in the arc of oscillation of the gravity pendulum between the first and second observations of each pair. This is made plain by Fig. 2. The time of reaching the state of things B is first noted; but the state of things D never occurs owing to the diminished amplitude of the gravity pendulum, and what is noted is the time

Fig. 2



of reaching the state of things D′. Now, AD′ being less than AB, the angle CAD′ representing the time between coincidence and the event D′ is greater than the angle BAC representing the time from the event B to coincidence. Hence when the mean of the observed times is taken the resulting epoch is too late to represent correctly the true time of coincidence. To obtain the true time it would be necessary from the three sides of an oblique triangle to calculate an angle.

To remedy this inconvenience, as far as possible, I employ, as in my previous experiments with the method of coincidences, a paper scale of millimetres horizontally fixed to the clock pendulum, in place of the disks. The observations are thus rendered somewhat more difficult, but are not materially less accurate. Besides noting the times of the events B and D for a selection of divisions the observer also notes the maximum or minimum scale reading which the pendulum reaches. In this way the arc of oscillation of the pendulum is observed; for this arc is equal to the observed maximum diminished by the excess of the zero reading of the scale over the half amplitude of the clock pendulum, and also to the sum of the zero and the clock's half amplitude diminished by the observed minimum. The following is an example. On 1882, May 28, the gravity pendulum being at rest the vertical wire of a reading telescope was brought into coincidence with the observing-point of this pendulum and the scale on the clock pendulum was read at the extremities *[. . .]*

[Design and Chance]

MS 494: December 1883–January 1884

The epoch of intellectual history at which the world is now arrived finds thought still strongly under the influence imparted to it in 1859 by Darwin's great work. But a new element has crept in, not introduced by any great book, yet already showing itself in different directions, and destined as it seems to me to play a considerable part in the coming years,—I mean the tendency to question the exact truth of axioms. It appears to me that the development of this general idea in the various realms of mathematics, positive science, and philosophy is, in the immediate future, likely to teach us more than any other general conception. It has already done important work in geometry, in which our own Professor Story has an honorable share. In physics we see its influence in the investigations of Crookes and Zöllner into the phenomena of spiritualism & supernaturalism, in regard to which the attitude of scientific men must now be essentially different from what it was 25 years ago. For my part, I cannot withhold my approval of the proceedings of the society for the prosecution of psychical research, which is engaged in the careful examination of all kinds of phenomena which suggest the possibility of the relation between body and soul being different from what ordinary experience leads us to conceive it. I do not mean to say, and that society does not say, that any facts have yet been established sufficient to call for a modification of existing conceptions; but I do say that enough evidence has been collected to make a careful & serious examination of the matter no waste of time; and that the bias that formerly existed and was rightly entertained in favor of the dicta of common sense upon this subject, is sensibly weakened and rightly weakened by its having been proved that the axioms of geometry are mere empirical laws whose perfect exactitude we have no reason whatever to feel confident of.

The scientific world will not be disturbed because all the weak-minded persons whose mental equilibrium was shaken by spiritualism during the period when it was in fashion will now turn round and say, we investigated these things long ago and always told you you were wrong not to investigate them,—and now we are glad you see your error. The scientific world was entirely right before, when it declined to waste time in absurd inquiries; and it is quite consistent in saying,—as I think it is about to say,—that the pretended facts seem now to deserve examination. More than that, as a general maxim in scientific method, I maintain that at one stage of inquiry it is quite right to insist strongly on the exactitude of established laws, to question which would only lead to confusion, while at a later stage it is proper to question the exactitude of those same laws when we are in possession of a guiding idea which shows us in what manner they may possibly be corrected.

I may illustrate this point by something which comes within the experience of every man. Every man at some time *mislays* something; I for my part, I am ashamed to confess, am rather apt to do so. I entirely forget what I did with the object and am obliged to hunt it up. Now at the beginning of my hunt, I am guided by the knowledge that I have of my own habits; I look for the object where the ordinary rule of my action would have led me to place it, and I rightly decline to spend my time in looking where I almost know I should never have left it. But at a later stage of my search when the likely places are exhausted I begin to look in the unlikely ones, and in doing so I am equally in the right.

In a somewhat similar way, when we first begin to question an axiom, we do not say that it is likely to be inexact;—far from it. We only say that the question whether it is exact or not has come to have a claim to consideration greater than it had had in a former state of science.

What I propose to do tonight is, following the lead of those mathematicians who question whether the sum of the three angles of a triangle is exactly equal to two right angles, to call in question the perfect accuracy of the fundamental axiom of logic.

This axiom is that *real things exist* or in other words, what comes to the same thing, that every intelligible question whatever is susceptible in its own nature of receiving a definitive and satisfactory answer, if it be sufficiently investigated by observation and reasoning. This is the way I should put it; different logicians would state the axiom differently. Mill, for instance, throws it into the form: *Nature is uniform.* I am not now concerned with inquiring how it ought to be stated. It is the axiom itself whatever be the proper form of it which I wish to call into doubt.

Let me be quite understood. As far as all ordinary and practical questions go I insist upon this axiom as much as ever,—as much as anybody can do. I should think that any man who proposed to *go* on any other principle as a maxim of reasoning would be as *insane* as Gauss, Lobachewsky, Riemann or Helmholtz would hold that geodesist to be, who should think that he could detect any departure from the accepted laws of geometry, in any triangle measured on this earth. It is worth while to notice how much it means to question the exactitude of an axiom. There are 25 stars whose parallaxes have been determined by unexceptionable methods. According to ordinary geometry, this parallax should slightly exceed *zero.* According to the non-Euclidean geometry, it might be either more or less than zero, and the value the nearest possible to zero should be proportional to the area of the triangle. Now of the 25 stars, there is but one for which the parallax comes out negative. It is α Cygni of which there is but one determination and the probable error is more than half the value of the negative parallax. There are however several whose parallax is less than 0.″1, among them Groombridge 1830, one of the best determined of all. We may therefore conclude that for a star so far distant that the area of the triangle is over a thousand millions of millions of millions of square miles the error of the ordinary Geometry is a quantity less than $\frac{1}{500}$ of the smallest speck that can be seen on the broad horizon, and the extinction of the human race is to be expected sooner than the applicability of the non-Euclidean geometry to any geodesic triangle. It is a doubt comparable to this which I propose in regard to the axiom of logic.

In order to explain what I mean, let us take one of the most familiar, although not one of the most scientifically accurate statements of the axiom viz.: that *every event has a cause.* I question whether this is exactly true. Bodies obey sensibly the laws of mechanics; but may it not be that if our means of measurement were inconceivably nicer, or if we were to wait inconceivable ages for an exception, exceptions irreducible in their own nature to any law would be found? In short, may it not be that *chance*, in the Aristotelian sense, mere absence of cause, has to be admitted as having some slight place in the universe.

Is this a mere idle doubt? Are there any considerations which lead to such a supposition and can any use be made of it if it be granted?

In the first place for the motive of the doubt. If we are to admit that every event has a cause, we are bound by every maxim of consistency to grant that every fact has an explanation, a reason. When we detect a motion among bodies, the demand for a cause is held to be just. Suppose then we find that cause to be that the bodies repel one another inversely as the fifth power of the distance, according to Maxwell's theory about molecules. Now that force is itself not an event; but are we, merely because it is not an event, but is a different kind of fact, not entitled to ask *why* molecules should repel one another inversely as the fifth power of the distance, with the confidence that some reason for it there must be? Gravitation seems less strange in its law, which is that of an emanation. In the case of heat we have energy radiated from the sun; but the energy of gravity does not follow the law of radiation. The singular analogy, therefore, between the acceleration of gravitation and the energy of heat demands an explanation. There has been an attempt to explain gravitation by the impact of particles, but the law of impact is as unreasonable as that of gravity or more so.

Among the things that demand explanation, then, are the laws of physics; and not this law or that law only but every single law. Why are the three laws of mechanics as they are and not otherwise? What is the cause of the restriction of extended bodies to three dimensions?

And then the general fact that there are laws, how is that to be explained?

The general idea of evolution governs science more and more; and every system of philosophy since Kant, however idealistic or however materialistic has strongly felt its influence. Evolution is the postulate of logic, itself; for what is an *explanation* but the adoption of a simpler supposition to account for a complex state of things.

Every theory of evolution that I have seen is more or less special. It is true that in order to be scientifically grounded a theory must be special; but nevertheless evolutionist science and evolutionist philosophy are more closely connected in logic than scientists //commonly suppose/are apt to think// them to be. Upon this subject, I refer to the remarks of Clifford's concerning very general conclusions *à* *propos* of Spontaneous Generation. A most important premise, playing a great part in the establishment of the Nebular Hypothesis or the Theory of Natural Selection, is that things must on the whole have proceeded from the Homogeneous to the Heterogeneous.

Now the theories of evolution that have hitherto been set forth, at least to the very limited extent, I am sorry to confess it, with which I am familiar with them, while they do go to make it probable that organisms and worlds have taken their origin from a state of things indefinitely homogeneous, all suppose essentially the same basis of physical law to have been operative in every age of the universe.

But I maintain that the postulate that things shall be explicable extends itself to *laws* as well as to states of things. We want a theory of the evolution of physical law. We ought to suppose that as we go back into the indefinite past not merely special laws but *law* itself is found to be less and less determinate. And how can that be if causation was always as rigidly necessary as it is now?

But let me state the point in all its generality. That very postulate of logic whose rigid accuracy I call in question, itself demands that every determinate fact shall have an explanation, and there is no reason in making any exception. Now among the determinate facts which ought thus to be explained is the very fact supposed in this postulate. This must also be explained, must be among the things which have been somehow brought about. How then can it be absolutely, rigidly & immoveably true?

So much for the motive of the doubt. Now for the question to what useful result will this hypothesis lead? It is not my purpose to offer any determinate explanation of a single one of the laws of nature. All that I can do is to suggest that they may perhaps be explicable by means of hypotheses having a certain general *[. . .]*

It has always seemed to me singular that when we put the question to an evolutionist, Spencerian, Darwinian, or whatever school he may belong to, what are the agencies which have brought about evolution, he mentions various determinate facts and laws, but among the agencies at work he never once mentions *Chance.* Yet it appears to me that chance is the one essential agency upon which the whole process depends. About the nature of the ordinary phenomena of chance there can be no dispute whatever. A certain antecedent, for example that I throw a die from a box, determines the general character of a consequent, namely that a number is turned up, but does not specifically determine the character of the consequent, that is what number that is to be; but that is determined by other causes which cannot be taken into account. I suppose that on excessively rare sporadic occasions a law of nature is violated in some infinitesimal degree; that may be called *absolute chance;* but ordinary chance is merely relative to the causes that are taken into account.

The laws of the two kinds of chance are in the main the same. Speaking first of ordinary and relative chance, a man with an indefinite number of silver dollars who sits down to a perfectly fair game and bets one dollar on every throw of the dice will go on losing and winning in about equal measure. Speaking of absolute chance, the same thing will happen, for if not there would *ipso facto* be a definite tendency toward winning or losing. The only difference between the two cases is this, that the hypothesis of absolute chance is part and parcel of the hypothesis that everything is explicable, not absolutely, rigidly without the smallest inexactitude or sporadic exception, for that is a self-contradictory supposition but yet explicable in a general way. Explicability has no determinate & absolute limit. Everything being explicable, everything has been brought about; and consequently everything is subject to change and subject to chance. Now everything that can happen by chance, sometime or other will happen by chance. Chance will sometime bring about a change in every condition; or, at least, this is as near a correct statement of the matter as can readily be drawn up, for quite correct it certainly is not.

Now I propose to prove that the operation of chance will always present this phenomenon when the objects operated upon are very manifold.

A million players sit down to play a fair game. Each bets one dollar each time which he has an even chance of winning or losing. Let each player be provided at the outset with a pile of a million silver dollars. Now it is a curious & apparently paradoxical result that although everything is supposed to happen by pure chance yet we know very closely how those million players will stand at the end of a million bets. About 10 will have lost \$2000 or more, no one over \$3000; and half of them after playing day and night for nearly a fortnight at the rate of one bet a second will stand within \$300 of where they started.

But now we will suppose that the dice used by the players become worn down in the course of time. Chance changes everything & chance will change that. And we will suppose that they are worn down in such a way that every time a man wins, he has a slightly better chance of winning on subsequent trials. This will make little difference in the first million bets, but its ultimate effect would be to separate the players into two classes those who had gained and those who had lost with few or none who had neither gained nor lost and these classes would separate themselves more and more, faster and faster.

If on the other hand the wearing down of the dice were to have the opposite effect and were to tend to make him lose who had heretofore gained and *vice versa*, the tendency would be to prevent the separation of rich and poor. But chance will act in various ways. At one time it will have one effect at another time another.

If these effects were to be alternated after billions of trials, the effect would be to make numbers of distinct classes of players.

It would be easy if I had time to state the solutions to a number of similar problems in probabilities.

Suffice it to say that as everything is subject to change everything will change after a time by chance, and among these changeable circumstances will be the effects of changes on the probability of further change. And from this it follows that chance must act to move things in the long run from a state of homogeneity to a state of heterogeneity.

These are unlikely states of things. It is unlikely that a player should go on winning money billions of times and never be poorer than he began. But this is the effect of chance. Nor can you prevent it by killing the player whom you see taking such a course. You deprive chance of one means but it supplies another in the person of another player and the ultimate result is unaffected.

The operation of chance, therefore, does show a definite tendency to bring about unlikely events by varying means under varying circumstances.

I have no time to give more than a slight inkling of the consequences upon science and philosophy of attention to this principle.

You have all heard of the dissipation of energy. It is found that in all transformations of energy a part is converted into heat and heat is always tending to equalize its temperature. The consequence is that the energy of the universe is tending by virtue of its necessary laws toward a death of the universe in which there shall be no force but heat and the temperature everywhere the same. This is a truly astounding result, and the most materialistic the most anti-teleological conceivable.

We may say that we know enough of the forces at work in the universe to know that there is none that can counteract this tendency away from every definite end but death.

But although no force can counteract this tendency, chance may and will have the opposite influence. Force is in the long run dissipative; chance is in the long run concentrative. The dissipation of energy by the regular laws of nature is by those very laws accompanied by circumstances more and more favorable to its reconcentration by chance. There must therefore be a point at which the two tendencies are balanced and that is no doubt the actual condition of the whole universe at the present time.

Certain laws of nature, the laws of Boyle and Charles, the second law of thermodynamics, and some others are known to be results of chance,—statistical facts so to say. Molecules are so inconceivably numerous, their encounters so inconceivably frequent, that chance with them is omnipotent. I cannot help believing that more of the molecular laws—the principles of chemistry for example—will be found to involve the same element, especially as almost all these laws present the peculiarity of not being rigidly exact.

Now when we take into account that feature of chance which I have been bringing to your notice we find that this agent, although it can only work upon the basis of some law or uniformity, or more or less definite ratio towards a uniformity, has the property of being able to produce uniformities far more strict than those from which it works.

It is therefore possible to suppose that not only the laws of chemistry but the other known laws of matter are statistical results. Thomson supposes matter to consist of eddies in fluid. If a fluid is composed again of molecules its laws will be mainly due to chance. Now I will suppose that all known laws are due to chance and repose upon others far less rigid themselves due to chance and so on in an infinite regress, the further we go back the more indefinite being the nature of the laws, and in this way we see the possibility of an indefinite approximation toward a complete explanation of nature.

Chance is indeterminacy, is freedom. But the action of freedom issues in the strictest rule of law.

*[*Design and Chance (A)*]*

Epicurus makes the Gods consist of atoms but their superiority is due to the finer material of which they are composed. Thus, divineness comes from a special cause & does not originate by chance from elements not containing it.

Darwin's view is nearer to mine. Indeed, my opinion is only Darwinism analyzed, generalized, and brought into the realm of Ontology. But Darwin holds the development of Animals and Plants to be due to certain special characters, Reproduction, Spontaneous Variation, Heredity, etc.

Herbert Spencer and many other evolutionists hold that the operation of chance is an important factor in the development of self-consciousness. But they all admit other primordial elements, the conservation of energy and the like, to be necessary factors. Whereas my principle is that *[. . .]* holds a place in Nature independent of every accident of matter.

Before I can prove my proposition I must first show what it means. I must analyze the conception of *Design* or *Intelligence* and find what it consists in.

In the first place, then, to eliminate the element of *feeling* as being either no essential element of intelligence or at least only a subsidiary one. The internal sense, reflection, which makes us aware of what we think, is, in truth, the main thing which distinguishes us from the brutes. It is by this means that we control our thoughts, and conquer impulses which we do not approve. But because it happens to be thus valuable to us, because it happens to be the instrument by which we make ourselves rational, it does not follow that it is essential to rationality. What is essential is that all our cognitions should be gathered into a unity and that our actions should proceed from the entirety of our knowledge. Because our thought is only imperfectly brought to unity, it requires effort to collect it, and it requires a watchful eye to be directed to the imperfections of this unity. But were we so happily constituted that we should always without reflection completely assimilate everything we learned, so as to take due account of it in every act, we might well be spared the trouble of reflecting; and we should be only the more rational if we could thus behave with intelligence by the first intention of the mind, without reflection,—and knowing no more of what was going on in our minds than a healthy man does of what is going on in his stomach.

I have several times shown to my classes how some of the main laws of cerebration and particularly the formation of habits could be accounted for by the principles of probability, and I have shown by experiment how a certain regularity of arrangement can be impressed upon a pack of cards by imitating the action of habit.

The main element of habit is the tendency to repeat any action which has been performed before. It is a phenomenon at least coëxtensive with life, and it may cover a still wider real realm. Imagine a large number of systems in some of which there is a decided tendency toward doing again what has once been done, in others a tendency against doing what has once been done, in others elements having one tendency and elements having the other. Let us consider the effects of chance upon these different systems. To fix our ideas suppose players playing with dice, some of their dice are worn down in such a way that the act of losing tends to make them lose again, others in such a way that the act of losing tends to make them win. The latter will win or lose much more slowly, yet after a sufficient length of time they will be in danger of being ruined and if the game is quite even, they will eventually be ruined and destroyed. Those whose dice are so worn as to reproduce the same effects, will be divided into two parts, one of which will quickly be destroyed, the other made stronger and stronger. For every kind of an organism, system, form, or compound, there is an absolute limit to a weakening process. It ends in destruction; there is no limit to strength. The result is that chance in its action tends to destroy the weak & increase the average strength of the objects remaining. Systems or compounds which have bad habits are quickly destroyed, those which have no habits follow the same course; only those which have good habits tend to survive.

May not the laws of physics be habits gradually acquired by systems. Why, for instance, do the heavenly bodies tend to attract one another? Because in the long run bodies that repel or do not attract will get thrown out of the region of space leaving only the mutually attracting bodies. Why do they attract inversely as the square of the distance? This may be only their average law of attraction; we see how a comet throws away its repulsive material as it approaches the sun. But in the long run, matter that attracts inversely to a higher power of the distance tends perhaps to aggregate itself together, so that the masses of planets which have long been separate tend to attract in this manner.

[On the Teaching of Mathematics]

MS 503: Winter–Spring 1884

Feeling, learning, willing,—these are the three elements of our mental life. It is nothing but the familiar platonic or Kantian enumeration improved. The elements are here supposed to be exhibited in their purity. I say willing, not desire, because desire is a mixed affair containing plainly an element of feeling. Kant took desire from the Wolffian division of the faculties and omitted to make the necessary modification when he introduced the third element feeling. I also put learning in place of knowing. For if you will go through with the whole list of our different kinds of cognition, and from each one will strike out every constituent that belongs either to feeling or to willing, you will find that nothing is left to you except the power of learning. But many things besides cognition can be learned so that the element as I have defined it extends beyond the old limits in some places and falls within them in others. These three elements of psychical life correspond to three elements of physiological life: feeling to irritability, learning to the tendency to take habits which belongs to all tissue, willing to the power of a nerve-cell to discharge itself upon another cell whether the latter belongs to the nervous system or not. But what is most extraordinary, we find in pure logic a constant repetition of three elements that appear to correspond to these: namely quality to feeling, relation to will, and the bringing of terms into relation corresponding to learning. The reality and accuracy of the logical trio is absolutely demonstrated which the others of course are not.

The first thing from a pedagogic point of view or indeed from almost any point of view in considering the operations of any discipline, is to determine in what proportions these three elements enter into it. In mathematics, as it appears to me the largest constituent is the will. Mathematics above all other sciences requires the exercise of attention, and attention is a modification of the will. Above all other sciences it demands painstaking for accuracy. A mathematician ought to have every memoir carefully read by a second person to insure its correctness in every particular. I have known two good computers to spend the best part of a week in merely verifying the plus and minus signs of a single formula. Accuracy can only be secured in such a science by continual effort. One of the faults of which mathematicians are accused bears testimony to the same strong will,—I mean their bad temper. I am sure that I have known one mathematician of whom this accusation was eminently not true—whose temper never rose too high never sunk too low, but that was because he was strong enough to have perfect command of himself. There is no doubt that a hot temper has been a characteristic of a larger proportion of geometers. And this fault in the form in which it appears in them, not as mere petulance, not as mere fickle crossness, that has got out of bed the wrong way, but as an overbearing arrogance, is a character of a strong will. I will not say of mathematicians what I have often heard said of cooks, they are good for nothing if they are not bad tempered, a thing which is to be explained I suppose by the fact that an easy going cook will be always putting up with makeshifts, will be using onions when she ought to use leeks. Musicians are also irritable: and between mathematicians and musical composers there are a good many points of slight resemblance. That is very natural, for the delight of the art as of the science consists in great measure in the contemplation of complicated systems of relationship, and there is certainly more affinity between reading a memoir of Cauchy and intelligently listening to a fugue of Bach, than there is between the schoolboy's lesson in elementary geometry and a study in the higher mathematics. The strange thing, if there be anything strange in the matter, is not the points of agreement between the two classes of men, but that every composer is not a mathematician and every mathematician a composer. The ruling difference is that the musician has so much more feeling the mathematician so much more will. Their different ways of being out of temper betrays this difference. The truth is that according to the common idea, and I think it is right, there is no science in which the element of feeling enters less than into mathematics. It is supposed to have no empirical element. But this I am quite sure is a serious error. Mathematicians have as a general rule not delicate perceptions. They go through the world with their eyes turned inward. They are poor though they possess the most powerful engine of gain. They run naked through the streets crying "Eureka" oblivious of their surroundings. Their style of writing has enormous energy but little grace,—a military sort of style. As for the third mental constituent the power of learning, I do not think that any particularly large share of it is needed in mathematics or that an extremely small one would suffice. At any rate I am pretty confident I am right in saying that in mathematics the will needs to be excessively strong while susceptibility is hardly called for at all.

All these things ought clearly to be taken account of by the man whose office it is to teach mathematics, especially to average pupils. Such a teacher has a hard task indeed. The proportion of boys to whom mathematics comes easy is small, they may be said to be exceptional cases. The teacher has therefore a problem in psychology before him. What are the faculties he is to call into play? Into what state of mind is he to endeavor to put his pupil? These are questions requiring psychological study. But besides these, logical questions of extreme difficulty present themselves to him. What is the real nature of mathematical demonstration? If the teacher cannot answer that how can he expect to get the demonstration into the head of his pupil? When I see what he has to do and how little for the most part he understands how to do it, I exclaim it is not he who is teaching, it is the genius, the intuitions of the pupils which is relieving him of his duty, and he gets so accustomed to this that when he meets a boy who really does require teaching and not mere instruction, he loses his patience and seems to think that the boy is in some way at fault, when it is really the teacher who does not know what the logic of mathematics is. If the teacher feels the force of a demonstration but doesn't understand why, which happens in 999,999 cases out of a million, he cannot make another person feel the force of that demonstration who does not do so already.

The great algebraist to whom the success of this university is so largely due, has appended this note to his outline theory of reducible cyclodes. All this is certainly extremely instructive. Far be it from me to say that Old Sylvester is a cussed old twadler and don't know at all what he is talking about. Let us treat the old jack with all due respect by all means. This is very interesting especially Sylvester's own predilection for these kinds of methods, he is a very unusual instance of a mathematician ploughing into his subject experimentally. But he is certainly quite mistaken if he supposes as he appears to do that those who assert that the reasoning of mathematics is purely deductive, ever intended to advance the truly absurd proposition that you could not reason about mathematics inductively as well as about *[. . .]*

Notes

The functions of these notes, which are keyed to page and line numbers, are generally self-explanatory. The notes identify obscure proper names not found in the standard reference works for our edition or give the full name of a person to whom Peirce refers only by last name (when that name does not appear in the Bibliography) or by a descriptive phrase. Our standard reference works are the *Dictionary of American Biography,* the *Dictionary of National Biography,* the *Dictionary of Scientific Biography,* the 15th edition of the *Encyclopedia Britannica, The Encyclopedia of Philosophy,* the *National Cyclopedia of American Biography,* and the *New Century Cyclopedia of Names.*

The notes identify the source of quotations, direct and indirect, and paraphrases. Every effort is made to cite the editions Peirce is known to have owned or had available to him. When we cannot provide such information or when the edition he used was not available to us, we cite one that was accessible to him. Although quotations are allowed to stand as Peirce gives them even if they differ from the originals, such differences (both in substantives and accidentals) are explained. Unless otherwise indicated, all translations are by the editors.

Finally, the notes explain some difficult or obscure passages and identify some philosophical and scientific terms; provide the historical background for, as well as Peirce's own commentary and estimation of, some of his writings; quote the commentary and estimation of others; and give diplomatic transcriptions of some of Peirce's annotations in offprints, of some relevant manuscript pages not incorporated into the text, of correspondence relevant to his work, and of published summaries and abstracts of his work and that of others.

Citations are given in shortened form; complete bibliographic information appears in the Bibliography of Peirce's References. (For works referred to by the editors, full bibliographic information is provided in the notes.) References to the first three volumes of the present edition appear as *W*1, *W*2, or *W*3.

1.1 Read] Carveth Read (1848–1931), English philosopher and psychologist.

1.16 question of fact] See "The Fixation of Belief," *W*3:244.27–30.

1.24–2.1 What . . . etc.] Ibid., 246.14–19.
2.5–6 If . . . "quantitative,"] *Theory of Logic,* p. 24.
2.26–27 Turning . . . defined.] Ibid., p. 25.
2.35 figure] Ibid.
3.8–11 These are. . . .] Ibid.
3.12–13 Succession is. . . .] Ibid., p. 26.
4.4 "On Ghosts . . ."] For a published version, see item 10, pp. 50–67.
4.4–5 "Comparison. . . ."] For two related versions, see items 4 and 37, pp. 10–11 and 269–98.
4.20 Rutherfurd] Lewis Morris Rutherfurd.
5.2 Ste.-Claire Deville] Henri Etienne Sainte-Claire Deville.
5.2 Mascart] Eleuthère Elie Nicolas Mascart.
5.4 Newtonian rings] Concentric bright and dark rings produced by the interference of light reflected between a convex lens and a plane surface, investigated but never satisfactorily explained by Isaac Newton, who held the corpuscular theory of light.
5.37 instrument maker] Not identified.
7–9 This may be part of a lecture on "The Relations of Logic to Philosophy" that Peirce gave to the Harvard Philosophical Club on 21 May 1879. In response to it, Thomas Davidson wrote to William Torrey Harris on 5 June: "Peirce's paper was captious, bright, and poor. After it was over, I had a long talk with Prof. Ben. Peirce, who undertook to prove to me mathematically that space has four dimensions. The Peirce's are all a little crazy, I think."
7.4 In a paper] See "How to Make Our Ideas Clear," *W*3:257–76.
7.29–8.3 There are. . . .] See "Upon Logical Comprehension," *W*2:70–86.
8.5 the tree of Porphyry] A classificatory system based on the extensional interpretation of terms given by Porphyry in *Isagoge,* an introduction to the Aristotelian categories.
8.19 music of the spheres] Based on the Pythagorean idea that numerical relations underlie the harmony of the cosmos; see Plato, *Timaeus* 35C–36D.
9.7–8 Hegel. . . .] *Wissenschaft der Logik,* vol. 1. bk. 1, ch. 1, §§A–C.
10.5 Patterson] Carlile Pollock Patterson was Superintendent from 1874 to 1881.
10.9 Van der Willigen] Volkert Simon Maarten van der Willigen (1822–1878), Dutch mathematician.
10.27 This line is Kirchhoff 1200.6; see note 269.18–21.
11.14 Chapman] D. C. Chapman, Coast Survey employee.
12.6–8 At the . . . meeting] For information, including Peirce's contribution to the meeting (held 27 September–2 October), see *W*3:217–34.
13.26–27 I have shown. . . .] See the contribution in the preceding note.
19.34–20.4 Although Faye's proposal was rejected by Cellérier and Plantamour, the Faye-Peirce double pendulum method of reducing flexure was accepted and has been widely used in the 20th century.
21–37 Some of what appears in this at times unpolished manuscript is given, in more finished form, in item 19 below, pp. 163–209.

21.23–26 I have. . . .] See "Description of a Notation," *W*2:360n.1–11; see also Peirce's letter to Jevons, *W*2:446.5–23.

25.2–3 and was. . . .] See Schröder's "Nachschrift" (which is a postscript to a note on pp. 86–87 of the *Repertorium*) and Peirce's "On an Improvement in Boole's Calculus," *W*2:12–23.

25.9–13 See *W*2:13–14.

25.14 *et seq.* All of Schröder's formulae referred to in this manuscript are from his *Operationskreis.*

28.17 From the proofs that follow it can be made out that the missing text must be a section on being ($x \prec 1$) and nothing ($0 \prec x$), and that (41) must be $0 \mathbin{+\!\!,} x \prec x$ and (assuming that the emended formula in 29.3 is indeed $n\text{-}y \prec x, n\text{-}y \mathbin{+\!\!,} n\text{-}x$ so that, by (2), $n\text{-}y \prec n\text{-}x$ can be derived from [51] and [50]) (42) will have to be $1,x \prec x$.

38–46 The main argument of these two manuscripts appears, in more concise form, in section 1 of chapter 1 in item 19 below, pp. 163–65.

39.9 Murphy . . . 169.] The formulation reads: "The definition of habit, and its primary law, is that all vital actions tend to repeat themselves; or, if they are not such as can repeat themselves, they tend to become easier on repetition." Joseph John Murphy (1827–1894) was an Irish writer on religion, psychology, and logic.

42.26–28 It used to. . . .] The reference is to Hume's doctrine of the origin of ideas; see *An Inquiry Concerning Human Understanding,* p. 29.

48.4 Helmholtz] Hermann Ludwig von Helmholtz.

48.6–8 it is. . . .] Peirce was right in his estimation of Rood's book, for it soon became the authoritative text on chromatics and was translated into French, German, and Italian.

50–67 Peirce here analyzes a phenomenon familiar to users of diffraction spectroscopes, the repetition of principal spectral lines. In 1872 Georg H. Quincke had observed faint lines on both sides of the principal spectrum of every order produced by diffraction gratings (as opposed to prism spectra), which lines are called ghosts, or *Rowland ghosts* today. Peirce here calculates the expected positions of the lines and then compares them with their observed positions which, he explains, are caused by periodic irregularities in the effective spacing of the rulings, which are in turn caused by periodic irregularities in the threads of the micrometer screw in the ruling engine. The period of the irregularities determines the position of the ghosts, and the depth and angle of the furrow determines their brightness. Peirce's analysis of "ghosts" was an outgrowth of his efforts to establish wave-lengths of light as a standard of length.

54.12 Hirsch's . . . Tables] Not located. Adolph Hirsch (1830–1901) was a Swiss mathematician and physicist.

59.25–60.2 In a note entitled "Mutual Attraction of Spectral Lines" (*Nature* 21 [4 December 1879]: 108), Peirce explains the point of this short paragraph more fully:

> I do not know that it has been remarked that a line in the diffraction-spectrum (whether bright or dark) must be shifted from its normal position in case another line falls very near it. Neighbouring lines must be attracted if both are bright or both

dark, and repelled if one is bright and the other dark. The reason is that the lines are only maxima or minima of light, and the differential coefficient of the sum does not vanish at the same points as the differential coefficients of the separate terms. The shifting will be the greatest in the case of a faint line near a very intense one. I have succeeded in this way in shifting the positions of lines by measurable amounts (1″ to 2″).

60.5 Mayer] See his "Spectrum," 242–44.

68–71 With the south pole as the point of origin, a stereographic projection produces a disc of the northern hemisphere; by using an inverse elliptic function, Peirce mapped that disc onto a square. With the north pole as the point of origin, he mapped the southern hemisphere onto a square. He then cut the second square along both diagonals and placed the resulting four triangles on each side of the original square. The resulting projection is azimuthal, with the north pole represented at the center of the inside square and the south pole represented at each corner of the outside square. These five points form a quincuncial pattern as on the *five* face of a die and, in accord with the repeating elliptic function along both axes, the pattern is repeating. In a letter to C. P. Patterson dated 24 January 1877, Peirce indicated that he had discovered two conformals which bring the sphere into an oblong shape suitable for a page and that both involve elliptic integrals. One of the two conformals is the Quincuncial Projection described here; the other is Peirce's Skew Mercator Projection, described briefly in L 330 and Robin MS 1353. The rhumb lines on his Quincuncial Projection deviate from great circles less than those on the standard Mercator Projection, a fact that makes Peirce's map ideal for showing international air routes as straight lines. (See Coast Survey Chart A-5082 for an edition of the quincuncial projection and Chart 3092 for an overlay showing air routes.)

68.11 In his "Note on C. S. Peirce's Paper on 'A Quincuncial Projection of the Sphere'" (*American Journal of Mathematics* 18 [1896]: 145–52), James Pierpont argues that Peirce's formula is in error in that, rather than x or y equalling $\frac{1}{2}F\varphi$, x is equal to $\frac{1}{2}F\alpha$ and y equal to $\frac{1}{2}F\beta$, and

$$\cos\alpha = cn\ 2x = \frac{\cos^2 l - 2\sqrt{\sin^2 l + \frac{1}{4}\cos^4 l\ \sin^2 2\theta}}{2\sin l + \cos^2 l\ \cos 2\theta}$$

$$\cos\beta = cn\ 2y = \frac{2\sin l + \cos^2 l\ \cos 2\theta}{\cos^2 l + 2\sqrt{\sin^2 l + \frac{1}{4}\cos^4 l\ \sin^2 2\theta}}$$

72.16 $s = \frac{1}{r}$] Being the inverse of error, s thus measures reliability.

77.17–20 Given . . . ?] In a rejected section of MS 307, Peirce indicates that it is this application that first suggested to him the whole theory of the economy of research.

73.37 Wollaston] William Hyde Wollaston.

79–144 The first notable American contribution to gravity research, Peirce's "Measurements of Gravity" received special notice at the 1880 meetings of the International Geodetical Association in Munich and has since been regarded as one of the classics in the field of geodesy.

81.3–7 A source. . . .] See "On the Theory of Errors," *W*3:114–60.

81.34 Plantamour] Emile Plantamour (1815–1882), Swiss astronomer and director of the Geneva Observatory.

81.38 Dufour's map] Guillaume Henri Dufour (1787–1875), Swiss general and cartographer.

82.8 MM. Brunner] Brunner Brothers, instrument makers in Paris.

82.11 Wallon] Henri Alexandre Wallon.

82.24 Borda] Jean Charles Borda.

82.24–39 Quoted from a 9 July 1876 letter from Charles Joseph Etienne Wolf (1827–1918), French astronomer, to Peirce (L 475):

> Borda worked in a room and against a wall that no longer exist; the elevation of the floor of that room is 67 meters above sea level. The register of the office of longitudes gives 65 meters.
>
> I determined the position of Borda's wall on an old map of the observatory: its center was 14.72 toises (28.69 meters) east of the meridian-mark, and 9.89 toises (19.28 meters) south of the observatory well whose axis is on the meridian.
>
> The coordinates of Biot's station, relative to the same mark and the same well, are:
> distance to the meridian: 7.34 meters east;
> distance to the well parallel to the meridian: 10.23 meters south.
>
> You were yourself positioned nearly on the meridian, less than a meter east, and 12.70 meters north of the well.
>
> The elevation of the last two stations [no doubt the floor is meant] is 7.05 meters above that of Borda, and consequently 74.05 meters above sea level.

83.1–3 Biot gives . . . level.] Biot and Arago, *Recueil d'observations géodésiques,* p. 493.

83.3 De Freycinet gives. . . .] Freycinet, *Observations du Pendule,* p. 20.

83.12 Förster] Wilhelm Förster.

83.15 April 15 to June 12] In the report of Peirce's pendulum work in P 158, the dates of the Berlin experiments are 19 April to 6 June.

83.30 Whipple] George Mathews Whipple.

84.6 Bessel reversible pendulum] Friedrich Wilhelm Bessel, superintendent of the observatory at Königsberg, designed the reversible pendulum with two different points of suspension which was swung first from one point and then from the other, giving two sets of data. He employed it for the determination of gravity at Königsberg in 1825–1827.

84.8 Repsold] A. Repsold and Sons, a manufacturing firm in Hamburg.

84.9 Albrecht's experiments] The Repsold-Bessel pendulum was swung in Europe in 1869–1870 by Carl Theodor Albrecht under the direction of Karl Christian Bruhns, who published the results of the experiments in "Bestimmung der Länge."

86.34–35 Stackpole and Brothers] See 273.9-11.

88.19 Breguet] Maison Breguet (also Bréguet), French engineering firm established by the Swiss watchmaker Abraham Louis Breguet.

90.17 Weddle's rule] In Weddle's *A New, Simple, and General Method.*

93.3–5 a form of equation. . . .] For Coulomb's equation, which describes the electrostatic force between two charged particles, see *Analytic Mechanics,* p. 46; the exact formulation quoted has not been found.

96.26 The section beginning here exhibits the care Peirce took with the multitude of corrections required in pendulum work. With reversible pendulums it was not usual to reduce results to a vacuum, but Peirce saw the advantage in using relative determinations in order to determine the absolute value of gravity. He describes his theory in an 8 January 1878 letter to Superintendent Patterson.

100.6 du Buat] Pierre-Louis-Georges du Buat.

100.7 particular . . . Bessel.] Bessel, *Untersuchungen*, §31.

100.7–8 It . . . Green] Green, "Researches on the Vibrations," 56.

104.8 a fine . . . Stokes.] The reference is to Stokes's "On the Effect of the Internal Friction."

104.28–29 what . . . friction] Ibid., 13.

104.29 kinematical viscosity] Not located.

105.6 Graham] For a description of Thomas Graham's experiments on transpiration, see Maxwell's "On the Viscosity," 257.

105.6 Meyer] Oskar Emil Meyer (1834–1909), German physicist, the younger brother of the famous chemist Julius Lothar Meyer.

105.6 Springmühl] Not identified.

105.9 von Obermayer] Albert Edler von Obermayer, Austrian physicist of the second half of the 19th century.

105.10 Wiedemann] Gustav Heinrich Wiedemann.

105.10 Holman] Silas Whitcomb Holman (1856–1900), American physicist.

105.10 Puluj] Jan Puluj, Czech physicist of the second half of the 19th century.

105.27 The theory of Stokes] See note 104.28–29.

105.31 Kundt and. . . .] See Kundt and Warburg's "Ueber Reibung und Wärmeleitung."

107.11–15 we have. . . .] The figure in Wüllner is 0.001293606.

107.12 Regnault] Henri Victor Regnault.

108.3 which density. . . .] Bruhns, "Bestimmung der Länge," 113.

112.3 Green's formula] See note 100.7–8.

116.35–36 These. . . .] Henry Farquhar, Coast Survey employee and Peirce's occasional assistant.

121.13 Fizeau] Armand Hippolyte Louis Fizeau.

121.20 Wertheim] Wilhelm Wertheim (1815–1861), Austrian physicist.

122.23–30 the following . . . 213.2] The table is taken from a letter from Wilhelm Förster to Peirce, dated 29 March 1878, in which the second column has negative signs; the difference can be explained by the fact that Förster subtracted Meter No. 49 from the Platinum Meter, whereas Peirce did the reverse.

131.3 Newton's rings] See note 5.4.

132.13–17 Plantamour finds. . . .] Plantamour, "Recherches expérimentales," p. 51.

132.33–36 Plantamour has observed. . . .] Ibid.

133.12–13 The comparisons. . . .] See note 122.23–30.

133.13–21 Bruhns's Report. . . .] See note 84.9.

137n.1–2 See note 84.6.

144.27 The figure has not been found in Plantamour's "Recherches expérimentales."

145–47 Peirce is considering a random variable x having a binomial distribution with $n = s$ repetitions and with probability p of success; this has mean $\mu = sp$ and standard deviation $\sigma = \sqrt{sp(1-p)}$. The note shows how this distribution can be approximated by a normal distribution for reasonably large values of s. The quantity d is the value of a random variable D representing the deviation of $\frac{x}{s}$ from its expected value P; thus $D = \frac{x}{s} - P$. Since D has mean $\mu = 0$ and standard deviation $\sigma = \frac{\sqrt{sp(1-p)}}{s} = \sqrt{\frac{(p(1-p))}{s}}$, the random variable $Z = \frac{D}{\sigma} = \sqrt{\frac{s}{p(1-p)}}\, D$ will be approximately normal, if $s = 60$ for example. Hence

$$\text{Prob } (D \leq d) \approx \text{Prob } (Z \leq z) \text{ for } z = \sqrt{\frac{s}{p(1-p)}})\, d,$$

and the latter can be obtained from normal distribution tables. As Peirce mentions, practice demands that d be replaced by $d + \frac{1}{2s}$; this is still carried out, and corrects for the discontinuity of the histogramlike cumulative function of the binomial distribution. Thus z becomes $z = \sqrt{\frac{s}{p(1-p)}}\,(d + \frac{1}{2s})$, and

$$\text{Prob } (D \leq d) \text{ is approximately } \int_{-z}^{z} \frac{1}{\sqrt{2\pi}}\, e^{\frac{-u^2}{2}}\, du = \psi(z).$$

Peirce's treatment differs from the modern approach only in the presence of a 2 within the square root. In place of the standard normal variate Z, he uses the closely related

$$T = \sqrt{\frac{s}{2p(1-p)}}\, D = \frac{Z}{\sqrt{2}}$$

Thus Prob $(D \leq d)$ Prob $(T \leq t) = \int_{-t}^{t} \frac{1}{\sqrt{\pi}}\, e^{-v^2}\, dv$ where $t = \sqrt{\frac{s}{2p(1-p)}}\,(d + \frac{1}{2s})$. This integral is Peirce's θt, and is called the error function. Today statisticians prefer the integral $\psi(t)$ above, because of its direct tie with the standard normal distribution.

145.13 Here the $\frac{1}{2}$ is the continuity correction. If there are ten sixes, then $D = 0$. Thus d $= \frac{1}{120}$ is the value zero plus the correction term $\frac{1}{2s} = \frac{1}{120}$.

145.19 *[equation 2]*] Peirce appears to be using some approximation technique, when he writes $\sqrt{216} = \frac{30}{2}$. Since $\sqrt{216} = 6\sqrt{6}$, he is replacing $\sqrt{6}$ by $\frac{5}{2}$; certainly these are close, for squaring gives 6 and $\frac{25}{4} = 6\frac{1}{4}$.

148.5 Kater] See note 529.12.

148.8–13 The three values are taken from Biot and Arago, *Recueil d'observations géodésiques,* p. 587.

148.17 a correction. . . .] Bessel, *Untersuchungen,* §31.

149.19 Mouchez] Amédée Ernest Barthélémy Mouchez (1821–1892), French astronomer and director of the Paris National Observatory.

150.21–22 the experiments . . . ,] See Sabine's "On the Reduction to a Vacuum."

150.34–37 Förster has. . . .] See item 13, p. 122.23–30 and the accompanying note.

151.3–4 According to. . . .] See Barnard and Tresca, "A Report," p. 136, which gives 1.00000336 m as the length of the U.S. meter.

151.3 Tresca] Henri Edouard Tresca (1814–1885), French engineer and director of the Conservatoire Impérial des Arts et Métiers in Paris.

152–56 Peirce evidently prepared this lecture for delivery at a Fourth of July gathering of Americans in Paris in 1880. But the manuscript itself is the only evidence that such an event took place.

152.20–24 See Candolle, *Histoire des sciences,* pp. 158–247; a table of rankings, excluding Spain, is on p. 188.

154.17–20 I know. . . .] Peirce may well have had in mind the following passage from the inaugural address of Charles Eliot, Harvard's president from 1869 to 1909 (reprinted in Eliot's *Education Reform, Essays and Addresses* [New York: Century, 1898], p. 27):

> Experience teaches that the strongest and most devoted professors will contribute something to the patrimony of knowledge; or if they invent little themselves, they will do something toward defending, interpreting, or diffusing the contributions of others. Nevertheless, the prime business of American professors in this generation must be regular and assiduous class teaching. With the exception of the endowments of the Observatory, the University does not hold a single fund primarily intended to secure to men of learning the leisure and means to prosecute original researches.

155.3 Herschel] Sir William Herschel.

155.28–30 One discovery. . . .] According to the 1963 *Harper Encyclopedia of Science* (2:539), the 1879 discovery, which is now known as the Hall effect, is that a voltage develops "across a material carrying an electric current when a magnetic field is applied perpendicular to the current. This voltage, called the Hall voltage, is of interest because its sign indicates whether the current is carried by electrons (negative) or holes (positive); its magnitude is related to the number of these current carriers."

156.9 President Garfield] Peirce predicted correctly, for James Abram Garfield became the 20th U.S. President on 4 March 1881; but he died half a year later on 19 September.

157–60 The body of the letter, in English translation, reads as follows:

> I have sad news of my father's health which forces me to return to America immediately. I am therefore unable to participate in the Munich congress. May I ask you to read the following note for me at the meeting of the association and to send it to the secretaries for insertion in the Proceedings.
>
> The central bureau has asked for thoughts on the best apparatus for the use of the pendulum. For some time I have thought that, in order to determine absolute gravity, it would be best to swing long and short pendulums in a vacuum, and I have been very pleased to learn that your research has led you to the same results. But I prefer an invariable pendulum for determining relative gravity. The reason is quite simple: for by that process one needs to determine only one quantity instead of two; I might even say three. In effect, with the long and short pendulums, as with the reversible pendulum, one needs to determine two periods of oscillation and one length. But experience has shown me that the measurement of the length is, at stations in the country, often the most difficult part of the operation, because of the difficulty one experiences in trying to have, at the same time, constant temperature

and sufficient lighting. The objections to using the invariable pendulum are, I believe, the following:

1. it requires a study of atmospheric influences;
2. the comparison of invariable pendulums and the verification of their invariability are difficult.

But any means of avoiding the study of atmospheric influences implies observed combinations of quantities that multiply the errors. I believe, therefore, that the study of atmospheric influences is well worth the trouble one takes. Experience has similarly shown me that it is not in the least inconvenient to utilize the pneumatic chamber at stations in the country. Even if the apparatus does not function well at one station, the coëfficients of the atmospheric influences should already have been derived by experiments made at other stations. I would give the pendulum a regular cylindrical form so as to be able easily to calculate the effects of the pressure and viscosity of the atmosphere.

This is not, so far as I know, a trait that is peculiar to invariable pendulums, so that it is necessary to compare the two with each other; for the results obtained by means of the different instruments employed for obtaining absolute gravity must also be compared, and as everyone knows, they sometimes present considerable differences. The danger of accidents that might modify the invariable pendulums is a serious difficulty. But I avoid it by making my pendulum both invariable and reversible. Any alteration in the pendulum would be immediately revealed by the change in the difference of the two periods of oscillation in their two positions. Once discovered, it is immediately taken into account by means of new measurements of the distance between the supports. Perhaps it is necessary to take the measures at all ten stations. In short, it seems to me that if the reversible pendulum is perhaps not the best instrument for determining absolute gravity, it is, assuming at least that it is truly invariable, the best for determining relative gravity. I would like to have it formed of a brass tube drawn 0.03 meters in diameter with heavy brass bobs similarly drawn. The cylinder should end in two hemispheres; the knives should be attached to sleeves that are fixed close to the ends of the cylinder. The center of gravity must be five times more distant from one knife than from the other.

I shall offer some remarks on the specific details regarding the construction of a pendulum apparatus.

Mr. Villarceau and I have separately recommended that the pendulum be swung on cylinders of 0.005 meters in diameter. I have had several cylinders of this kind marvelously constructed, but they have thoroughly disappointed me. The pendulum suspended in such a way comes to a very quick stop, and the cylinder quickly loses its polish at the point of contact. My experiments have made me reject fully this idea that on first glance looked so attractive.

I continue to believe that the knife must play an integral part in the supports and that the plans must play a part in the pendulum.

All the modes I have tried in observing the period of oscillation leave something to be desired. I have made a great many experiments on the different ways of observing the coincidences, and the results have in all cases been unfavorable. In effect, one can observe a coincidence with precision only with the aid of more or less complicated arrangements that require the construction of new pillars. The momentary lighting of the pendulum by the electrical spark at each beat of the chronometer or clock is rarely convenient. The primary objection to the chronographic recording of the observed passes is that the reading of the signals requires considerable work. The automatic instrument constructed for me by Mr. Breguet has given me excellent results, and I would ordinarily employ it if I had a good Hipp chronoscope.

If I had been able to go to Munich, I would have requested that the association permit me to present a memoir on the flexure of stands.

This memoir takes account of numerous kinds of experiments on the static and dynamic flexure of different stands and on the periods of oscillation of the pendulums attached thereto. I discuss the proper means of examining the flexure, and

I show the superiority of the optical method. I demonstrate that the difference between the static flexure and the dynamic flexure is insignificant when the stands have been judiciously set up, and that the static flexure represents the effect produced on the duration of the oscillation as well as the dynamic flexure does; that it is a mistake to believe that the flexure is noticeably modified by the suspension of a medium or considerable weight. I describe the curious effects one obtains by tightening or loosening the nuts that hold together the pieces of the Repsold tripod and how the loosening can, under certain circumstances, diminish the effect of the flexure. There is a connection between this fact and the one I established in my first communication, namely that the flexure is not rectilinear, with the result that it differs noticeably for the parts of the top of the stands moved apart by a small number of millimeters. The results obtained without one's having taken this circumstance into account must be repeated.

It will be difficult to procure in the country a stand on which the flexure won't produce a noticeable effect. It would certainly be imprudent to claim that with a certain stand this effect is nil, without having tried to obtain some experimental proof thereof. A reversible Hardy pendulum might perhaps suffice for that, but it appears to me safer to measure the flexure.

I have for a long time been working on a study of the relative importance of the different sources of error in pendulum experiments.

Would you, I beg you, Sir, express to the association my gratitude for the friendly and eager cooperation I have encountered with all its members, and would you yourself please accept the assurance of my profoundest respects.

158.38 *Villarceau*] Yvon Villarceau (1813–1883), French engineer.

159.19 chronoscope de Hipp] Matthaeus Hipp (1813–1893), best known for his chronoscope measuring short intervals of time.

160.5 Hardy] Not identified.

161.3–6 If any. . . .] See "Note on the Sensation of Color," *W*3:211–16.

161.8–9 Rood's *Modern Chromatics*] For Peirce's review of the book, see item 9 above, pp. 47–49.

161.17–19 Burnham's lists . . . 467)] The lists of binary stars compiled by Sherburne Wesley Burnham are incorporated (as Tables I and II) in Edward Singleton Holden's "Note on a Relation," 469–72.

161.28–29 Ibid., 471–72.

163.23 all vital. . . .] See 39.7–9 and note 39.9

165.6 In the offprint in MS 366, Peirce has written following this paragraph, which ends at the bottom of the page:

Deductive logic perhaps does not involve the principle that there is any special character in the peripheral excitation but only that reasoning proceeds by habits that are consistent

Deductive Consistency of thought with itself

Inductive Consistency of the world (Uniformity of Nature.)

165.27–8 explain on any general principle] In the offprint in MS 367, Peirce has written in the margin: "'Explain on any general principle.' That comes to saying that if we can't form a *habit* which takes account of certain cases not occurring, it is the same as if they did not occur."

166n.1 See Boole, *Laws of Thought,* p. 119.

167.20 *nota notae*. . . .] The logical principle which, in Peirce's translation in Baldwin's *Dictionary of Philosophy and Psychology* (1901–1902), says that "The predicate of the predicate is the predicate of the subject."

Peirce traces the principle back to Aristotle, although the Latin phrase first occurred in Kant. Peirce's use of the principle in the argument that follows suggests "The mark of a mark is the mark of the thing itself."

169n.1–2 "On the . . . ,"] See *W*2:23–48.

169n.4 "Logic of Relatives,"] See *W*2:359–429.

170.4–8 De Morgan. . . .] See De Morgan's "On the Structure of the Syllogism," the first in a series of five papers on syllogisms presented before the Cambridge Philosophical Society and subsequently published in its *Transactions.* Throughout this item, Peirce gives the dates of presentation rather than publication of De Morgan's papers.

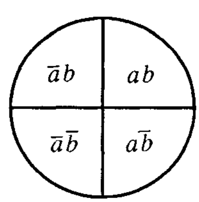
170.9 *universe* of discourse] De Morgan introduced the terms "universe" and "universe of a proposition" on p. 380 of his first paper; see also his *Formal Logic,* pp. 40–41, 55. The term "universe of discourse" was first used in Boole's *Laws of Thought.*

171.1–3 Boole. . . .] *Laws of Thought,* p. 63; Boole's example is "Some men are not wise."

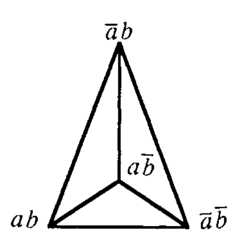
171n.1–3 Jevons's attempt. . . ."] The relevant passage in Jevons's *Principles of Science* reads: "We can always substitute for it more definite expressions if we like" (p. 49).

173.4–5 This has. . . .] The reference is to William Hamilton's theory of quantification of the predicate, in appendix V of his *Lectures on Logic.*

173.6–8 Peirce and his students sought to complete the set of propositional forms. In the offprint in MS 369, he wrote "15 states of the universe" and "1438907-16 states *of knowledge,*" and he discussed the matter in his lectures in the fall of 1880 (according to lecture notes taken by Allan Marquand, one of his students). Peirce used a diagram similar to the one on p. 172, but he modified it to show the four variations that two objects (or terms) can take with respect to existence (or truth-values):



Peirce suggested that the relations among the four alternatives be represented by a triangular pyramid:



Peirce appears to suggest that the universe might be such that it is represented by the pairs at any one of the four corners of the pyramid (four states of the universe); by the combination of pairs at any two corners (six states of the universe); by the combination of pairs at any three corners (four states); or by the combination of pairs at all four corners of the pyramid (one state). This yields fifteen possible states of the universe with respect to two objects (or terms). Peirce does not mention a sixteenth state of the universe in which none of the combinations obtains.

In MS 370, Christine Ladd's class notes with a number of Peirce annotations, she developed elaborate diagrams of possible universes with respect to specified objects and carefully analyzed how the space of a universe can be divided into regions containing the four possible combinations of two objects (or terms) with respect to existence (or truth-values). If space is divided into four regions, we have (referring to Peirce's diagram above) one possible state of the universe in which (A&B), (A&$\bar{B}$), ($\bar{A}$&B), and ($\bar{A}$&$\bar{B}$) can co-exist. If space is divided into three regions, there are four possible states: (A&B), (A&$\bar{B}$), ($\bar{A}$&B); (A&B), (A&$\bar{B}$), ($\bar{A}$&$\bar{B}$); (A&B), ($\bar{A}$&B), ($\bar{A}$&$\bar{B}$); and (A&$\bar{B}$), ($\bar{A}$&B), ($\bar{A}$&$\bar{B}$). If space is divided into two regions there are six states: (A&B), (A&$\bar{B}$); (A&B), ($\bar{A}$&B); (A&B), ($\bar{A}$&$\bar{B}$); (A&$\bar{B}$), ($\bar{A}$&B); (A&$\bar{B}$), ($\bar{A}$&$\bar{B}$); and ($\bar{A}$&B), ($\bar{A}$&$\bar{B}$). Finally, if space has only one region, there are four states: (A&B); (A&$\bar{B}$); ($\bar{A}$&B); and ($\bar{A}$&$\bar{B}$). Altogether, then, according to Ladd, there are fifteen possible states of the universe:

These considerations have a geometrical air. The same result may be obtained analytically.—By the principle of contradiction, A, B, $\bar{A}$, $\bar{B}$ can coexist not more than two at a time. By the principle of excluded middle they must coexist as much as two at a time. The number of possible coexistences of four things two at a time is four. Of these four coexistences the number of possible combinations taken, one, two, three, and four at a time is fifteen.

A complete proposition is one which completely describes the construction of space with respect to the two terms which it contains. Fifteen (mutually exclusive) constructions of space are possible. There are therefore fifteen distinct forms of the complete proposition.

Ladd summarized her results in the following table:

$0 \prec 0$	1	1	$\infty \prec \infty$
$0 \prec A$	8	9	$\infty \prec A$
$0 \prec B$	10	11	$\infty \prec B$
$0 \prec \bar{A}B$	5	15	$\infty \prec AB$
$0 \prec \bar{A}\,\bar{B}$	4	14	$\infty \prec \bar{A}\,\bar{B}$
$0 \prec A\,\bar{B}$	3	13	$\infty \prec A\,\bar{B}$
$0 \prec \bar{A}\,B$	2	12	$\infty \prec \bar{A}\,B$
$0 \prec AB + \bar{A}\,\bar{B}$	7	6	$\infty \prec AB + \bar{A}\,\bar{B}$

She included a sixteenth place in her table (where instead of $0 \prec 0$ perhaps she should have put $0 \prec \infty$ for the state of the universe in which nothing exists, or where nothing is true), but she did not yet fully recognize this as a distinct state. Yet later in her class notes we see that she had overcome her reluctance to imagine an empty universe: "I see now that it is not only permissible but actually essential to symmetry and completeness. . . ." Her finished paper for *Studies in Logic* includes the following table of "the sixteen possible combinations of the universe with respect to two terms":

$\bar{a}\bar{b}$ 8	$a\bar{b}$ 4	$\bar{a}b$ 2	ab 1	
0	0	0	0	0
0	0	0	1	1
0	0	1	0	2
0	0	1	1	3
0	1	0	0	4
0	1	0	1	5
0	1	1	0	6
0	1	1	1	7
1	0	0	0	8
1	0	0	1	9
1	0	1	0	10
1	0	1	1	11
1	1	0	0	12
1	1	0	1	13
1	1	1	0	14
1	1	1	1	15

174.5–6 *dictum de omni*] The *dictum de omni et nullo,* which states that whatever can be said of a class can be said of its members, is the principle on which the classical theory of syllogism is based. Though formulated by the scholastics, it is usually attributed to Aristotle.

174n.11–14 De Morgan. . . .] In De Morgan's paper, the words "elimination" and "inference" are italicized.

176.17 Peirce numbered this formula ($13\frac{1}{2}$).

176.33–177.2 In the offprint in MS 366, Peirce wrote in the right margin: "(16) is deducible from and asserts less than ($13\frac{1}{2}$). Now from (16), can be deduced (14) and (15) and it is unnecessary for that purpose to admit ($13\frac{1}{2}$)."

177.16 *principle of contradiction*] In MS 366, Peirce added the following note: "The principle of contradiction is deducible from ($13\frac{1}{2}$) but not from (16). Hence a modification of the text is necessary."

177.32 the principle . . . ,] In MS 366, Peirce changed $A \prec x$ to "(16)."

178.7 The principle . . . (19)] According to Quine (*Isis* 22 [1934]:296), Peirce's form (19) more precisely represents the principle of contraposition.

178n.1–6 See item 22, p. 214.7–14. According to Arthur N. Prior, Peirce is mistaken in his claim that (20) and (21) follow from $\bar{A} = A \prec x$ ("The Algebra of the Copula," in Edward C. Moore and Richard S. Robin, eds., *Studies in the Philosophy of Charles Sanders Peirce* [Amherst: University of Massachusetts Press, 1964], p. 88).

180.21 In MS 366, Peirce noted that "Formula (3) has the same relation to (14) of Chapter II that (3′) has to (34)."

182n.1–2 See Boole's *Laws of Thought,* p. 47, and Peirce's "Description of a Notation," *W*2:386.

182n.14–16 "On an . . . ,"] See *W*2:12–23.

182n.16–17 In another paper. . . .] See "Description of a Notation," *W*2:360.

182n.18 brother of . . . ,] The reference is to Hermann Günther Grassmann, also a noted mathematician.

182n.20–23 The very notation. . . .] Robert Grassmann, *Begriffslehre,* pp. 15–16.

182n.34 several papers] McColl's seven papers on the subject appeared in the *Proceedings of the London Mathematical Society* between 1877 and 1898.

183n.2 "Logic. . . .] See "Description of a Notation," *W*2:389, formula (94); another of the four formulae appears there as (21).

184.3 the proof. . . .] In 1867 Peirce had published a diagrammatic proof of the law of distribution for class logic (*W*2:13–14) and in 1879 he succeeded in producing (in item 6, pp. 33–34; see also pp. 27–28) a proof from a logical basis similar to the one here. But when, soon after the publication of the present item, he was challenged by Schröder to produce his proof, he found that he could not do so and concluded that he had been mistaken. The editors of the *Collected Papers* (CP3.200n) report that Peirce wrote in the margin of an offprint: "It seems that $(a+b) \times c \prec (a \times c)+(b \times c)$ cannot be proved from the definitions. The propositions L are needed." In a footnote to P 296 (CP3.384n) Peirce conceded publicly to Schröder's claim that half of the law of distribution was independent in a system based on purely syllogistic principles. But Peirce never fully yielded. By 1903, in his second Harvard lecture on pragmatism (Robin MSS 302 and 303) he retook his ground:

> Of this mathematics under its original limitations confining its applicability *[to]* non-relative logic no masterly presentation has ever been given. The nearest approach to such a thing in print is contained in the first two chapters of my paper in Vol III of the *Am. Jour. Math.;* and I may mention that Schröder's criticism of my definitions of aggregation and composition there given, although at first I assented to it, is all wrong and that the demonstration which Schröder professes to demonstrate cannot exist does exist and is perfect.

At the beginning of 1904 Peirce was in correspondence with Edward V. Huntington about Huntington's soon-to-be-published "Sets of Independent Postulates for the Algebra of Logic" (*Transactions of the American Mathematical Society*, vol. 5) and sent him his recently discovered proof of the law of distribution. Huntington answered on 7 February: "There is no doubt in my mind that the demonstration is perfectly complete, if one grants the necessary assumptions from the algebra of the copula." Peirce replied a week later with a letter that Huntington published as a footnote to Peirce's proof as it appeared in his article:

> Dear Mr. Huntington: Should you decide to print the proof of the distributive principle (and this would not only relieve me from a long procrastinated duty, but would have a certain value for exact logic, as removing the eclipse under which the method of developing the subject followed in my paper in vol. 3 of the American Journal of Mathematics has been obscured) I should feel that it was incumbent upon me, in decency, to explain its having been so long withheld. The truth is that the paper aforesaid was written during leisure hours gained to me by my being shut up with a severe influenza. In writing it, I omitted the proof, as there said, because it was 'too tedious' and because it seemed to me very obvious. Nevertheless, when Dr. Schröder questioned its possibility, I found myself unable to reproduce it, and so concluded that it was to be added to the list of blunders, due to the grippe, with which that paper abounds,—a conclusion that was strengthened when Schröder thought he demonstrated the indemonstrability of the law of distributiveness. (I must confess that I never carefully examined his proof, having my table loaded with logical books for the perusal of which life was not long enough.) It was not until many years afterwards that, looking over my papers of 1880 for a different purpose, I stumbled upon this proof written out in full for the press, though it was eventually cut out, and, at first, I was inclined to think that it employed the principle that *all* existence is individual, which my method, in the paper in question, did not permit me to employ at that state. I venture to opine that it fully vindicates my characterization of it as 'tedious'. But this is how I have a new apology to make to exact logicians.

For a full account of these matters, see Nathan Houser, "Peirce's Algebra of Logic and the Law of Distribution," Diss. Waterloo 1985.

184.13 This proposition. . . .] See Schröder's *Operationskreis* and Robert Grassmann's *Begriffslehre*, p. 13.

184n.1–2 See Boole's *Laws of Thought*, p. 33; Jevons's *Pure Logic*, p. 26; and Peirce's "On an Improvement in Boole's Calculus," *W*2:13.

186.21 $b = c = 0 \ldots a = d = \infty$] According to Quine (*Isis* 22 [1934]:297), Peirce here made a technical slip that may be corrected by reading "$b = \infty \quad c = 0$" for "$b = c = 0$" and "$a = \infty \quad d = 0$" for "$a = d = \infty$."

187.8 The first. . . .] Boole, *Laws of Thought*, p. 72.

187.8–9 I showed. . . .] See "On an Improvement in Boole's Calculus," *W*2:16.

187.19 Boole gave. . . .] Boole, *Laws of Thought*, p. 103; Peirce, "On an Improvement in Boole's Calculus," *W*2:17.

190.29 their $\left\{ \begin{matrix} \text{product} \\ \text{sum} \end{matrix} \right\}$] In MS 366, Peirce replaced "their" with "a propositions have the," and he completed the sentence by adding "of their $\left\{ \begin{matrix} \text{predicates} \\ \text{subjects} \end{matrix} \right\}$." He probably intended to say "a proposition having."

190.30–31 a well-known. . . .] Boole, *Laws of Thought*, p. 147.

193.27–194.3 The germ . . . a fact.] See Kant's *Kritik*, A659–60, B687–88.

In 193.31–34, Peirce symbolizes the following sentence from the *Kritik* (in Norman Kemp Smith's translation): "In short, there are no species or subspecies which (in the view of reason) are the nearest possible to each other; still other intermediate species are always possible, the difference of which from each of the former is always smaller than the difference between these."

195n.1–2 See "Description of a Notation," *W*2:418.

202.23–25 According to Quine (*Isis* 22 [1934]:297), the fourth entry is fallacious but can be made valid by saying: "Changing the first and second marks from | to ˘ or from – to ≚ or conversely and then interchanging the two resulting marks."

204n.2–3 (except . . . operation)] Robert Grassmann, *Begriffslehre,* p. 41.

204n.3 "Logic of Relatives"] See "Description of a Notation," *W*2:361–62.

204n.4 A similar. . . .] De Morgan, "On the Syllogism, No. IV," 343.

208.37 *To be Continued*] Although Peirce worked on a continuation (see items 20 and 22, as well as Robin MSS 527, 547, and 747) he never completed it. His next major publication on the algebra of logic appeared in the *American Journal of Mathematics* in 1885 (P 296), which will be published in *W*5.

209.1–5 The exact reference is 208.17–209.17. The editors of the *Collected Papers* remark (CP3.227n) that this correction was probably made after Peirce had received proofs and was therefore published at the end of the paper. They reproduce it as a footnote keyed to the end of the sentence ending on 199.12, although it might be more appropriately keyed to the next sentence. In the absence of certainty, the present editors have followed the original publication.

210.5 In the last. . . .] The reference is to the immediately preceding item.

211n.1–2 This is . . . Categories."] See *W*2:359–429 and 49–59.

212–213 In essence a summary of the results of item 13 above, this short paper is, according to Victor Lenzen (*Transactions* 8 [1972]: 92), the first published record of Peirce's methods in mathematical geodesy.

212.4–23 The following . . . not important.] See Clarke, *Geodesy;* the figures for New York, Paris, and Kew appear on p. 349, the value of ellipticity on p. 348.

212.24–213.2 The five page numbers are those of the original publication of item 13 above. Page 416 is not included, but pp. 204 and 318–20 are here pp. 84 and 140–43, respectively.

214.4 a second paper] The first paper is item 19 above, pp. 163–209.

214.7–11 See item 19, pp. 176.33–178.1.

214.12–13 In my circular. . . .] The reference is to MS 368, a one-page letter of corrections dated 15 September 1880, which Peirce had printed for circulation with item 19.

215.3 with Cayley] See Cayley's "Note on the Calculus of Logic," 282.

216.7–9 Opposite these lines, on an otherwise blank page, Peirce wrote:

$$\psi = x - \varphi = \frac{-lu + (x,y) + (k-i)x - (k-l)y}{-i + j + k - l}$$

216.25–27 Opposite these lines, on an otherwise blank page, Peirce wrote:

Same thing other form expresses by

$$-lu + (x,y) + (k - i)x - (k - l)y = 0$$

Negative $-lu + (x,y) + (k - i)x - (k - l)y > 0$

216.28–29 See note 215.3.

217.1–3 See "Description of a Notation," *W*2:367; instead of the symbol $>$ Peirce then used $<$. ("p 7" refers to the 1870 reprint by Welch, Bigelow and Company, Cambridge.)

217.23 De Morgans . . . proposition] De Morgan, "On the Structure of the Syllogism," 381.

218–21 This paper is the first known anticipation of the stroke function described by H. M. Sheffer in "A Set of Five Independent Postulates for Boolean Algebras, with application to logical constants," *Transactions of the American Mathematical Society* 14 (1913): 481–88.

218.12–14 The apparatus . . . ;] Boole, *Laws of Thought,* p. 27.

222–24 The axiomatization of natural numbers given here is nearly equivalent to that given in item 38 (see also MSS 393 and 394), but there are differences which strongly suggest that the present item was composed earlier. The significant differences rest chiefly upon the formulation of mathematical induction (axiom VIII in the present item) and upon differing definitions of addition and multiplication in the two papers.

For comparison, the axioms of item 38 provide (1) partial order, (2) connection, (3) closure with respect to predecessors, (4) a minimum but no maximum, and (5) mathematical induction.

Let R be the relation "greater than" and let N be the set of "positive discrete numbers." The axioms of item 24 may then be expressed:

I. R is irreflexive (alio-relative) $(x)\,(-x\mathrm{R}x)$
II. R is transitive $(x)(y)(z)(x\mathrm{R}y\ \&\ y\mathrm{R}z \rightarrow x\mathrm{R}z)$
III. $(x)(y)(x\epsilon\mathrm{N}\ \&\ y\mathrm{R}x \rightarrow y\epsilon\mathrm{N})$
IV. R connects N $(x)(y)(x\epsilon\mathrm{N}\ \&\, x \neq y \rightarrow x\mathrm{R}y\ \mathrm{V}\ y\mathrm{R}x)$
V. N has no maximum $(x)(\mathrm{E}y)(x\epsilon\mathrm{N} \rightarrow y\mathrm{R}x)$
VI. "Unity" is a minimum in N $1\epsilon\mathrm{N}\ \&\ (x)\,(-1\mathrm{R}x)$
VII. N is closed with respect to predecessors
$(w)(x)(\mathrm{E}y)(z)(x\epsilon\mathrm{N}\ \&\ w\mathrm{R}x \rightarrow y\epsilon\mathrm{N}\,\&\,w\mathrm{R}y\,\&\,(w\mathrm{R}z \rightarrow\ -z\mathrm{R}y))$
VIII. Number is singly infinite: any number can be reached by successive minimum steps. More precisely, if every number greater than but not greater than a number greater than another is in any transitive relation to that other, then every number greater than another is in the same transitive relation to that other.

I–VII parallel the first four axioms from item 38. I and II insure a (strict instead of weak) partial ordering, III confines R somewhat and is largely cosmetic, and IV–VII match up with 2–4.

Peirce intended VIII to do the work of mathematical induction, i.e., to guarantee that "any number can be reached by successive minimum steps." A generous rendition of VIII might be as follows:

VIII* For every transitive relation T, if T obtains between every number and its immediate successor then T must obtain between every number and all greater numbers.

If this is what Peirce intended, then I–VIII might well comprise an adequate axiom system for the natural numbers. When the transitive relation T is interpreted simply as indicating conditional membership in a given class, VIII* appears to reduce to the usual formulation of mathematical induction.

Nonetheless, even if VIII* were to prove an equivalent alternative, it would still be more cumbersome to work with, and more conceptually opaque, than the formulation in terms of propositions and classes given in item 38.

225.6 $\sqrt{-}$] Here this may be an abbreviation for $\sqrt{-1}$, which Peirce calls a unit vector at 233.13; but it may be intended more generally as a sign of any quantity whose square is negative, which Peirce calls simply a vector at 233.12. (See B. Peirce's *Linear Asociative Algebra,* Addendum 1, on $\sqrt{-1}$ as a non-independent symbol of inversion.)

225.11 The first. . . .] Peirce uses the definition given by his father: "when an expression raised to the square or any higher power vanishes, it may be called *nilpotent;* but when, raised to a square or higher power, it gives itself as the result, it may be called *idempotent*" (p. 8).

225.6–7 My father . . . algebra] All references in this item are to Benjamin Peirce's *Linear Associative Algebra.*

228.14–23 My father . . . number.] Ibid., pp. 119–215.

229.2 Peirce's b_2.] The multiplication table for b_2 appears on p. 121.

229n.1–3 See "On the Application of Logical Analysis," *W*3:177–79.

230.12 Cauchy] Augustin Louis Cauchy.

231.9–10 See Clifford's "Preliminary Sketch of Biquaternions."

231.16 Grassmann's . . . extension] See Hermann Grassmann's "Die Mechanik."

231n.3 See "Note on Grassmann's Calculus," *W*3:238–39.

238.13–16 "It seems. . . ."] Sylvester as quoted in Jevons, p. xi, except that Peirce has corrected the spelling of "heuristic."

238.17 "To such. . . ."] Ibid.

240 Peirce suggests here that the line at 5624825 be adopted as a standard reference line. Although Henry Augustus Rowland corrected it to 5624660, he said when he was awarded the Rumford medal by the American Academy of Arts and Sciences in February 1888: "My map of wavelengths is based upon Professor Charles S. Peirce's measurement of a line in the green portion of the spectrum" (Victor Lenzen, *Transactions* 11 [1975]: 161). In a letter fragment (L 41a) Peirce gives the value as $0.^{m}5624860$.

240.3 the superintendent] See note 10.5.

240.24 Ångström. . . .] *Recherches sur le spectre solaire,* p. 31.

241.26 Hegel . . . ;] The reference is to Hegel's *Wissenschaft der Logik*, vol. 2, bk. 2, ch. 2, §C, especially Observations 2 and 3.

249.4–5 Juan Perez de Manchena] Peirce has conflated the names of two friars, Antonio de Marchena and Juan Pérez, who were instrumental in getting Columbus the support of Isabella.

249.8–9 Sir Philip Francis was credited with the *Letters of Junius*, a political pamphlet noted for style (and sometimes attributed to Edmund Burke, Edward Gibbon, or Thomas Paine).

251.27–31 "Nota. . . ."] See note 167.20.

251.32–34 For a definition close to Peirce's formulation, see De Morgan's "On the Symbols of Logic," 104.

256.9 certain modes. . . .] Ibid., 100–101.

257 In Cayley's "A Problem in Permutations," the mouse-trap problem is stated as follows:

> A game called Mousetrap gives rise to a singular problem in permutations. A set of cards, ace, two, three, etc., say up to thirteen, are arranged in a circle with their faces upwards-you begin at any card, and count one, two, three, etc., and if upon counting suppose the number five, you arrive at the card five, that card is thrown out; and beginning again with the next card, you count one, two, three, etc., throwing out if the case happens the new card as before, and so on until you have counted up to thirteen, without coming to a card which ought to be thrown out. It is easy to see that, whatever number of the cards is, they may be so arranged as to be all thrown out in the order of their numbers; but that it is not possible in general to arrange the cards so that all the cards, or any specified cards, may be thrown out in a given order.

258.3–4 Dr. Franklin. . . .] See his "Note on Indeterminate Exponential Forms."

258.27 Neperian] The reference is to John Napier (or Neper).

259.15–16 Riemann's surface] The reference is to Georg Friedrich Bernhard Riemann.

265.15 Boole takes. . . .] *Laws of Thought*, pp. 47–48.

268.3 Fermatian inference] Today known as mathematical induction.

269–98 In a very brief and incomplete earlier version of this report (MS 338), L. M. Rutherfurd is listed as co-author. In another, fuller version (MS 340), the second paragraph reads as follows:

> The work may be regarded as a natural sequel to the long labours of Mr. Rutherfurd which have produced the finest ruling-machine yet constructed, and most of it has been done in his laboratory with the aid of his trained assistant Mr. Chapman. In beginning it, I expected to work in collaboration with Mr. Rutherfurd; but circumstances have unhappily prevented his following the work very closely, although I have in several critical emergencies profited from his invaluable advice.

269.18–21 The result of the first part of the whole operation was that the line Kirchhoff 1200.6 (5623.44Å) has a double deviation of 90°03′52″ when observed in the second order. Peirce's method was called the method of minimum deviation. According to the theory of the diffraction-grating, the relation between the wave-length of the line being observed (λ), the grating space (b), and the angle of deviation (θ), is $n\lambda = 2\ b\frac{\sin - \theta}{2}$ where n is the order of the spectrum in which the line is being observed. If we take the grating space b to be $\frac{1}{6809}$, this gives

$$2\lambda = 2\left(\frac{1}{6809}\right)\ \frac{\sin 45°01'56''}{2}$$

$$\lambda = \frac{0.3829}{6809}$$

$$\lambda = 5623.4\text{Å}$$

The second part contains some of the numerous comparisons Peirce made in his effort to build up the meter from a basic unit of about 7 lines of the Rutherfurd gratings. His comparator could not accommodate lengths longer than a decimeter, so that his comparisons of a decimeter with a meter are very rough. His general result seems to be that a centimeter is about 6808 lines, which gives for the principal line

$$\lambda = \left(\frac{1}{6808}\right)\ (0.3829)$$

$$\lambda = 5624.2\text{Å}$$

271n.1–2 See item 10 above, pp. 50–67.
273.15 Byrne] Instrument maker; not further identified.
273.35 Alvan Clark] Instrument maker; not further identified.
274.28 Casella . . . theodolite] A surveying instrument for measuring horizontal angles upon a graduated circle, by Louis P. Casella, maker of portable instruments.
276.20 Schneider] Not identified.
276.21 C. S. P. . . . E. D. P.] Charles S. Peirce and E. D. Preston, a Coast Survey employee.
276.28 Baudin] Not identified.
279.28 Kirchhoff] Gustav Robert Kirchhoff.
279.30 V. der W.] van der Willigen; see note 10.9.
280.1 a. c. a.] An unidentified Coast Survey employee.
283.28 Wales] Not identified.
297.14 Fizeau's apparatus] An apparatus for measuring the velocity of light in water and air, constructed by Hippolyte Louis Fizeau.
297.16 my Report. . . .] See item 13 above, pp. 79–144.
297.19 Office of . . . ,] The U.S. Office of Weights and Measures was attached to the Coast Survey as a department in 1832; it became the National Bureau of Standards in 1901.
298.20 Schott] Charles Anthony Schott, a Coast Survey assistant.
298.25 An undated thirteen-page letter (L 41a), possibly to Louis Bell, with

its first page missing, gives a summary report of Peirce's spectrum metre findings. It concludes as follows:

> The coefficient of expansion of No 49 is $18.^{\mu}82$ per degree C (See Measurements of Gravity at Initial Stations p 74) making $397.^{\mu}29$ for $21.^{\circ}11$. The metre No 49 was assumed originally to be $5.^{\mu}2$ too short at 0°C; but afterward (Pendulum experiments p 107) it was found that the assumed metre was $16.^{\mu}0$ too short; so that No 49 is really $21.^{\mu}2$ too short at zero or $376.^{m}1$ long at 70°F; so that glass metre No 1 was $371.^{m}6$ too long at that temperature.
>
> This makes the Decimetre No 3 too long by $+27.^{\mu}65$ and the errors of the four gitters to be

No 1	$+3.^{\mu}084$
$\frac{1}{2}$ H	+2.845
$\frac{1}{2}$ I	+3.909
F	+3.090

> The deduced wave-length of the principal line according to gitter H is then at 30 in and 70°F
>
> $0.^{\mu}5624860$
>
> Almost every part of the work is confirmed by other processes not herein described.
>
> To suppose Angstrom's value right would be to suppose such errors at the following in my work

1′30″	in the double deviation
28 in.	in reading the temperature of the Gitter
5^{μ}	in the breadth of Gitter H
27^{μ}	in the length of Decimeter No 3
267^{μ}	in the length of the metre
4	in the number of spaces.

> I believe my result is correct to $2\frac{1}{2}$ millionths.

299–309 According to Quine (*Isis* 22 [1934]: 294), this paper "contains what is still probably the best known systematization of the arithmetic of positive integers."

299.7–9 so that. . . .] The reference is to J. S. Mill's interpretation of truths of mathematics as generalizations from experience; see his *System of Logic,* bk. 2, ch. 6, §2.

299.9–11 The object. . . .] In Marquand's notes on Peirce's advanced logic course in the fall of 1880 (Marquand Papers, Princeton University Libraries), Peirce has written:

> A syllogism is said by some to be a petitio principii if considered as an inference. Why? Because the conclusion is "implied in the premisses." But to say that A is implied in B means simply that whenever B were true A would in all logically possible circumstances be true. Hence the objection amounts to this, that when the premises are true the conclusion is true also. This is no imperfection because if it were otherwise, *then* indeed the inference would not be absolutely valid.
>
> But it is said that whoever knows the premises also already knows the conclusion. Not so. The Theory of numbers is purely syllogistic yet it is a most mysterious & surprizing branch of mathematics.

300.9–22 MS 394, a brief fragment consisting of two pages numbered 15 and 16, may be an earlier version of this section; it reads as follows:

Continuous, simple quantity

This kind of quantity, to be subject to mathematics, needs to be capable of measurement. That is, the *interval* or difference from any one quantity to any second must be capable of being comparable as to being greater or less or equal with that from any third to any fourth.

The mode of measurement may be elliptic, hyperbolic, or continuous. (Klein.) An elliptic mode of measurement is one according to which it is possible to find two quantities A and B such that there is no third C as much larger than B as B is than A. A hyperbolic mode is one according to which a series of equidistant quantities A, B, C etc may be taken such that no matter how far it be continued, the greatest will never be so great as a certain given quantity Q. A parabolic mode is one according to which a series of equidistant quantities may be continued indefinitely and so far that the greatest will exceed any assigned quantity Q.

300.24–28 In MS 412, an offprint of this paper, Peirce inserted a lined sheet between the page containing this paragraph and the following page, on which he wrote: "Defining the limited & finite systems as above. To show

1st That every limited system is finite

2nd That every finite system is limited"

309.10–15 From this . . . lot] In a letter draft to Georg Cantor dated 23 December 1900 (L 73), Peirce wrote:

Even before I knew any of your papers, I had been led, in 1881, from the study of the Logic of Relatives, to a few ideas about numbers, and had particularly seen that finite classes differ from infinite ones in that a certain form of reasoning is valid of the former, that is not valid of the latter. It is De Morgan's *Syllogism of Transposed Quantity* of which this is an example:

Every Hottentot kills a Hottentot
No Hottentot is killed by more than one Hottentot
∴ Every Hottentot is killed by a Hottentot.

Every property which distinguishes a finite from an infinite class can be deduced from that.

312–27 Items 39–42 appeared as two of Peirce's many notes and his two addenda to the 2nd edition of his father's *Linear Associative Algebra,* which was published in AJM in 1881 and as a separate monograph by D. Van Nostrand in 1882. The AJM publication was preceded by the following note:

The consent of the family of the late Professor Benjamin Peirce has been kindly given to the publication in the *American Journal of Mathematics,* of this valuable and unique memoir on Linear Associative Algebra, of which only a small number of copies in lithograph were taken in the author's lifetime, for distribution among his friends. This publication will, it is believed, supply a want which has been long and widely felt, and bring within the reach of the general mathematical public a work which may almost be entitled to take rank as the *Principia* of the philosophical study of the laws of algebraical operation.

The Van Nostrand publication had the following preface:

Lithographed copies of this book were distributed by Professor Peirce among his friends in 1870. The present issue consists of separate copies extracted from The American Journal of Mathematics, where the work has at length been published.

The body of the text has been printed directly from the lithograph with only slight verbal changes. Appended to it will be found a reprint of a paper by Professor Peirce, dated 1875, and two brief contributions by the editor. The foot-notes contain transformations of several of the algebras, as well as what appeared necessary in order to complete the analysis in the text at a few points. A relative form is also given for each algebra; for the rule in Addendum II. by which such forms may be immediately written down, was unknown until the printing was approaching completion.

The original edition was prefaced by this dedication:

To My Friends.

This work has been the pleasantest mathematical effort of my life. In no other have I seemed to myself to have received so full a reward for my mental labor in the novelty and breadth of the results. I presume that to the uninitiated the formulae will appear cold and cheerless; but let it be remembered that, like other mathematical formulae, they find their origin in the divine source of all geometry. Whether I shall have the satisfaction of taking part in their exposition, or whether that will remain for some profound expositor, will be seen in the future.

In a letter to Frederick Adams Woods dated 11 September 1913 (L 477), Peirce wrote:

In my edition of B. P.'s *Linear Associative Algebra* (a research he never would have undertaken but for my constantly pestering him to do so) there are a number (not large) of individual oversights that escaped me as well as one *general* oversight, namely that though an algebra may not be separable into *two*, it may be separable into *three*.

312.1–313.2 This note refers to the algebra designated as (g_4), with the multiplication table

(g_4)	i	j	k	l
i	i	j	0	0
j	0	0	i	j
k	k	l	0	0
l	0	0	k	l

Benjamin Peirce remarked that this algebra is a form of quaternions. The J in the footnote has the property $J^2 = -1$.

312.4–7 This algebra. . . .] See "Description of a Notation," *W*2:359–429, especially 408–15.

313.3–318.25 In this note Peirce treats those algebras having five units (i, j, k, l, m), with the multiplication table of these units having zeros wherever j or l is a factor and either zeros or linear combinations of j and l elsewhere. One such system derived in the text is labeled (bm_5), with the multiplication table

(bm_5)	i	j	k	l	m
i	j	0	l	0	0
j	0	0	0	0	0
k	$-l$	0	0	0	$j+al$
l	0	0	0	0	0
m	j	0	$bj-al$	0	cj

According to H. E. Hawkes (*American Journal of Mathematics* 24 [1902]: 91), by a mixed algebra Benjamin Peirce meant one whose units could be divided into two or more sets (not necessarily disjoint) such that the product of two units of different sets is zero, while the product of units of the same set is in that set or is zero.

321.7–10 Thus . . . matrix.] In a long letter to William James dated 26 February 1909 (L 224) Peirce again gave a proof of this theorem and again used the LAA algebra (bd_5) as an example. Although that proof is very similar, it has much more detail and a different notation. The *brochure* reference is to item 43 below, pp. 328–33.

321.11 (bd_5)] The following definition appears in *Linear Associative Algebra*, pp. 91–92:

> $[242^2 1^3]$ The defining equation in this case can be reduced to
>
> $$ki = j + \mathfrak{r}l.$$
>
> This gives a quintuple algebra which may be called (bd_5), its multiplication table being

(bd_5)	i	j	k	l	m
i	j	0	l	0	0
j	0	0	0	0	0
k	$j+\mathfrak{r}l$	0	$i+m$	$\mathfrak{r}j$	$-j-\mathfrak{r}l$
l	0	0	j	0	0
m	$(\mathfrak{r}^2-1)j$	0	$-l$	0	$-\mathfrak{r}^2 j$

321.13 $\mathfrak{r}$] Sign for either of the two imaginary cube roots of 1.

321.26 the foot-note] "In relative form, $i = A{:}D + D{:}F + B{:}E + C{:}F$, $j = A{:}F$, $k = \mathfrak{r}A{:}B + \mathfrak{r}B{:}C + D{:}E - \frac{1}{\mathfrak{r}}D{:}F + E{:}F$, $i = A{:}E - \frac{1}{\mathfrak{r}}A{:}F + B{:}F$, $m = \mathfrak{r}^2 A{:}C - A{:}D - B{:}E - C{:}F$" (*Linear Associative Algebra*, p. 91).

321n.1–3 The "brief proof" in 1875 ("On the Application of Logical Analysis," *W*3:177–79) is very sketchy and leaves much for the reader to do. But even if we grant that in 1875 Peirce had a proof, however brief, that any linear associative algebra can be put into relative form, the comment that the proof of that theorem is "perhaps essentially the same as the above" hardly seems justified. What Peirce does in the present item is to prove a general rule whereby, given the multiplication table of such an algebra, we can derive a relative form for it by following a procedure in which every step is determinate. By contrast, in 1875 he had said that the resolution of each letter (unit) of the algebra into a sum of relatives is usually done easily although sometimes "a good deal of ingenuity is required," but he had not found that general rules help and does not say whether he had actually derived any general rules.

321n.1–3 See "On the Application of Logical Analysis," *W*3:177–79.

323.5 §40] The section reads as follows in the *Linear Associative Algebra* (p. 13):

> In every linear associative algebra, there is at least one idempotent or one nilpotent expression.
>
> Take any combination of letters at will and denote it by A. Its square is generally independent of A, and its cube may also be independent of A and A^2. But the number of powers of A that are independent of A and of each other, cannot exceed the number of letters of the alphabet; so that there must be some least power of A which is dependent upon the inferior powers. The mutual dependence of the powers of A may be expressed in the form of an equation of which the first member is an algebraic sum, such as
>
> $$\Sigma_m (a_m A^m) = 0.$$
>
> All the terms of this equation that involve the square and higher powers of A may be combined and expressed as BA, so that B is itself an algebraic sum of powers of A, and the equation may be written
>
> $$BA + a_1 A = (B + a_1)A = 0.$$
>
> It is easy to deduce from this equation successively
>
> $$(B + a_1)A^m = 0$$
> $$(B + a_1)B = 0$$
> $$\left(-\frac{B}{a_1}\right)^2 = -\frac{B}{a_1}$$
>
> so that $-\frac{B}{a_1}$ is an idempotent expression. But if a_1 vanishes, this expression becomes infinite, and instead of it we have the equation
>
> $$B^2 = 0$$
>
> so that B is a nilpotent expression.

The first paragraph notwithstanding, Benjamin Peirce actually proves that there is an expression whose square "vanishes" (instead of just some power of it) or there is an idempotent expression; it is this result that Charles uses. By "letters" Benjamin means the letters representing the units of the algebra, and the "alphabet" is the set of all these letters. He uses i for single algebras, i and j for double algebras, and on to i, j, k, l, m, n for sextuple algebras.

323.9 §41] When there is no expression whose square vanishes, any idempotent expression may be taken as one of the units, may be denoted by i, and may be called the *basis*. Then if A is any expression in the algebra, let $B = iA$. Then $iB = i^2A = iA$, and (since $i \neq 0$) the assumption in §1 of the present item yields $B = A$, or $iA = A$. Similarly, if $C = Ai$, then $Ci = Ai^2 = Ai$, whence $C = A$, or $Ai = A$. The equations $iA = Ai = A$ put A into what Benjamin calls "the first group" of expressions; thus in the algebra under consideration, *every* expression belongs to the first group.

323.12 §53] The relevant portion of §53 reads as follows (*Linear Associative Algebra,* p. 16):

> By a process similar to that of §40 and a similar argument, it may be shown that for any expression A, which belongs to the first group, there is some least power which can be expressed by means of the basis and the inferior powers in the form of an algebraic sum. This condition may be expressed by the equation
>
> $$\Sigma_m (a_m A^m) + bi = 0.$$

329.25–28 'self-x' . . . 'alio-*x*'] See "Description of a Notation," *W*2:416.

330.22–28 (1), External. . . .] See item 19 above, 204.6–8.

330.29–31 See p. 212 in Christine Ladd's article.

331.3–5 I have shown. . . .] See the preceding item, especially 327.8–11.

332.33–35 I have. . . .] See note 321n.1–3.

333.10–14 Professor Sylvester. . . .] Peirce's reference is probably to a course of lectures Sylvester gave at the Johns Hopkins in 1881–83, later published as "Lectures on the Principles of Universal Algebra."

333.17 the illustrious geometer] The reference is to Sylvester.

345–48 The paper by Benjamin Ives Gilman (1852–1933), American psychologist and logician, as well as Peirce's response, was presented before the Johns Hopkins Metaphysical Club in April 1882 and published in the *Circulars.* The abstract of Gilman's paper reads as follows:

> In this paper general terms were considered as of two kinds: the intensively definite, denoting a fixed group of objects. Six kinds of propositions involving such terms were described, viz: Verbal Propositions, asserting that two groups of qualities have components in common (Propositions of Definition) or that two groups of objects have components in common (Propositions of Division); and Real Propositions, asserting (1) that two groups of qualities are sometimes found in the same object (Propositions of Concomitance), or (2) that two groups of objects possess qualities in common (Propositions of Resemblance), or (3) that, in some cases, components of a group of objects possess a group of qualities (Propositions of Possession), or (4) that, in some cases, components of a group of qualities are common to a group of objects (Propositions of Inherence). Each of these propositions may be denied as well as asserted, the affirmative species being particular and the negative universal; and each of these latter gives rise by negating the terms in all ways to four forms. Three of the six kinds of propositions, those of Definition, Resemblance, and Inherence, are the reciprocals of the other three, referring to qualities as the latter do to objects; and take a negative similarly reciprocal to the ordinary negative. The syllogistic of these forty-eight propositions consists of arguments following either the particular or universal type of ordinary syllogism, and in the case of Real Propositions consists of twelve such arguments, six universal and six particular.
>
> From Propositions of Concomitance, signifying the assertion or denial of the existence of a class, a complex propositional system was developed containing propositions asserting or denying more than one class. Such a system contains seventy-five varieties, or eighty if the possibility is allowed of the non-existence of the Universe of Discourse. These may be reduced to eighteen types, or twenty on the supposition just made. For the expression of these propositions a special notation

was employed. The syllogistic of these eighteen types embraces thirty-four forms of complex argument. A similar complex syllogistic may be developed from either one of the other five kinds of propositions named above.

346.28–30 What De Morgan. . . .] See his *Formal Logic,* p. 153. Peirce later wrote the "Spurious Proposition" entry for Baldwin's *Dictionary of Philosophy and Psychology* (1901–1902).

350.4 Herschel] John Herschel, Royal Engineer, employee of the India Survey.

350.4 Superintendent] Julius Erasmus Hilgard, Superintendent from 1881 to 1885.

350.10 Newcomb] Simon Newcomb.

351.11 King] Clarence King.

353.1 Wolf] Charles Joseph Etienne Wolf; see note 82.24–39.

353.2–7 "Je vous . . . stabilité."] I congratulate you heartily on your work on the pendulum and especially on your having taken another track than the one which, following the Germans and the Swiss, the Americans have taken. I have had lively discussions here with Mr. Ch. Peirce about his experiments with the Repsold reversible pendulum, an instrument which seems to me to be constructed in a way that makes it deplorably unstable.

355.9 Malaspina] Alejandro Malaspina.

355.10 Foster] Henry Foster.

355.10 Lütke] Fedor Petrovich Litke (or Friedrich Benjamin von Lütke), Russian admiral (1797–1882).

355.11 Basevi] James Palladio Basevi (1832–1871), English geodesist.

357.27–29 I am . . . purpose.] See items 4, 21, 29, and 37.

357.38–358.2 the measurements. . . .] The references are to Biot and Arago's *Recueil d'observations géodésiques* and to Bessel's "Bestimmung der Länge."

358.3–5 the value . . . Heaviside] See Heaviside's "Preliminary Abstract."

359.2–6 Mendenhall. . . .] See his *Measurements of the Force of Gravity.*

361.11 the late Superintendent] C. P. Patterson.

361n.1 The brackets enclosing this note indicate that it is a remark by someone other than the author, a practice followed throughout this item and in items 53 and 54. In the annotated offprint of the *Report* in MS 492, Peirce wrote the following above the title of item 51: "Comments upon the following papers by the different gentlemen present at the conference are printed along with them; but are distinguished from the text by being placed in square brackets."

369.25 Duperrey] Louis Isidor Duperrey.

370.17 Young's rule] See his article on "Tides."

370.18–20 Stokes reaches. . . .] See his "On the Variation of Gravity."

370n.1 *Bouguer's* formula] See his "De la manière de déterminer."

372.20 Godin] Louis Godin.

372.21 Condamine] Charles Marie de la Condamine.

372n.1 Gehler] Johann Samuel Traugott Gehler (1751–1795), German physicist and mathematician.

378.10–11 Logic had. . . .] The two references are, respectively, to the

Port-Royal Logic, and to Ueberweg's *System der Logik* (where logic is defined as "the science of normative or ideal laws of human cognition").

378.12–14 *"Dyalectica . . . ,"*] This is the first sentence in the *Summulae logicales* of Petrus Hispanus, except that Peirce has added *"aliarum scientiarum"* from its second sentence. For Peirce's repeated use of the sentence, and his translation thereof, see 400.17–20 below.

378.14 *viam habens*] On a fragmentary page in MS 425, Peirce penned the following note:

> [viam habens = ὁδὸν ἔχουσα.] This is the worthiest conception of the study.
>
> The word *logic* is supposed to have originated with the early peripatetics (i.e. followers of Aristotle). It is used in two senses, which were distinguished by Duns Scotus, the Doctor Subtilissimus, as *logica docens* and *logica utens;* the former being the science, the latter the practice of logic. *Logica docens,* says Ockham, is that science which teaches us to define, to divide (or make distinctions), and to distinguish truth from error by means of arguments.

378.21 Ohm] The unit of electrical resistance named after Georg Simon Ohm.

379.11–13 But read. . . .] The reference is to Philodemus of Gadara's treatise *On Methods of Inference,* defending Epicurean empirical methods against the Stoics. See also 408.6–10.

379.34–36 a voice crying. . . .] A free paraphrase of Matthew 3:3.

380.23 the economists] The classical economists Adam Smith and David Ricardo who developed the concept of free trade.

380.25 Wundt] Wilhelm Wundt.

380.26 Galton] Francis Galton; see also 440.6 and 488.21 (and the accompanying note).

380.27 Morgan] Lewis Henry Morgan.

380.28 Cournot] Antoine Augustin Cournot.

380.29 The philologists] The neogrammarians, most notably Friedrich Karl Brugmann, who applied methods of the natural sciences to linguistics.

380.30 The astronomers] Those astronomers who apply the spectroscopic method in astronomy, especially Lewis Morris Rutherfurd in the United States.

380.31 radiant heat . . . ;] Sir George Stokes illustrated phenomena of emission and absorption of heat radiation by the resonance in sound.

380.32 the mental. . . .] The reference is to Wilhelm Wundt, who applied to psychology the experimental methods of the natural sciences.

382.21 Kepler's. . . .] *De Motibus* is a part of Kepler's *Astronomia Nova.*

383.5–14 The editors of the *Collected Papers* have transposed the order of the two paragraphs (CP 3.324), without evidence that such was Peirce's intention.

384.2 B. Peirce's form (a_1)] *Linear Associative Algebra,* p. 120.

385.2 Hamilton's biquaternions] See William Rowan Hamilton's *Lectures on Quaternions.*

389.7–9 Sherman] Orray Taft Sherman (1856–1921), American physicist.

394.1 Mitchell] Oscar Howard Mitchell (1851–1889), American mathematician who was one of Peirce's students at the Johns Hopkins.
400.13–21 In the . . . methods.] See notes 378.12–14 and 378.14.
400.20–22 One of. . . .] See 378.10–11 and the accompanying note.
401.28–31 Transcendental . . . Jevons.] See Hegel's *Wissenschaft der Logik,* Everett's *Science of Thought,* Whewell's *Novum Organon,* Mill's *System of Logic,* and Jevons's *Principles of Science.*
403.2–3 for 'actual'. . . .] See Kant's famous argument that existence is not a predicate, in his *Kritik* A598–99, B626–27.
403.11–12 A proposition. . . .] In "On a New Algebra of Logic."
406.14–18 A series. . . .] See Jevons's *Pure Logic,* Peirce's "On an Improvement in Boole's Calculus" (*W*2:12–23), Robert Grassmann's *Begriffslehre,* Schröder's *Operationskreis,* and McColl's "Calculus of Equivalent Statements."
406.21–25 Boole and Venn. . . .] Boole, *Laws of Thought,* pp. 33–34, and Venn, *Symbolic Logic,* pp. 43–44.
406.27–407.15 Mr. McColl . . . different.] In "The Calculus of Equivalent Statements," p. 181, McColl used the terms "implication" and "nonimplication." Peirce introduced the symbol of inclusion in his 1870 "Description of a Notation" (*W*2:359–429); the symbol of non-inclusion did not appear until 1880, in his "On the Algebra of Logic," item 19 above, 166.17–19.
406.27–28 I find . . . *existence;*] Peirce's view on the necessity of the copulas is recorded in Christine Ladd's "On the Algebra of Logic" (p. 23n) as follows: "Every algebra of logic requires two copulas, one to express propositions of non-existence, the other to express propositions of existence. This necessarily follows from Kant's discussion of the nature of the affirmation of existence in the *Kritik der reinen Vernunft.* —C. S. Peirce."
407.4 as De Morgan . . . ,] *Formal Logic,* pp. 49–50 and 52.
407.16–21 Miss Ladd . . . sixth.] See Ladd's "On the Algebra of Logic," pp. 23–25, Mitchell's "On a New Algebra of Logic," and De Morgan's "On the Structure of the Syllogism," 381.
407.27 Ellis] Robert Leslie Ellis (1817–1859), English mathematician and editor of Bacon.
407.28 MacFarlane] Alexander MacFarlane (1859–1913), American mathematician.
408.1–3 In one. . . .] See Gilman's "Operations in the Relative Number."
408.3–5 In the other. . . .] See Marquand's "A Machine for Producing Syllogistic Variations."
408.6–10 In the first. . . .] See Marquand's "The Logic of the Epicureans"; see also 379.11–13 and the accompanying note.
411.36–38 Locke. . . .] *Essay Concerning Human Understanding,* bk. 4, ch. 15.
413.2–5 The celebrated law. . . .] Gustav Theodor Fechner's mathematical formula for the relationship between sensation and physical stimuli; also known as the Weber-Fechner law.

418n.12 De Morgan, *Formal Logic,* ch. 8.

419.2 In his 1902 grant application to the Carnegie Institution (L 75), Peirce wrote:

> I shall then come to the important question of the classification of arguments. My paper of April, 1867, on this subject divides arguments into Deductions, Inductions, Abductions (my general name, which will be defended) and Mixed Arguments. I consider this to be the key of Logic. In the following month, May, 1867, I correctly defined the three kinds of simple argument in terms of the categories. But in my paper on Probable Inference in the Johns Hopkins "Studies in Logic," owing to the excessive weight I at that time placed on formalistic considerations, I fell into the error of attaching a name[,] the synonym I then used for Abduction, to a probable inference which I correctly described, forgetting that according to my own earlier and correct account of it, abduction is not of the number of probable inferences. It is singular that I should have done that, when in the very same paper I mention the existence of the mode of inference which is true abduction. Thus, the only error that paper contains is the designation as Abduction of a mode of induction somewhat resembling abduction, which may properly be called abductive induction. It was this resemblance which deceived me, and subsequently led me into a further error contrary to my own previous correct statement. Namely, continuing to confound abduction and Abductive Induction, in subsequent reflections upon the rationale of Abduction, I was led to see that this rationale was not that which I had in my Johns Hopkins paper given of induction; and in a statement I published in the *Monist,* I was led to give the correct rationale of abduction as applying to Abductive Induction and so, in fact, to all induction. All the difficulties with which I labored are now completely disposed of by recognizing that Abductive Induction is quite a different thing from abduction. It is a very instructive illustration both of the dangers and of the strength of my heuretic method. Similar errors may remain in my system. I shall be very thankful to whoever can detect them. But if its errors are confined to that class, the general fabric of the doctrine is true. I at first saw that there must be three kinds of arguments severally related to the three categories; and I correctly described them. Subsequently studying one of these kinds, I found that besides the typical form, there was another, distinguished from the typical form by being related to that category relation to which distinguishes abduction. I hastily identified it with abduction, not being clear-headed enough to see that, while related to the category, it is not related to it in the precise way in which one of the primary divisions of arguments ought to be, according to the theory of the categories. This is the form of error to which my method of discovery is peculiarly liable. One sees that a form has a relation to a certain category, and one is unable, for the time being, to attain sufficient clearness of thought to make quite sure that the relation is of the precise nature required. If only one point were obscure, it would soon be cleared up; but the difficulty is at first that one is sailing in a dense fog through an unknown sea, without a single landmark. I can only say that if others, after me, can find some way of making as important discoveries in logic as I have done while falling into less error, nobody will be more intensely delighted than I shall be.

419.28–29 Newton. . . .] Newton's famous *hypotheses non fingo* appears in the "General Scholium" of his *Principia* (2:201–2).

419.31–36 The inferences. . . .] See Kepler's *De Motibus.*

421.5–9 This classification . . . other.] See note 407.16–21.

422n.1–2 See "How to Make Our Ideas Clear," *W*3:257–76.

429n.1–5 The doctrine that the study of affirmative instances should be accompanied by investigating negative instances was first laid down by Francis Bacon in the Tables of Presence and Tables of Absence (*Novum*

Organum, XI–XII), and was further elaborated by Mill as the Joint Method of Agreement and Difference (*System of Logic,* bk. 3, ch. 8, §4).

435.17–23 See Wheeler's *Brief Biographical Dictionary.* Christian Aagard was a Danish poet, Gaspar Abeille a French poet, Abulola (973–1057) and Abunowas (762–810) Arab poets, and Stephen Tabourot Accords (1516–1561) a French poet.

435.36–438.2 so accomplished . . . chance.] See Playfair's "Note on the Numerical Relations"; the first two tables appear on p. 2, the third on p. 18.

438.3–4 "Bode's law". . . ,] Also known as the Titius-Bode law of planetary distance, it was first pointed out in 1766 by Johann Daniel Titius (1729–1796) and popularized by Johann Elert Bode (1747–1826). The law approximates the mean distances from the Sun of the planets known at the time. It gives a close value for the distance of Uranus but not for Neptune, both discovered afterwards.

440.3–5 The influence . . . Philodemus,] See note 379.11–13.

440.6 Galton] See 488.21 and the accompanying note.

440.15–18 This kind. . . .] Mill, *System of Logic,* bk. 3, ch. 3.

441.22–24 Then. . . .] In his *Lettres sur la théorie des probabilités* (p. 18), Quételet gives a similar example of the probability of a future occurrence of an event already observed.

441n.1 Laplace's book also includes a discussion of the probability of future events on the basis of events already observed.

442.7–10 But Boole. . . .] For Boole's discussion of the principle of the equal distribution of knowledge or ignorance, see his *Laws of Thought,* pp. 369–75.

444.13–16 Mill. . . .] *System of Logic,* bk. 2, ch. 3; among his followers are Alexander Bain and John Venn, a defender of Mill against Jevons.

444.16–19 The Abbé. . . .] In his *Logique,* Gratry characterizes induction as the discovery of the eternal laws imposed by God, but he does not claim that it involves God's miraculous intervention.

446.7 as I have elsewhere shown] See "The Order of Nature," *W*3:310.

446.17–447.29 This presentation of the relationship between rainfall and illiteracy draws on Peirce's paper "On the Coincidences of the Geographical Distribution of Rainfall and Illiteracy," which was read before the Philosophical Society of Washington in 1872 and then published, in a somewhat different form and under the title "Rainfall," in the 1874 *Atlantic Almanac;* see *W*3:167–72.

447.1–12 This table is derived from the values of monthly precipitation given in C. A. Schott's *Tables and Results,* pp. 127–32; the percentages are Peirce's.

448 The map is based on the U.S. Rain Chart, which is attached to Schott's *Tables and Results.* The original map, engraved for the Smithsonian Institution by H. Lindenkohl, covers the entire country and shows more detail.

449 The map is taken from the *Statistical Atlas of the Ninth Census of 1870.*

453.2–4 But the. . . .] See note 346.28–30.

453n.1–3 See note 345–348.

454.33 Boole's multiplication] *Laws of Thought,* pp. 29–32.

463.13 Dr. Mitchell's . . . propositions] See his "On a New Algebra of Logic," p. 87.

463.33 Dr. Mitchell's. . . .] Ibid., p. 90.

464n.1 See item 66, pp. 454.31–455.6.

466.26–27 Mr. Schlötel. . . .] Peirce's reference is to his article published in 1880 (item 19) and to the following note which appeared in the *Proceedings of the London Mathematical Society* 12 (1880–1881): 215: "Herr W. Schlötel, of Strassburg, in a letter to the Secretaries, dated January 21, 1881, 'in order to secure priority' as against Messrs. C. S. Peirce and McColl, refers to papers communicated by him to the *Augsburger Allgemeine Zeitung,* in 1868, 1871, and 1876."

On the title page of his copy of Schlötel's *Logik,* Peirce has written: "C. S. Peirce first set eyes on this book 1881 Nov. 11."

466.29 Professor Drobisch] Moritz Wilhelm Drobisch.

467.4–5 Sylvester's . . . *"Erratum"*] The *"Erratum"* is on p. 46 of the *Circulars,* and reads as follows:

> In the article headed 'A Word on Nonions' in the *Circulars,* No. 17, p. 242, near the middle of the page, for the words *Those forms can be derived from an algebra given by Mr. Charles S. Peirce, ('Logic of Relatives', 1870),* read *Mr. C. S. Peirce informs me that these forms can be derived from his 'Logic of Relatives', 1870.* I know nothing whatever of the fact of my own personal knowledge.[1] [[1]I have also a great repugnance to being made to speak of Algebras in the plural: I would as lief acknowledge a plurality of Gods as of Algebras.] I have not read the paper referred to, and am not acquainted with its contents. This mistake originated in my having left instructions for Mr. Peirce to be invited to supply in my final copy for the press, such references as he might think called for. He will be doing a service to Algebra by showing in these columns how he derives my forms from his logic.[2] [[2]I had understood Mr. Peirce to say that these forms were actually contained in his memoir.] The application of Algebra to Logic is now an old tale–the application of Logic to Algebra marks a far more advanced stadium in the evolution of the human intellect; the same may be said as regards the application by Descartes of Analysis to Geometry, and the reverse application by Eisenstein, Dirichlet, Cauchy, Riemann, and others, of Geometry to Analysis–so that if Mr. Peirce accomplishes the task proposed to him, (his ability to do which I do not call into question), he will have raised himself as far above the level of the ordinary Algebraic logicians as Riemann's mathematical stand-point tops that of Descartes.
>
> It is but justice to Boole's memory to recall the fact that, in one of his papers in the *Philosophical Transactions,* he has made a reverse use of logic to establish a certain theorem concerning inequalities, which is very far from obvious, and which I think he states it took him ten years to deduce from purely algebraical considerations, having previously seen it through logical spectacles—I mean, by the aids to vision afforded him by his logical calculus: this theorem I believe (or at least did so when it was present to my mind) must of necessity admit a much more comprehensive form of statement.

Sylvester followed up his "Erratum" with a "Note" dated 30 March 1883 which, though to some degree a reply to Peirce's "Communication," is printed right before it. The "Note" reads as follows:

> My attention has been called to an appearance of contradiction between an erratum which I inserted on page 46 of the *Circulars* and a remark of mine in a previous number (*No.* 15, *May,* 1882). I think the seeming discrepancy will disappear if the point I desire to make is duly apprehended. I wished (and I still wish) it to be understood that it is Mr. Peirce's statement and not mine that the "forms" in question can be derived from his Logic of Relatives. I certainly know what he has told me and should attach implicit credit to any statement emanating from him, but have not the knowledge which would come from having myself found in his Logic of Relatives the forms referred to: as previously stated I have not read his Logic of Relatives and I am not acquainted with its contents.

For further discussion of this controversy, see the Introduction as well as the textual headnote for the present item.

469.25–27 in the ninth . . .] See "Description of a Notation," *W*2:359–429.

470.2–4 "I can . . . way,"] Ibid., p. 413.

470.5 I afterwards. . . .] See "On the Application of Logical Analysis," *W*3:177–79, and item 41 above, p. 321.8–9.

470.6–7 "all such. . . ."] See "Description of a Notation," *W*2:413; the entire passage reads: "In other words, all such algebras are complications and modifications of the algebra of (156). It is very likely that this is true of all algebras whatever. The algebra of (156), which is of such a fundamental character in reference to pure algebra and our logical notation, has been shown by Professor Peirce to be the algebra of Hamilton's quaternions."

470.10 *quadrates*] In "On the Uses and Transformations of Linear Algebra," p. 396, Benjamin Peirce credits William Kingdon Clifford with having introduced the name at a meeting of the London Mathematical Society in 1870.

471.20 In fact . . . 53.] See "Description of a Notation" (*W*2:359–429) and the accompanying editorial headnote. Although the official publication of Peirce's memoir in 1873 was paginated 317–378, it was printed as a separate brochure in 1870, beginning with page 1. Page 53 corresponds to page 365 in the official publication (and to p. 419 in *W*2).

483.7 Porphyry] See note 8.5.

483.7 Durandus] Durandus of Saint-Pourçain.

486.10 Herbart] Johann Friedrich Herbart.

487.1 Trendelenburg] Friedrich Adolf Trendelenburg.

487.11–12 The *ars magna*. . . .] In his *Ars magna et ultima,* Raymond Lully (Ramón Lull) developed a mechanical procedure facilitating inference which had some influence on Leibniz.

487.18–19 The *Parva Logicalia* ("small works of logic") was a series of treatises on problems in logic which emerged in Scholastic philosophy. Peirce lists treatises on the rules of the substitution of terms (suppositiones), on the distribution of terms (distributio), on the exponible propositions (exponibilia), on paradoxes (insolubilia), and on the rules of disputing (obligationes). See Peirce's entry in Baldwin's *Dictionary of Philosophy and Psychology* (1901–1902).

487.22 Ass of Buridanus] The traditional example of Buridan's ass, which starved in indecision between two equally appealing stacks of hay, illustrates the problem of choice in the absence of preference. It is not found in John Buridan's writings and was probably formulated as a refutation of his theory of mind and will, according to which we must will what presents itself to the reason as a greater good.

488.8–9 law of Fechner] See note 413.2–5.

488.21 Galton's methods] Francis Galton's statistical methods of investigating mental phenomena, influenced by Darwin.

488.22 Ferrero's theory] Annibale Ferrero (1839–1902), Italian mathematician, geographer, and geodesist, wrote on applications of the method of least squares. See Peirce's review of Ferrero's *Esposizione del metodo dei minimi quadrati*, *W*3:375–81.

489.11–12 See note 249.8–9.

489.14 The reference is to Galileo's *Dialogo sopra i due massimi sistemi del mondo* (translated as *Dialogue Concerning the Two Chief World Systems, Ptolemaic and Copernican*).

489.22 Stallo's objections] See his *Concepts and Theories*, ch. 8.

490.3–4 the. . . .] See Christine Ladd's "On the Algebra of Logic."

495.29–33 Ibid., p. 66, and Mitchell's "On a New Algebra of Logic," p. 76.

497.13–16 See item 19 above, pp. 163–209.

507.4 this course] The reference is probably to Peirce's advanced logic course at the Johns Hopkins in the fall of 1883.

507.6 *Summulae*] The first part of Petrus Hispanus's book is strongly Aristotelian, while the second deals with new problems posed by Scholastic logic, among them the doctrine of supposition.

507.17–23 Thus . . . premises.] Peirce's example of the fallacious syllogism which illustrates the confusion of material and personal suppositions does not come from the *Summulae*, where a similar fallacy resulting from the confusion of simple and personal suppositions can be found (7.45): "homo est species; Socrates est homo; ergo Socrates est species."

507.20 Sortes] Latin for Socrates.

507.22 *suppositio*] According to Petrus Hispanus, supposition is "the acceptance of a substantive term as denoting something." The related term "suppositum" is defined as "the thing denoted by a name in a given proposition" in the *Century Dictionary*.

508.5 Krug, Esser, Hamilton] Wilhelm Traugott Krug; Wilhelm Esser (1798–1854), German philosopher; and Sir William Hamilton.

509.2 *Elenchi*] The reference is to the last book of Aristotle's *Topics* known as *De Sophisticis Elenchis* (*Sophistic Refutations*), which contains a theory of fallacious inferences.

509.4–5 And when . . . ,] Halstead, "Boole's Logical Method," 91.

509.14–15 You will . . . *Discussions*] William Hamilton, *Discussions*, pp. 131–35.

509.16–17 *Commentary* . . . Conimbricenses] The reference is to the sixteenth-century commentaries on Aristotle from the Jesuit University of Coimbra in Portugal.

509.17–20 Petrus . . . habens.] See notes 378.12–14 and 378.14.

509.33–34 the science . . . ,] Not located, although Peirce may have derived the phrase "the science of pure quality" from the title of Jevons's book: *Pure Logic or the Logic of Quality apart from Quantity.*

510.1 *normative laws of thought*] See note 378.10–11.

515.24–30 See Bessel's *Untersuchungen,* pp. 7–8.

516.1 Basevi] See note 355.11.

516.4–6 a writer . . . flexure,] Edward Sang (1805–1890), English mathematician and physicist, in the article "Pendulum."

516.7 Cellérier] Charles Cellérier (1818–1889), Swiss physicist.

516.19–24 "The necessity . . . to question."] Baeyer, "Rapport," pp. 90–91. The translation from the French is Peirce's.

516.37–517.5 "The question . . . instrument."] Bruhns, "Rapport," p. 93.

517.6–16 "The fear . . . air."] Hirsch, "Rapport," p. 96.

517.38–39 "there remains. . . ."] Hirsch, "Rapport" (1877), p. 18.

517.40 Oppolzer] Theodor Ritter von Oppolzer.

518.1–3 In the. . . .] See "On the Influence of the Flexibility," *W*3:217–34.

518.21 "étude. . . ."] Not located, although it is possible that this phrase (and the one in 518.30) was communicated orally.

519.1–2 *grossissement*] Plantamour actually uses the term "magnification" ("Recherches expérimentales," p. 5).

520.1 Fig. 26] See page 87 above.

522.10 Atwood's machine] An apparatus for measuring the accelerative action of gravity, invented by George Atwood (1746–1807), English mathematician and natural philosopher.

526.11–12 I have shown . . . 113)] See item 5 above, 13.23–24.

529–34 At the February 1881 meeting of the Johns Hopkins Scientific Association, Peirce presented a related paper entitled "A New Computation of the Compression of the Earth, from Pendulum Experiments," an abstract of which appeared in the April 1881 *Circular:*

> The principles adopted were as follows: 1. Only experiments with the Kater invariable pendulum were used. 2. These were newly reduced, using the temperature and pressure coefficients determined by the India survey. 3. The continents were considered to be formed by upheaval, so that the usual reduction for continental attraction disappeared. In estimating the small residual effect, it was assumed that the thickness of the crust upheaved is $\frac{1}{8}$ of the diameter of the arch which gives $\frac{1}{2}$ of the usual correction. 4. The entire attraction of the ocean has been allowed for. It was shown that the adoption of these principles in the computation greatly reduces the station errors. The resulting compression is $\frac{1}{293.0 \pm 0.5}$.

In a letter to Superintendent Hilgard dated 29 April 1882, Peirce describes the advantages of using the Kater invariable pendulum for the determination of the figure of the earth.

529.12 Kater . . . pendulums] Henry Kater (1777–1835), English geodesist, introduced in 1819 an invariable compound pendulum with a single knife-edge, thirteen of which were constructed and swung throughout the world.

529.27–530 The . . . Sabine,] See Sabine's "On the Reduction to a Vacuum."

530.4–6 Baily. . . .] Baily, "Report," 3.

530.7 Lütke's numbers] Lütke made observations of gravity during a trip around the world; they are recorded in his *Observations du pendule invariable.*

530.27 Young's rule] See note 370.17.

531.18–20 according to. . . .] See J. H. Pratt's *A Treatise on Attractions.*

533.12 The contour . . . ,] Wild, *Thalassa,* p. 14.

534.32 Foster] Henry Foster.

544–54 Peirce presented a lecture on "Design and Chance" to the Johns Hopkins Metaphysical Club on 17 January 1884, for which he almost certainly used the present manuscript. For a discussion of this important paper, see the Introduction, especially pp. lxviii–lxx.

544.5 Darwin's great work] The reference is to *On the Origin of the Species.*

544.13 Story] William Edward Story (1850–1930), American mathematician, who taught at the Johns Hopkins and at Harvard.

544.14 Crookes] William Crookes (1832–1919), English chemist and physicist.

544.15 Zöllner] Johann Karl Friedrich Zöllner.

546.3–4 Mill. . . .] *System of Logic,* bk. 3, ch. 3, § 1.

546.11 Gauss] Karl Friedrich Gauss.

546.11 Lobachewsky] Nikolai Ivanovich Lobachewsky.

546.15–20 There are . . . triangle.] In the left margin, Peirce has drawn a rough diagram that may be intended to illustrate his point about parallax.

547.40–548.1 the remarks. . . .] See his "Cosmic Emotion."

551.20 the laws of Boyle and Charles] Robert Boyle formulated the ideal-gas law stating that the volume of a gas varies inversely with its pressure; Jacques Charles' law says that at a constant pressure the volume of a gas is directly proportional to the temperature.

555.7–9 Kant took. . . .] The reference probably is to Christian Wolff's followers, the Pietists, who added will to Wolff's single faculty of cognition. Kant was the first to formulate the theory of the three faculties of cognition, feeling, and desire.

557.34–36 The great algebraist. . . .] The reference is to J. J. Sylvester, who was professor at the Johns Hopkins from 1876 to 1883.

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*The degree symbol (°) indicates works Peirce is known to have owned.

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Chronological List
1879–1884

Three kinds of materials are included in this list, which covers the beginning of 1879 through the spring of 1884:

1. All of Peirce's known publications, identified by P followed by a number. For these numbers and for further bibliographical information, see *A Comprehensive Bibliography of the Published Works of Charles Sanders Peirce with a Bibliography of Secondary Studies,* 2nd ed. rev., ed. Kenneth Laine Ketner (Bowling Green, OH: Philosophy Documentation Center, 1986), the letterpress companion volume to the 161-microfiche edition of Peirce's published works.

2. All of Peirce's known manuscripts, typescripts, and annotated offprints, identified by MS followed by a number. These numbers reflect the Peirce Edition Project rearrangement and chronological ordering of the Peirce Papers, the originals of which are in the Houghton Library of Harvard University, and of papers found in other collections. Parentheses after the MS number give either the name or location of those collections, or they identify the Harvard manuscript number. For the latter, see Richard S. Robin, *Annotated Catalogue of the Papers of Charles S. Peirce* (Amherst: University of Massachusetts Press, 1967) and "The Peirce Papers: A Supplementary Catalogue," *Transactions of the Charles S. Peirce Society* 7 (1971): 37–57.

3. Those letters and letter drafts that are included in the edition, identified by L followed by a (Harvard) number. Parentheses give the location of letters not contained in the Peirce Papers.

Not included here are (1) those items in *A Comprehensive Bibliography* that merely mention Peirce's Coast Survey duties and observations (in the annual *Report of the Superintendent of the United States Coast Survey*) or give mere titles or descriptive notes of papers (as in the annual *Report of the National Academy of Sciences*) which were presented at professional meetings or on other occasions, but for which there are no manuscripts; (2) the second of the French "Illustrations of the Logic of Science" articles, published in January 1879 but included in our third volume with the other Illustrations (P 162; *W*3:355–74); and (3) the twenty-five manuscripts (MSS 466–490) that belong to Peirce's "Great Men" project, begun at the Johns Hopkins University in the fall of 1883 and lasting through the fall of 1884. As some of these manuscripts were not written until the latter part of the one-year period, when the project came to preliminary fruition, it has been decided to include the "Great Men" in volume 5 of our edition—and to list the twenty-five manuscripts there.

Manuscripts and a few rarely republished items that have appeared in earlier editions are identified in brackets at the end of the entry. CP refers to the *Collected Papers;* HPPLS to *Historical Perspectives on Peirce's Logic of Science: A History of Science,* 2 parts, ed. Carolyn Eisele (Berlin: Mouton, 1985); N to *Charles Sanders Peirce: Contributions to THE NATION,* 4 parts, ed. Kenneth L. Ketner and James E. Cook (Lubbock: Texas Tech Press, 1975–88); and NEM to *The New Elements of Mathematics,* 4 vols., ed. Carolyn Eisele (The Hague: Mouton, 1976).

Dates of publication or composition appear to the right; those in italics are Peirce's own. Descriptive or supplied titles are given in italic brackets, and journal titles are abbreviated. Letters and numbers in boldface indicate that the item is published in the present volume.

MS 334 (National Archives) *25 January–10 February 1879*
*[*Allegheny Pendulum Computations*]*.

MS 335 (National Archives) *17 February 1879*
*[*Report on Kew Observatory Pendulum Observations*]*. [Cf. P 137.]

MS 336 (National Archives) *1 March 1879*
Four Independent Comparisons . . . of Centimetres.

MS 337 (1355a) March 1879
A Quincuncial Projection of the World (Map). [Cf. P 135 and 183.]

P 148. Item 1 3 April 1879
"Read's *Theory of Logic.*" *Nation 28,* 234–35. [N 1:56–58.]

MS 338 (1075) spring 1879
Preliminary Account of the Comparison of a Wave-length with the metre (with L.M. Rutherfurd).

MS 339 (1074) spring 1879
Determination of the relative length of a wave of light and a metre bar.

MS 340 (1073) spring 1879
Determination of the relative length of a wave of light and a metre bar.

MS 341 (1073) spring 1879
Determination of the relative length of a wave of light and a metre bar. [Amanuensis.]

P 163. Item 2 1 May 1879
"Spectroscopic Studies." *Science News* 1, 196–98. [Reprinted in *Nature* 20 (29 May 1879): 99–101 (P 155).]

MS 342 (933). Item 3 May 1879
*[*Lecture on Logic and Philosophy*]*.

MS 343 (1098, S 87) spring–summer 1879
*[*On Ghosts in Diffraction-Spectra*]*. [Cf. P 134.]

MS 344 (National Archives) July 1879
Pendulum Observations. [Cf. P 208.]

P 136. Item 4 July 1879
"Note on the Progress of Experiments for comparing a Wave-length with a Meter." *Am J of Science and Arts,* 3rd ser. 18, 51. [German abstract in O 139.]

MS 345 (National Archives) *21 August–18 November 1879*
[Ebensburg and Allegheny Longitude Determinations].

MS 346 (National Archives) *26 August–12 September 1879*
*[*Ebensburg Latitude Computations*]*.

MS 347 (National Archives) *26 August–6 October 1879*
*[*Allegheny Time Computations*]*.

P 137. Item 5 August 1879
"On a method of swinging Pendulums for the determination of Gravity, proposed by M. Faye." *Am J of Science and Arts,* 3rd ser. 18, 112–19. [See Faye's discussion in O 140. German abstract in O 169.]

MS 348 (575). Item 6 fall 1879
On the Algebraic Principles of Formal Logic.

MS 349 (748b, 875, 1114) fall–winter 1879
Logic. Chapter I. Of Thinking as Cerebration.

MS 350 (748a, 748d). Item 7 fall–winter 1879
Logic. Chapter I. Of Thinking as Cerebration.

P 149. Item 9 16 October 1879
"Rood's *Chromatics.*" *Nation* 29, 260. [N 1:58–61.]

MS 351 (Marquand Papers) October 1879–January 1880
*[*Logic Course Lecture Notes, by Allan Marquand*]*.

P 143 11 November 1879
"Questions concerning certain Faculties claimed for Man." *Johns Hopkins Univ Circulars* 1:2 (Jan. 1880):18. [Abstract of P 26 (*W*2:193–211).]

P 156 *14 November 1879*
"Mutual Attraction of Spectral Lines." *Nature* 21 (4 Dec. 1879): 108. [German abstract in O 170.]

P 150 25 December 1879
["The current number of the *American Journal of Mathematics"]*. *Nation* 29, 440. [N 1:61–62.]

MS 352 (1366) 1879
*[*Bowditch's *The Growth of Children]*

P 134. Item 10 1879
"On the Ghosts in Rutherfurd's Diffraction-Spectra." *Am J of Mathematics* 2, 330–347. [Abstract in *Johns Hopkins Univ Circulars* 1:4 (April 1880): 45. See also H. A. Rowland's remarks in O 181. German abstract in O 191.]

P 158 1879
"Pendulum-Observations." *Coast Survey Report 1876,* 6–9.

P 135. Item 11 1879
"A Quincuncial Projection of the Sphere." *Am J of Mathematics* 2, 394–396. [Reprinted in *Coast Survey Report 1877,* 191–192 (P 183) and, in part, in Thomas Craig, *A Treatise on Projections*

(Washington, D.C.: Government Printing Office, 1882), 132 and 247 (P 238). Abstract in *Johns Hopkins Univ Circulars* 1:4 (April 1880): 45.]

P 159 1879
"A Catalogue of Stars for Observations of Latitude." *Coast Survey Report 1876,* 83–129. ["Selected under the direction of Assistant C. S. Peirce."]

P 160. Item 12 1879
"Note on the Theory of the Economy of Research." *Coast Survey Report 1876,* 197–201. [CP 7.139–157. *Operations Research* 15 (1967): 641–48.]

P 161. Item 13 1879
"Measurements of Gravity at Initial Stations in America and Europe." *Coast Survey Report 1876,* 202–337, 410–16.

MS 353 (1576) winter 1879–80
[Peirce *vita*].

MS 354 (748a, S 104). Item 8 winter–spring 1880
Logic. Chapter I. Thinking as Cerebration. [Cf. P 167, MSS 349 and 350.]

MS 355 (Marquand Papers). Item 14 winter–spring 1880
A large number of repetitions of similar trials.

MS 356 (1537, 747, 1095) winter–spring 1880
[Notes on Probability].

MS 357 (700) winter–spring 1880
[Examination Questions on Probability].

MS 358 (National Archives) *5 March–22 May 1880*
[York Time Computations].

P 177 9 March 1880
"On Kant's 'Critic of the Pure Reason' in the light of Modern Logic." *Johns Hopkins Univ Circulars* 1:4 (April 1880): 49. [Abstract.]

MS 359 (1600) spring 1880
[Notes toward a French Academy Paper]. [Cf. P 171.]

MS 360 (L 229) *1 April 1880*
[Logic Course Examinations].

MS 361 (National Archives) *18–27 April 1880*
[Spectrum-Meter Comparison of Scales].

MS 362 (National Archives) *24 May–8 June 1880*
[York Chronometer Comparisons].

P 256. Item 15 *14 June 1880*
"On the Value of Gravity at Paris." *Coast Survey Report 1881,* 461–63. [Translation of article first published in French, "Sur la valeur de la pesanteur à Paris," *Comptes Rendus Hebdomadaires des Séances de l'Académie des Sciences* 90 (Jan.–June 1880): 1401–3 (P 171). See also O 172.]

MS 363 (1330). Item 16 June–July 1880
[On the State of Science in America].

MS 364 (548) summer 1880
Logic of Relatives.

P 215. Item 17 *23 July 1880*
Letter, Peirce to Hervé Faye. *Verhandlungen der vom 13. bis 16. September 1880 zu München abgehaltenen Sechsten Allgemeinen Conferenz der Europaeischen Gradmessung,* 30–32. Berlin: Georg Reimer.

P 179. Item 18 29 July 1880
"On the Colours of Double Stars." *Nature* 22, 291–92.

MS 365 (National Archives) August 1880
Results of Pendulum Experiments. [Cf. P 168.]

P 167. Item 19 September 1880
"On the Algebra of Logic." *Am J of Mathematics* 3, 15–57. [CP 3.154–251. Abstract in *Johns Hopkins Univ Circulars* 1:5 (May 1880): 63. See also O 209 and 221.]

MS 366 (1600) September 1880
*[*Annotated reprint of "On the Algebra of Logic" (P 167)*]*.

MS 367 (1600, 528) September 1880
*[*Annotated reprint of "On the Algebra of Logic" (P 167)*]*.

MS 368 (1600) *15 September 1880*
*[*Printed Letter of Corrections to "On the Algebra of Logic" (P 167)*]*.

MS 369 (1600) September–October 1880
*[*Annotated Reprint of "On the Algebra of Logic" (P 167)*]*.

MS 370 (L 237, 780, 781, 782) fall 1880
*[*Christine Ladd's Notes to "On the Algebra of Logic" (P 167)*]*.

MS 371 (747, 839). Item 20 fall–winter 1880
Chapter IV. The Logic of Plural Relatives.

MS 372 (839) fall–winter 1880
*[*Notes to "On the Algebra of Logic" (P 167)*]*.

MS 373 (574, 1574) fall–winter 1880
*[*Notes on Logical Operations*]*.

MS 374 (574) fall–winter 1880
*[*The 'greater than' Relation*]*. [Cf. P 187.]

P 168. Item 21 October 1880
"Results of Pendulum Experiments." *Am J of Science and Arts,* 3rd ser. 20, 327. [Reprinted in the *London, Edinburgh, and Dublin Philosophical Mag and J of Science,* 5th ser. 10 (November 1880): 387 (P 174). German abstract in O 190.]

MS 375 (339). Item 22 *6 November 1880*
*[*The Logic Notebook*]*.

MS 376 (National Archives) December 1880
*[*On Pendulum Experiments*]*. [Cf. P 237.]

MS 377 (1600) winter 1880–81
*[*Annotated offprint of "Results of Pendulum Experiments" (P 168)*]*.

MS 378 (535). Item 23 winter 1880–81
*[*A Boolian Algebra with One Constant*]*. [CP 4.12–20.]
MS 379 (40) winter 1880–81
Axioms of Number.
MS 380 (41, 278). Item 24 winter 1880–81
The Axioms of Number. [NEM 1:227–29.]
MS 381 (75). Item 25 winter 1880–81
*[*On Associative Algebras*]*. [NEM 3:536–38.]
MS 382 (75, 747, 839). Item 26 winter 1880–81
Notes on Associative Multiple Algebra. [NEM 3:529–33.]
MS 383 (75). Item 27 winter 1880–81
*[*Unequivocal Division of Finites*]*. [NEM 3:533–36.]
MS 384 (1600) winter 1880–81
*[*Annotated offprint of "Gravity at Initial Stations" (P 161)*]*.
MS 385 (National Archives) *10 January 1881*
Comparisons of certain Absolute measures of gravity.
MS 386 (National Archives) *10 January 1881*
*[*Meters A and B*]*.
P 210 February 1881
"A New Computation of the Compression of the Earth, from Pendulum Experiments." *Johns Hopkins Univ Circulars* 1:10 (April 1881): 128. [Abstract.]
MS 387 (National Archives) *19 March 1881*
Testing Stackpole Spectrometer.
P 198. Item 28 31 March 1881
["Jevons's *Studies in Deductive Logic*"*]*. *Nation* 32, 227. [N 1:63–64.]
MS 388 (747, 768, 875) spring–summer 1881
*[*A Theory of Probable Inference*]*. [Cf. P 268b.]
MS 389 (National Archives) *6 June–6 July 1881*
*[*Baltimore Time Determination*]*.
MS 390 (National Archives) *6 June 1881–30 June 1882*
*[*Baltimore Pendulum Computations and Chronometer Comparisons*]*.
MS 391 (National Archives) *15 June–2 July 1881*
*[*Coast Survey Office Time Computations*]*.
MS 392 (National Archives) *30 June 1881*
Width of Mr. Rutherfurd's Rulings. [Cf. P 204.]
P 204. Item 29 21 July 1881
"Width of Mr. Rutherfurd's Rulings." *Nature* 24, 262.
MS 393 (23, 748, 278) summer 1881
*[*On the Logic of Number*]*. [Cf. P 187.]
MS 394 (235) summer 1881
Continuous, simple quantity.
P 186 August 1881
"Comparison Between the Yard and Metre by Means of the Re-

versible Pendulum." *Proc of the Am Assoc for the Advancement of Science 1881,* 20. [Abstract.]

MS 395 (748a) fall–winter 1881
[Logic and the Methods of Science].

MS 396 (748a, 748s). Item 30 fall–winter 1881
Logic; and the Methods of Science. [NEM 3:752–54; incomplete first chapter only.]

MS 397 (748s, 748a). Item 31 fall–winter 1881
Methods of Reasoning. [NEM 4:363–73. Cf. P 296.]

MS 398 (232, 1574). Item 32 fall 1881–spring 1882
Note on the Mouse-Trap Problem.

MS 399 (214, 278). Item 33 fall 1881–spring 1882
Note on 0^0.

MS 400 (788). Item 34 fall 1881–spring 1882
[On Propositions and Syllogisms of Differing Order].

MS 401 (562). Item 35 fall 1881–spring 1882
Note on the Boolian Algebra.

MS 402 (47). Item 36 fall 1881–spring 1882
Proof of the Fundamental Proposition of Arithmetic. [NEM 1:230–31.]

MS 403 (1072, 1065). Item 37 fall 1881–summer 1882
Comparison of the Metre with a Wave-Length of Light.

P 208 1881
"Pendulum Observations." *Coast Survey Report 1879,* 27–29.

MS 404 (1600) 1881
[Annotated offprint of "Sur la valeur de la pesanteur à Paris" (P 171)]. [See P 256.]

P 187. Item 38 1881
"On the Logic of Number." *Am J of Mathematics* 4, 85–95. [CP 3.252–288. Abstract in *Johns Hopkins Univ Circulars* 1:13 (Feb. 1882): 184 (P 222).]

P 188. Items 39–42 1881
"Linear Associative Algebra." *Am J of Mathematics* 4, 97–229. [Abstract in *Johns Hopkins Univ Circulars* 1:15 (May 1882): 214 (O 223).]

a. *[*"Note on the Algebra g_4"*]*, 132.
b. *[*"Note on the Class of Algebras 242^3"*]*, 190–94.
c. "On the Relative Forms of the Algebras," 221–25. [CP 3.289–296.]
d. "On the Algebras in which Division Is Unambiguous," 225–29. [CP 3.297–305.]

MS 405 (1627) *1 January–31 December 1882*
[1882 Diary].

P 220. Item 43 *7 January 1882*
Brief Description of the Algebra of Relatives. Privately printed brochure. Baltimore. [CP 3.306–322.]

MS 406 (L 283) January 1882
[Associative Algebra Notations].
P 226. Item 44 *January 1882*
"On the Relative Forms of Quaternions." *Johns Hopkins Univ Circulars* 1:13 (Feb. 1882): 179. [CP 3.323.]
MS 407 (76) winter–spring 1882
On the Relative Forms of the Algebras.
MS 408 (77, 278, 1573, 75) winter–spring 1882
On the Algebras in which division is unambiguous. [Cf. P 188d.]
MS 409 (National Archives) *11–20 February 1882*
[Baltimore Length Measurements].
MS 410 (National Archives) February–May 1882
Conference on Gravity Observations. [Cf. P 260–262.]
MS 411 (1600) spring 1882
[Annotated reprint of "On the Logic of Number" (P 187)].
MS 412 (38) spring 1882
[Annotated reprint of "On the Logic of Number" (P 187)].
MS 413 (557, 1574). Item 45 spring–summer 1882
[On the Logic of Relatives]
MS 414 (557). Item 46 spring–summer 1882
[On Relative Terms].
MS 415 (1072) spring–summer 1882
[Pendulum Flexure Measurements]. [Cf. P 253.]
MS 416 (1095) spring–summer 1882
[Computations of Earth's Ellipticity]. [Cf. P 254.]
MS 417 (National Archives) *11 April–18 July 1882*
Construction of Peirce Reversible Pendulum.
MS 418 (National Archives) *13 April–20 May 1882*
Micrometric Measurement of Scales.
P 230. Item 47 April 1882
"Remarks on [B.I. Gilman's 'On Propositions and the Syllogism']." *Johns Hopkins Univ Circulars* 1:17 (Aug. 1882): 240.
MS 419 (National Archives) *27 May–31 July 1882*
['Peirce No. 2' Pendulum Experiments].
P 260–262. Items 48–55 May 1882
"Report of a Conference on Gravity Determinations, held at Washington, D. C. in May, 1882. *Coast Survey Report 1882,* 503–16.

a. ["Introduction"], 503.
b. "Letter from Professor Hilgard to Major Herschel," 503.
c. "Reply of Major Herschel," 504–5.
d. "Six Reasons for the Prosecution of Pendulum Experiments," 506–8. [CP 7.13–20. HPPLS 602–6.]
e. "Notes on Determinations of Gravity," by C. A. Schott, 508–10.
f. "General Remarks upon Gravity Determinations," by John Herschel, 510–12.

g. "Opinions concerning the Conduct of Gravity Work," 512–16.
h. "Resolutions," 516.

MS 420 (National Archives) *7–12 June 1882*
[Baltimore Pendulum Experiments].

MS 421 (National Archives) July 1882
[Force of Gravity]. [Cf. P 259.]

MS 422 (National Archives) *2 August 1882*
Centimetre Scale, 1882, A.

MS 423 (1081) *9–17 August 1882*
[Hoboken Pendulum Experiments].

MS 424 (1082) *6–13 September 1882*
Pendulum Experiments at the Observatory of McGill College, Montreal.

P 225. Item 56 September 1882
"Introductory Lecture on the Study of Logic." *Johns Hopkins Univ Circulars* 2:19 (Nov. 1882): 11–12. [CP 7.59–76. HPPLS 940–44.]

MS 425 (747, 1580, 588, 839) fall 1882
[Logic Lecture Fragments].

MS 426 (839, 1580) fall 1882
[On Multiple Algebras]. [Cf. P 224.]

P 224. Item 57 18 October 1882
"On a Class of Multiple Algebras." *Johns Hopkins Univ Circulars* 2:19 (Nov. 1882): 3–4. [CP 3.324–327.]

P 218. Item 58 October 1882
"On Irregularities in the Amplitude of Oscillation of Pendulums." *Am J of Science and Arts,* 3rd ser. 24, 254–55. [Spanish abstract in *Cronica Cientifica* 6 (25 Oct. 1883): 447–49 (P 241). Review in O 246.]

MS 427 (747). Item 59 fall–winter 1882
[On Junctures and Fractures in Logic].

MS 428 (416) fall–winter 1882
Note on a Limited Universe of Marks. [P 268c.]

MS 429 (588, 747) November–December 1882
Preface [to *Studies in Logic*]. [P 268a.]

MS 430 (National Archives) *30 November 1882*
[Savannah and St. Augustine Longitude Determinations].

MS 431 (National Archives) *30 November–3 December 1882*
[St. Augustine and Savannah Longitude Determinations].

MS 432 (National Archives) *30 November–3 December 1882*
[Savannah Time Observations].

MS 433 (National Archives) *16–18 December 1882*
[St. Augustine Time Observations].

MS 434 (National Archives) *16 December 1882–10 January 1883*
[St. Augustine and Savannah Longitude Determinations].

L 294 (Mitchell Papers). Item 60 *21 December 1882*
Letter, Peirce to O. H. Mitchell.

P 242 December 1882
"Preface to Contributions to Logic by Members of the Johns Hopkins University. C. S. Peirce, Editor. (Little, Brown & Co., Boston, 1882)." *Johns Hopkins Univ Circulars* 2:20, 34. [Cf. P 268a.]

MS 435 (278, 839, 1574) 1882
*[*Multiple Algebra Notes*]*.

MS 436 (1583) 1882
*[*Outline of Treatise on Logic*]*.

MS 437 (834, 278) 1882
*[*On Degrees of Knowledge*]*.

MS 438 (747) 1882
*[*On Logical Universes*]*.

P 237 1882
"Pendulum Observations." *Coast Survey Report 1880,* 19–20.

MS 439 (1059) 1882
*[*Star List*]*.

MS 440 (National Archives) 1882
*[*Report on Pendulum Observations*]*. [Cf. P 252.]

MS 441 (1095, 1062) winter 1882–83
*[*Notes on the Short Pendulum*]*.

MS 442 (1512) winter 1882–83
*[*Craig's *A Treatise on Projections]*.

MS 443 (749). Item 61 winter–spring 1883
*[*Beginnings of a Logic Book*]*.

MS 444 (789). Item 62 winter–spring 1883
*[*On Propositions*]*.

P 268. Items 63–66 1883
Studies in Logic, By Members of the Johns Hopkins University. (Edited by C. S. Peirce.) Boston: Little, Brown & Company. [Reprinted Amsterdam: John Benjamins, 1983. Reviews, of original publication, in O 248, 250, 265, 278.]

a. "Preface," iii–vi. [Cf. P 242, MS 429.]
b. "A Theory of Probable Inference," 126–81. [CP 2.694–754, without maps.]
c. "Note A: On a Limited Universe of Marks," 182–86. [CP 2.517–531, with 1893 revisions. See MS 428.]
d. "Note B: The Logic of Relatives," 187–203. [CP 3.328–358.]

MS 445 (1064). Item 78 February 1883
Additional Note on the Method of Coincidences. [Cf. P 255.]

MS 446 (National Archives) *13 March–12 April 1883*
*[*Smithsonian Pendulum Experiments*]*.

MS 447 (National Archives) *13–25 April 1883*
*[*Smithsonian Pendulum Observations and Computations*]*.

P 245. Item 67 April 1883
"A Communication from Mr. Peirce." *Johns Hopkins Univ Circulars* 2:22, 86–88. [CP 3.646–648.]

MS 448 (1095) spring 1883
*[*Ebensburg Gravity Determinations*]*. [Cf. P 290.]
MS 449 (1095) spring 1883
On an Error in absolute determinations of gravity by the Reversible pendulum.
MS 450 (1095, 1062). Item 68 spring–summer 1883
A Problem relating to the Construction of a reversible pendulum.
MS 451 (1062) spring–summer 1883
Plan of a New Reversible Pendulum.
MS 452 (Gilman Papers) *16 May 1883*
*[*Notice of Forty Lectures on Logic*]*.
MS 453 (1310) May 1883
*[*Transcription of Petrus Peregrinus' *Treatise on the Lodestone]*.
[HPPLS 66–95.]
MS 454 (National Archives) *19 June–3 July 1883*
*[*London Comparisons of Yards 57 and 1*]*.
MS 455 (National Archives) *19 June–12 July 1883*
*[*London Comparisons of Yards 1 and 58 with Yard 57*]*.
MS 456 (National Archives) *19 June–2 August 1883*
*[*London Thermometer Comparisons*]*.
MS 457 (National Archives) *6 July–1 August 1883*
*[*London Comparisons of Yards 57 and 58*]*.
MS 458 (745, 1580, 278) summer 1883
*[*Syllabus of Fifty Lectures on Logic*]*.
MS 459 (745). Item 69 summer 1883
*[*Syllabus of Sixty Lectures on Logic*]*. [NEM 3:1096–1109.]
MS 460 (1575) summer 1883
*[*Notes on Joannes Taisnier's *Opusculum . . . de natura magnetis]*.
MS 461 (1541, S 104) summer 1883
*[*Notes on a Fourteenth-Century Manuscript*]*.
MS 462 (549). Item 70 summer–fall 1883
*[*Lecture on Propositions*]*.
MS 463 (560, 174). Item 71 summer–fall 1883
*[*Lecture on Types of Propositions*]*.
MS 464 (558, 278, 174). Item 72 summer–fall 1883
*[*From a Lecture on the Logic of Relatives*]*.
MS 465 (174, S 25) fall 1883
Note on a New Rule for Division in Arithmetic. [Cf. P 266.]
MS 491 (746). Item 73 fall–winter 1883
*[*Introductory Lecture on Logic*]*.
MS 492 (1092) November 1883
*[*Annotated copy of Gravity Conference Report (P 260–262)*]*.
MS 493 (National Archives) *17 December 1883–6 January 1885*
Measurement of Lengths of Pendulums.
P 266. Item 74 21 December 1883

"A New Rule for Division in Arithmetic." *Science* 2, 788–89. [Cf. MS 465.]

P 253. Item 75 — 1883
"On the flexure of Pendulum Supports." *Coast Survey Report 1881,* 359–441. [HPPLS 607–11; historical section only, but excluding table and final paragraph.]

P 254. Item 76 — 1883
"On the Deduction of the Ellipticity of the Earth from Pendulum Experiments." *Coast Survey Report 1881,* 442–56. [HPPLS 612–16; only to the Attraction of Water table which is, however, incomplete.]

P 255. Item 77 — 1883
"On a Method of Observing the Coincidence of Vibration of Two Pendulums." *Coast Survey Report 1881,* 457–60.

MS 494 (875). Item 79 — December 1883–January 1884
[Design and Chance].

P 257 — 1883
"Figure of the Earth." *Coast Survey Report 1882,* 4. [Abstract.]

P 264 — 1883
["Memorial Tribute to Carlile P. Patterson"] Coast Survey Report 1882, 563.

MS 495 (1098, 1095) — 1883
[Pendulum Calculations].

MS 496 (1175) — 1883
Examples of Mathematical Definitions suitable for Imperial Dictionary.

MS 497 (1244) — 1883
[Words in A from the Imperial Dictionary].

MS 498 (Houghton Library) — 1883
[Annotated copies of Studies in Logic (P 268)].

MS 499 (269) — 1883–84
[Bibliographical Notes on Algebraic Theorems].

MS 500 (L 91) — 1883–84
[On Hardy's Noddy].

MS 501 (1095) — 1883–84
On the Mutual Influence of Two Pendulums.

MS 502 (1095) — 1883–84
[On the Oscillation of Two Pendulums on the Same Support].

MS 503 (748). Item 80 — winter–spring 1884
[On the Teaching of Mathematics].

MS 504 (National Archives) — *12 February–29 April 1884*
[Coast Survey Office Pendulum Observations].

MS 505 (868) — spring 1884
[On Science and Religion].

Essay on Editorial Method

Like the first three volumes of *Writings of Charles S. Peirce,* Volume 4 1879–1884 (*W*4) presents a wide range of topics and forms. As a consequence, the techniques of editing the two general kinds of materials, published and unpublished, vary according to the nature and demands of particular documents. Forty-four of the eighty items in the present volume appeared during Peirce's lifetime in eleven different publications, each with its own house style; and the copy-text of eight of the eighty items was prepared on a typewriter.

Thirty-two items in this volume are published in toto for the first time. (Item 79 appeared previously in an unpublished dissertation.) Peirce wrote exclusively by hand twenty-eight items included here, in addition to the typewritten documents and his offprints, many of the former and some of the latter of which he altered with his pen. Though many manuscript pages have had to be reassembled, most of them are in good physical condition. But some frayed, rough, or torn edges, brittle sheets, soiled pages, water smudge damage, and ink smears, spots, or blots do appear. However, reconstruction of Peirce's text has not been problematic in most instances, although in some of his manuscripts there are leaves that have not been located and perhaps are not extant. In only one case was it necessary to restore one character.[1]

Some of the documents, however, do pose problems that stem mainly from Peirce's working methods. Though he published more than half the eighty items, he made alterations in offprints of eight of them later on. A handwritten or typewritten source is the basis for the text in five additional items published during Peirce's lifetime. The problems posed by these thirteen items that have more than one extant authorial form of the text are discussed at length below. Eight of the thirty-six unpublished items are untitled fragments, ten are untitled but relatively complete manuscripts, and eighteen (including item 60, a letter from Peirce) are manuscripts that have titles designated by Peirce and/or the editors but remain either unfinished or unpolished.

A number of Peirce's manuscripts consist of what at first appear to be

[1]Items 3, 6, 59, 62, 71, 72, and 78 have missing pages. The restoration is in item 24.

different "drafts" of a single item. But closer examination suggests otherwise. Besides occasionally generating genuine "drafts" or revised leaves for an earlier version, Peirce generally was inclined to start again and thus to create corresponding or related manuscript material that may repeat the title, or have the same or similar opening sentence, but then diverges from the direction of the first to make different versions of one work. These discrete versions, separate forms of one textually related item, are less problematic here than they were in the first three volumes. But sometimes the evidence does not demonstrate the sequence in which Peirce composed the discrete versions, which leaves each one a potential choice for publication. Five of Peirce's surviving manuscripts published here continue to illustrate his practice of composing more than one discrete version of a paper,[2] and most of the others reflect his editing or altering with excisions, additions, interpolations, transpositions, false starts, unfinished revisions, and memos to himself. The majority of his manuscripts in *W4* are "working copies." Though he continues to strive for clarity of meaning, and felicity of style,[3] and though his spelling and punctuation are generally accurate, occasionally he still omits letters from words. At other times, and especially in fragments, he repeats a character or syllable. In the relatively complete but unpolished items, editorial emending has been somewhat less necessary. Peirce clearly has become even more conscious of the value of details in spelling and punctuation, as well as of the importance of precise choices for words from several possibilities. Relatively little punctuation, such as a missing dash, parenthesis, or quotation mark, has needed emendation, for Peirce has generally taken pains to ripen his work and thus forestall intervention or unwanted pruning.

The first step in editing and preparing manuscripts for publication is the transcription process. To assure readers that they have before them what Peirce actually wrote, certain transcription guidelines based on editorial theory and on the available working materials are followed.

The Peirce Edition Project owns a complete set of the Harvard microfilm copy of the Charles S. Peirce Papers deposited in the Houghton Library of Harvard University, and two sets of photocopies of nearly all the manuscripts contained in the microfilm. We also own photocopies of all other letters and manuscripts deposited in other libraries and archives. A physical description of each manuscript is included with our photocopies. The legibility of our photocopies is generally good, partly because Peirce used either black or dark blue ink as his basic writing medium. When he used graphite pencil or different colors of ink and pencil for revisions and annotations, the legibility of our copies lessens markedly. After the initial transcriptions are made from the photocopies, in which process the microfilm is often consulted, they are

[2]Item 6, "On the Algebraic Principles of Formal Logic," is exemplary of Peirce's methods of work. The two versions of "Logic. Chapter I" (items 7 and 8) illustrate the same work habits over a longer time period. Items 45 and 78 also contain discrete version material.

[3]Item 37 is an excellent example of Peirce's active refinement of verbal distinctions, in this case for numerical orders. Items 16 and 79 have the only undecided choices of a paragraph and a phrase in *W4*.

team-proofread twice by two different editors against the photocopies; passages that are difficult to read are marked. One of the editors then rereads the transcripts aloud to a second person against the originals in the Houghton Library (or another depository), paying particular attention to the "questionable" and "difficult to read" passages. At that time such features as holes, ink blots, or colored pencil contained in the originals but not distinguishable on the copies are noted. The transcripts are then revised to coincide with the original manuscripts.

During the process of transcription, Peirce's incomplete revisions, misspellings, misplaced or omitted punctuation, and the like are typed as they appear on the manuscript page. Material that Peirce crossed out is omitted, as is accompanying punctuation that he failed to delete; his careted-in revisions are inserted, passages he marked for transposition are transposed, and his instructions for moving large blocks of material are followed. For the most part, Peirce clearly marked such revisions and instructions. When he did not, the following guidelines for resolving difficulties are used.

If the context permits, Peirce's uncertain revisions and marginal annotations are inserted into the text. As both are instances of editorial intervention, they are recorded in the Emendations, and an accompanying Textual Note indicates the original placement of the material on the manuscript page. When the revision or marginal annotation cannot be incorporated into the text without creating an incoherent reading, it is reproduced in the Textual Notes, again with an explanation of its placement on the manuscript page. What the reader will not find reproduced in the text or in an emendation are Peirce's own deletions, page numbers, and doodles, or annotations by other persons. Peirce's instructions are followed but not reproduced.

Determining copy-text for Peirce's unpublished autograph manuscripts is generally unproblematic. As his discrete versions cannot be collated, each theoretically stands as a separate item as would any other single-text item. Where discrete versions have essentially the same content, we have chosen for publication the version that is most carefully written, fully developed, and best argued. But no limit is imposed on the number of variant texts that might be published. Though another version may be referred to for helpful information, each version serves as its own copy-text, and no other version is considered as having any authority for that one.

Most of the items in this volume that Peirce published during his lifetime pose few problems regarding copy-text, though the four 1881 *American Journal of Mathematics* selections were republished in 1882 by Van Nostrand. All others, even those printed more than once, present no editorial difficulties for choice of copy-text. The cases where there *are* difficulties regarding choice of copy-text consist of items in which Peirce's working methods in his offprints present problems (as in items 19 and 48–55) or those for which documents other than the original publication serve as copy-text (as in items 4, 5, 12, 29, and 63). These exceptions will now be discussed.

Item 19. "On the Algebra of Logic" (P 167)

In an undated letter, probably from Baltimore in about November 1879, Peirce wrote to his father that he "was getting up a logical paper for the next no. of the Journal of Mathematics. I am working very hard over it. It is to be quite long."[4] He needed to "consult certain books" for his "memoir on Logic now going through the press," he wrote a librarian at Baltimore's Peabody Museum Library on 16 March 1880. In another note of the same day or of 18 March (when the library received the note that Peirce may have misdated 16 March during his bout with bronchitis), he requested three books from the same library, stating "my memoir goes out of my hands today." He and J. J. Sylvester, editor in chief of the *American Journal of Mathematics* (AJM), sailed for Europe on 28 April 1880. In writing the work, Peirce had "complied with" Sylvester's request "to abridge his most valuable memoir in order that the proper limits might not be exceeded and *above all* that the publication of the number that was due *might not be delayed.*"[5] According to a note from the press to Gilman, the number still was not out as late as 27 July. Because of his father's illness, Peirce returned to America in early August. He wrote to Gilman from Cambridge on 19 August and projected that "This paper which is appearing in the Journal will probably be in 3 parts and will cover over 100 pages. The first part appears in the number which is nearly ready."

On the cover sheet of MS 366, an offprint of his article which he received between late August and early September 1880, Peirce inscribed "Corrections not final," and entered his provisional changes. He had received "extra copies of . . . [his] paper on the Algebra of Logic, published in the Mathematical Journal. . . . [T]he paper was chiefly intended for use in . . . [his] classes."[6] On another of these offprints, MS 367, Peirce wrote "Working Copy" on the cover sheet, and he noted that "This copy contains the corrections to be entered in the others," below which he listed his changes. On the cover sheet of MS 367 Peirce inscribed his alterations, and on pages 17 and 18 he made additional changes. His reference to "the others" is probably to the other offprints used in his logic classes, for on the cover of MS 369, on which he wrote "Lecture Copy" in the upper right corner, Peirce listed the names of five of his students below, then eight students' names below "Advanced Class," and three along with two blanks below "Elementary Class." He altered his "Lecture Copy" MS 369 on yet another page, and between pages 36 and 37 he inserted a folded sheet of additional material, on the verso of which "Feeling" is inscribed and deleted. A short while later he decided to make "notes and corrections [taken from one of the annotated copies, which] should be inserted before reading the article," as he announced to his readers in a privately printed letter, MS 368, dated 15 September 1880:

> I have the honor of sending you herewith the first part of a new memoir by me on the Algebra of Logic. The manuscript left my hands

[4]L 333. Peirce's correspondence is housed in the Houghton Library, Harvard University, unless otherwise noted.

[5]Sylvester to D. C. Gilman, President of the Johns Hopkins University, 22 July 1880. The Gilman Papers, including Peirce's letter to D. C. Gilman, are in the Johns Hopkins University Libraries.

[6]Peirce to D. C. Gilman, 2 April 1882.

> in April last before I had seen several important publications,—Mr. McColl's third paper, Prof. Wundt's Logik, etc.

The copy-text for item 19 is P 167; it is emended by MSS 366, 367, and 369, and by the MS 368 printed letter in which "some very curious and unaccountable errors, one of which is very prominent and important . . . [were] detected and corrected . . . in a new statement before anybody else."[7] The available evidence does not answer whether the letter was inserted by the journal's staff into copies of AJM that had not yet been sent, or mailed to subscribers of AJM by the journal, sent by Peirce, or a combination of such actions.

ITEMS 48–55. *REPORT OF A CONFERENCE ON GRAVITY DETERMINATIONS* (P 260)

Items 48 to 55 comprise the report which Peirce had originally edited but for which he had "never furnished any copy *for the press* . . . [but for his approval] only furnished copy for Herschel,"[8] one of the central participants in the Conference.

> On examining the Report . . . [Peirce continued,] I find that the matter of my copy has got transposed so as to produce confusion, that in one place a line or two of the copy has dropped out, that in other cases there are deviations from my copy. Besides that, . . . before going to press, all sorts of corrections ought to have been made for the printer. I never saw the matter after it returned from Herschel and never read my own proofs.
>
> I now send you a corrected copy. . . .

This is MS 492, an offprint of the *Report* sent to Peirce, who made a number of alterations and wrote "Copy read by C. S. Peirce" on the cover sheet. Another copy which, according to the same letter, he had originally ordered to "be made on the type-writer," since it is unlikely that he typed the entire *Report* himself, has not survived. Having served as printer's copy, the typescript would have perished as foul matter once it had performed its function for the compositor. However, Peirce's original manuscript of "Six Reasons" and an earlier version of his "Opinions" are extant among the documents bound in MS 410 in the National Archives, Record Group 23. Peirce made relatively few alterations in these two manuscripts. His manuscript of "Six Reasons" is dated 1 February 1882, and the Coast Survey Office stamped it as received on 24 February 1882.

MS 410 also contains amanuensis copies of Peirce's "Introduction," Hilgard's "Letter," and Herschel's "Reply," and "Six Reasons," followed by copies of Coast Survey Assistant Schott's "Notes" without a title and an

[7]Peirce to T. C. Mendenhall, 18 December 1891. Mendenhall Papers, American Institute of Physics.

[8]Peirce to J. E. Hilgard, Coast Survey Office, 7 November 1883. In National Archives, Record Group 23, which contains all other cited letters from Peirce to Coast Survey personnel.

incomplete version of the "Conclusions." The amanuensis copy of Peirce's "Six Reasons" adheres to Peirce's autograph copy except where noted in the Emendations and Rejected Substantive Variants. MS 410 lacks the section of Herschel's "Remarks," and its sequence also differs from the published *Report*'s order of the concluding parts: "Six Reasons," Peirce's "Opinions," Schott's "Notes,"and "Conclusions." Edward Goodfellow, an employee of the Coast Survey Office who was officially given the duty of editorial administration in July 1882, wrote on the first page of MS 410: "These papers changed somewhat in form by Ass't. Peirce and sent by Supd. to Assis't. in Ch.[arge] for App.[endix] 22 of '83 [*Report*]."

As early as 31 March 1883, however, Peirce had written to Hilgard that "Some work has been done . . . upon the report of the conference." Between May and September 1883 Peirce was in Europe, and thus was not able to supervise the *Report* to his satisfaction while it went through the press. However, in his 7 November 1883 cover letter with MS 492, the "corrected copy" of the *Report,* Peirce specified the following:

> . . . I should recommend that this be returned to me with directions to prepare it for the printer's hands, that the existing edition be cancelled, and that a new edition be printed . . .
>
> By what I have said above, you might suppose I meant to say that none of the blame of the matter, attached to me. I do not mean to say this. . . .
>
> The square brackets could not be made on the typewriter, and I had intended to insert them in the copy for the press.

He then cited his corrections in detail, and ended with:

> P.S. In the copy I have sent you, I have allowed the communications to remain in their transposed order. I had intended to put them
> Herschel's General Remarks
> Peirce's Six Reasons
> Schott's
> Peirce's Opinions

In his annual report for 1883, which was received in the Coast Survey Office in November 1883, Peirce noted: "Edited pendulum conference report." Peirce used the autograph documents in MS 410 while working on the *Report.* "To be prepared by Assistant Peirce As Appendix No. 22 Report for '82" is inscribed in an unidentified hand across the top of page 1 of MS 410. On the copy of Herschel's 5 May 1882 "Reply" in MS 410, Peirce wrote in the top margin of page 10: "Copyist please start as the page starts and copy rest of paper numbering the 1st page '10'." The MS 410 amanuensis copy of Herschel's letter has been cut and pasted so that the approximately two-line portion "England, and of Basevi . . . opportunities for" (355.11–14) is omitted in the two copies, one made by the first copyist and the other by Peirce's copyist of this part of the letter. However, the published *Report* contains this passage cut from the two amanuensis copies, which suggests that MS 410 provided copy for a typist whose typewritten manuscript then "furnished copy for Herschel," who in turn provided a complete version of the letter for the compositor, save the three paragraphs excised by Peirce.

The copy-text for items 48 to 55 is P 260. It is emended by MS 492, which is Peirce's corrected offprint of the *Report,* by MS 410, and by the published "Errata." MS 410 has authority for thirteen emendations in P 260.

ITEM 4. "NOTE ON THE PROGRESS OF EXPERIMENTS FOR COMPARING A WAVE-LENGTH WITH A METRE" (P 136)

On 3 June 1879, in anticipation of leaving for Europe near the end of August, Peirce wrote to C. P. Patterson, Superintendent of the Coast Survey, from Cambridge, Mass.:

> I want to get a preliminary paper on the spectrum metre in print before I sail. This paper to give a sufficient account of the methods, omitting small details, and also to give the results of measurements in detail. The final result, or at least, the length of a glass metre in terms of the wave-length to be given.

He went on to suggest that the article might be published in the *Proceedings* of the American Academy of Arts and Sciences (where he was to read a paper on the subject on 11 June) and that it could then be used as printer's copy for the *Coast Survey Report.* But a penciled note in an unknown hand at the bottom of the letter states, "Some years ago the Public Printer would not set up what had been published outside of the Public Print Office."

In an apparent effort to satisfy the strain of time and the priority of Peirce's method, and as a compromise solution to the policies of the public printer, a very brief, one-page note on the progress of Peirce's experiments was published in letter form in the July number of the *American Journal of Science and Arts* (AJSA). The text for this article was not, as he proposed in his 3 June letter, "a preliminary paper" giving "at least the length of a glass metre in terms of the wave-length." Rather, it was taken from his hastily inscribed 23 May letter to Patterson, which is an informal progress report. This autograph document, stamped by the Coast Survey Office as received 27 May 1879, is one of many that Peirce sent to the Office at irregular intervals regarding his work, though few were explicitly intended for publication. Patterson and possibly one other person at the Office made pencil alterations on the four pages of the letter. Patterson also added a title, which differs slightly from the AJSA title, and he or someone else deleted the "U. S. Geodetic Survey Service" letterhead on page 1; deleted Peirce's first-page date line and reinserted it after the close and Peirce's signature; and, after an abortive effort to alter the final two single-sentence paragraphs, deleted them altogether. Also, there is a change in the last sentence of the third from last paragraph. These pencil revisions attempted to turn a semi-private, semi-official letter into a public statement.

Following usual procedure, the Coast Survey Office undoubtedly made a fair copy of the revised letter and sent that as printer's copy to AJSA. No printer's copy is extant, but collation of the AJSA text and Peirce's edited letter reveals that the text of the published note follows verbatim the pencil-revised letter with two exceptions: a correction (at 11.20), possibly incorporated into the fair copy, and a repetition of data (at 11.5) due to double-column printing.

In addition to the two substantive differences above, there are forty-eight accidentals in the AJSA printing that vary from Peirce's letter. It is possible that some of these were added in the fair copy; the most likely additions are the five ampersands changed to "and," three periods added to abbreviations, and three symbols added for consistency. Peirce would probably have made these eleven changes if he had prepared the fair copy himself. The present editors have accepted the emendation of ampersands made in AJSA and have made other changes that Peirce had expected, as he noted in his letter of 23 October 1873 to the Coast Survey Office regarding the advance printed copy of P 77: "The punctuation has had no attention in the proof reading. Had I supposed it would not have, I would have taken pains to have it right in the MS. But it has never been my habit to do that as proof-readers attend to it."[9] The remaining accidental variants all point to general house styling and other changes made in an effort to conserve space. The AJSA printing, which fills a single page, runs two lines longer than its average page.

The above collation commentary concerns only the body of the letter. The inside address, salutation, and closing have been altered and rearranged in AJSA to conform to house style. There are no pencil revisions of these portions of the letter that indicate what was or was not changed in the fair copy, with the exception of the above-noted date line, which does not appear in AJSA. It was probably deleted by AJSA because of lack of space. Peirce more than likely would have included it.

Because of the close substantive correlation between the letter and its AJSA printing, and because of the many accidentals identified as not originating with Peirce, the letter itself is the copy-text. Coast Survey Office pencil changes necessary for publication, and the revisions within the body of the letter which appear in the AJSA printing, and which likely were present in the fair copy, are accepted as emendations. In addition, two emendations of "coefficient" are added, since this was Peirce's hastily written letter not prepared for publication. His customary arrangement of inside address, salutation, and closing are emended by the AJSA format for them, but the deleted date line is reinstated.

ITEM 5. "ON A METHOD OF SWINGING PENDULUMS FOR THE DETERMINATION OF GRAVITY, PROPOSED BY M. FAYE" (P 137)

In his 17 February 1879 cover letter to Superintendent Patterson from Pittsburgh, Pa., Peirce wrote:

> I enclose a report which I have written with the object of pleasing M. Faye. I think the best way to have it printed would be in Sylvester's

[9]See also *W*3:558–59.

> Journal (altering its form slightly) provided that will not involve much delay. I would suggest your sending it to him or to Mr. Story and I will myself write to Mr. Sylvester. If they can't print it before summer, or if they think it travels out of their beat, then I suggest its being separately printed. I might perhaps abridge and translate it and send it to Faye for insertion in the Comptes Rendus. I can consult Sylvester about that . . .
> P.S. I will have figures properly drawn.

Bound in directly following this letter is the report-in-letter-form, neither of which is marked "received" by the Coast Survey. All evidence suggests that the report-in-letter-form document was Peirce's final version of what was to go to the compositor, for such documents were often used to prepare reports for publication by the Coast Survey. Peirce carefully inscribed his letter-report document, which was copy-edited and set up for the Government Printing Office house style. The letter-report was then recopied according to the usual practice at the Coast Survey, for preceding it in Patterson's hand is this note: *"Mr. Hilgard*—Insert it [what is extant and now in the National Archives as distinct from the discarded amanuensis copy and new diagrams] all with copy & diagram made of the paper to send to Silliman's Journal, as soon as possible?-CPP." Then there is a notation by Hilgard: *"Mr. Schott*[.] Please execute directions of Supt—JEH," which Schott initialed. This suggests that the extant letter-report (which incorporates three roughly-drawn diagrams), its bound-in two pages of additional diagrams carefully drawn by someone else, an amanuensis copy of Peirce's report, and a third set of yet more detailed drawings were all inserted together and sent to AJSA for them to use to prepare the article for the compositor. The amanuensis copy and third set of diagrams were lost as foul matter. The rest survives because it never went to the compositor; it was used by the AJSA staff to prepare the article and then was returned to the Coast Survey. There is no record indicating that Peirce read proof for his article. The transposing of the redrawn diagrams with the inappropriate text in AJSA, almost certainly done by its staff, suggests that he did not see proofs at all. He would have had ample opportunity while in Washington to do some revising, however, before the article was sent to the compositor. This accounts for some of the variation between the article as printed in AJSA and the letter-report, which does not have material like the change of the formulae that follow "Suppose . . . the two pendulums are vertical at once" (17.27–18.1).

The problem of the transposition of the order of the diagrams to which the descriptive text refers in AJSA stems from the following. Peirce had sent this 21 March 1879 telegram from Cambridge, Mass., which was stamped "received" in the Coast Survey Office the same day: "Diagrams forwarded today by mail." Two additional pages of carefully done "figures properly drawn," as Peirce had promised in the postscript to his cover letter to Patterson, are bound in with the letter-report. There are directions not in Peirce's hand on the additional two pages. Below the "wave" diagram is:

"This may be reduced a little—say one fifth in photographing on blocks." Above the diagrams of the stand is: "May be slightly reduced—the two figures should be brought more closely together as shown by brackets—." These drawings differ from the diagrams roughly drafted on the pages with Peirce's manuscript text. AJSA printed only two diagrams (the "wave" diagram and the top view of the stand, both newly redrawn), but Peirce's letter-report has three drawings: the wave diagram, a top view of the suggested construction of a pendulum stand, and an accompanying side view of the stand. The AJSA "wave" diagram is similar to those found in and filed with the letter-report. The AJSA printed diagram of the top view of the stand shows a different arrangement and construction of the legs with the cross-braces replaced by support-braces in the new drawing. Only the top view was specifically mentioned in the text, and thus the side view was not published. Sometime during the process of preparing copy for AJSA, its staff probably misnumbered and transposed the diagrams. The pendulum stand diagram in AJSA is even more detailed and meticulously drawn than the two earlier versions, and the wave diagram is also more refined.

Collation points to over twenty substantive variants between the letter-report and the AJSA printing, excluding the changes from letter format to printed article, such as inside address, salutation, closing, as well as the added title. All substantive changes (except what is probably a compositor's error at 13.32, and the incorrect diagrams), including a rewritten paragraph and an added paragraph incorporated into AJSA, are accepted. Peirce's rough drawings in the body of his letter-report are rejected in favor of the AJSA figures in their correct order. Refined versions of the original drawings appear in the Textual Notes. There are over eighty changes in accidentals, excluding readings in the rewritten paragraph and in the inserted paragraph in AJSA. Two additional commas are the result of a Coast Survey Office substantive change and are accepted with the substantive. Two are the result of Peirce's alterations in his manuscript. They were probably included in the recopying process and thus are accepted. Three appear to be misreadings by the copyist. One of these also resulted in a corresponding substantive change; these are all rejected. Four are the result of substantive changes incorporated into the AJSA printing and are accepted. Also accepted are two expansions of arabic numerals to words and one exchange of the symbol ℈ for π, both of which correct Peirce's inconsistency, and eleven corrections of oversights by Peirce. All but one of the eight paragraph indentions or nonindentions probably made by AJSA are rejected, though meaning or conservation of space does not seem to have been a consideration. Of the seven (probably AJSA) spelling and hyphenation changes, two are accepted, five rejected. All sixteen italicizations of "d" are rejected from the AJSA printing, for Peirce was careful to underline all roman alphabetical symbols in mathematics and logic (with the two exceptions at 16.10(2) and 16.11(2, numerator)) but these. Thirty-four instances of changes of Peirce's usual practices to house style are rejected. Consequently, Peirce's letter-report is the copy-text. It is emended by the Coast Survey Office editing in pencil and by the AJSA printing.

ITEM 12. "NOTE ON THE THEORY OF THE ECONOMY OF RESEARCH" (P 160)

Peirce first mentioned this paper in an 18 November 1876 letter to Superintendent Patterson:

> I will now at once go to work on the Appendices which I expect to furnish—viz:— . . .
> 4. On the economical distristribution [*sic*] and arrangement of observations.
>
> This last has not yet been submitted to you, although it has been ready for a long time. But I thought of adding something to it or of rewriting it.

In a 14 May 1877 letter to Patterson, sent from New York, Peirce noted: "I enclose a paper for the Report on the Economy of Research." The paper was accepted for publication, for on 18 May Peirce wrote Patterson, "Thank you for inserting the Note on the Economy of Research in the '76 report." In the top left corner of MS 307, *"Copied A.D. June 4, '77"* is inscribed in purple ink in Edward Goodfellow's hand. Across the margin in pencil is *"Appendix Report '76"* in an unidentified hand. Goodfellow inscribed in pencil in the top right margin: "(Notes and copy *retained by E. G.*)." Peirce's enclosed paper had been copied by an amanuensis by 4 June, and MS 307 along with the amanuensis copy was retained by Goodfellow at the Coast Survey Office. Peirce was in New York then, and there is no indication that he went to Washington before leaving for Europe in September 1877. His "Note" seems to have lain dormant until 1879, when on 10 February he wrote Goodfellow from Pittsburgh: "I herewith return Appendix No 14 of the '76 Report after revision as requested." (Apparently, Peirce did not return to Washington until just prior to the National Academy of Sciences meeting on 15–18 April 1879.) Goodfellow noted on Peirce's letter, which is stamped received by the Survey Office 11 February 1879, "Rec^{d} App[endix] 14—revised & partly re-written G[oodfellow] 11th." No transmittal letter from Goodfellow or anyone else at the Coast Survey clarifies what revisions had been requested. Nor is it clear as to whether the 1877 amanuensis copy, galleys, or even page proofs were sent to Peirce. No corrections were made on MS 307, even though it is apparent from the substantive change at 74.6–7 that Peirce did have MS 307 in his hands in 1879.

Collation indicates that Goodfellow's notation was accurate regarding revision and rewriting. Peirce revised his paper, and completely rewrote the ending so that the example of a practical application of the theory of economy of research would be more recent. The rest of the substantive revisions tend to be single words or phrases which either clarify a point or enhance Peirce's style. Twenty-seven substantive alternatives were incorporated into the printed version. The two major revisions, an insertion of a new five-paragraph section at 74.26–75.24 and a totally rewritten ending, are accepted. Five substantives are the result of either a misreading by the amanuensis or the compositor, or of dropped type, or of typographical errors, and thus are rejected. Ten substantives are revisions of a clarifying nature, though four of these may have been Peirce's oversights, and five of them seem to be stylistic changes; all are accepted. One substantive change, dependent on another revision, is also accepted. Of the nine other possible emendations made for stylistic purposes, which are not atypical of the kinds of changes Peirce is tending more and more to make, eight are accepted and one is rejected.

Sixty-five accidental variants appear in the *Report.* Two capitalizations have been lowercased, two needless spelling changes were made to accord with house style, one indention of a very short paragraph was deleted; two letters were italicized probably by the compositor or the copyist. All the above are rejected as contrary to Peirce's intent. Three emended punctuation marks were required by accompanying substantive changes; four of Peirce's oversights in spelling, punctuation, or italicization were corrected; and five punctuation points were added after display equations, the lack of which in MS 307 seems attributable to Peirce's inattentiveness since appropriate punctuation does appear in nine similar cases. All the above are accepted. Forty-five other changes in punctuation are rejected. Some of these might be regarded as "corrections," but they reflect a styling by someone other than Peirce. None is needed for clarity, and many of them alter Peirce's usual cadence or phrasing. Consequently, MS 307 is the copy-text; it is emended by the published article in the *Report.* In addition, it is notable that second proofs of pages 202–16 of Appendix 15 were received from Peirce and "sent to [the] Printer May 10. '79" according to a memo following the list in the National Archives, Record Group 23, regarding the illustrations in that Appendix Report. Memos inscribed on Peirce's 26 April 1879 letter to Hilgard indicate that "pp. 202–215—1st proofs ret^{d} to printer May 2 | 216 retained | 2nd proof 202–215 to CSP for revision | May 5 | also p 216 1st P," and that the "Manuscript of App 15 . . . [was] sent to CSP . . . May 10th," all of which suggests that Appendix 14 was set in pages before about early April 1879.

ITEM 29. "WIDTH OF MR. RUTHERFURD'S RULINGS" (P 204)

Peirce wrote Superintendent Patterson from Baltimore on 30 June 1881: "I enclose for publication a statement in regard to the average width of Rutherfurd's rulings. I suggest that it be sent to *Nature.*" The letter is stamped as received on 1 July 1881, and in the top margin "Ack[nowledge] & will be sent to Nature" is pencil-inscribed in Patterson's hand, as is "A fair copy to be made," which is in the bottom half of MS 392. Peirce made two substantive alterations in ink in this typewritten document which he enclosed with his letter. The fair copy is not extant. MS 392 has five substantive alterations not in Peirce's hand, which are rejected as intrusions upon his style of writing. MS 392, the copy-text, is emended by the Coast Survey inscriptions, and by *Nature* in the case of the title and spelling of "Rutherfurd." The other variants in *Nature* are rejected as house stylings without basis in evidence of authorial intent.

Additional problems regarding this paper grow out of Peirce's use of the typewriter. The present article is one of the earliest extant typescripts in the Peirce Papers. As early as 1847 a model typewriter had been invented, and by 1856 a considerably improved machine had been developed, its patent expiring so that it became public property in 1870.

Peirce was to furnish typewritten copy for his editing in 1883 of the *Report of a Conference on Gravity Determinations*, which would have been prepared by a typist for the Coast Survey. But even in 1881 he himself had actually typewritten the half-page of single-spaced lines beginning "WIDTH OF MR. RUTHERFORD'S RULINGS." The "O" in "RUTHERFORD'S" was corrected by hand in the Coast Survey Office. Peirce made other autograph revisions in this typescript. But his insertion of the diacritical marks for the Danish "Ångström" and of other characters which his typewriter could not make points to the nature of the machine he used in June 1881 to type his article. It is the same machine on which he typed his 13 May 1881 letter from Johns Hopkins University in Baltimore to the Disbursing Agent of the Survey in Washington. Not until 14 September 1884 from Afton, Va., did Peirce express a formal written "desire [for] permission to purchase a typewriter for . . . [his] work. I have considerable practice with one, and if you [J. E. Hilgard] do not object, will get one out of my appropriation." By 14 August 1885 his Survey party was using the typewriter to make "manifold" or carbon copies. In fact, his own 13 May 1881 letter is the copy "in manifold" that he retained for his records. MS 392 Peirce may have "written in manifold on a type-writer," as he termed carbon copying in his 14 August 1885 letter to Superintendent Thorn. Or he may have had it fair copied by typewriter from an autograph document, which he might have retained for his records; neither it nor any carbon copies have survived.

ITEM 63. *STUDIES IN LOGIC* "PREFACE" (P 268a)

MS 429, Peirce's autograph "Preface," is folded in thirds as though it had been sent in an envelope through the mails. The soiling, black smudges, and black fingerprints on its five sheets suggest that it is the printer's copy that Peirce prepared probably sometime in the latter part of 1882, since *The Johns Hopkins University Circulars* text of the "Preface" has "December 1, 1882" printed opposite Peirce's name, while the Little, Brown text is dated "Dec. 12, 1882." The *Circulars* for September 1882 also had this announcement: "A volume of LOGICAL CONTRIBUTIONS, by members of the Johns Hopkins University, will be published early in the autumn." In the top left corner of MS 429, "Pica" is pencil inscribed, probably in the compositor's hand. Peirce's fragmentary pre-copy-text version of the "Preface" also has survived, which makes this item unique in *W4* in the sense that it is the only one for which there is a pre-copy-text version, a printer's copy, a serial printing, and a book publication. The circular gives 1882 as the publication date for *Studies in Logic,* but the book itself has the copyright date 1883. Collation of the circular and the book indicates that, despite the datings of the two prefaces, the circular was typeset from the book pages. MS 429 is the copy-text; it is emended by a pre-copy-text leaf, by the book, and by the circular.

As mentioned earlier, because of the two types of materials included in this volume, namely previously published and hitherto unpublished manuscript or typescript items, a variety of editorial approaches is required. With published items, the editing focuses on the effort to locate and eliminate editorial or printing corruptions and thereby to restore Peirce's original intentions. With autograph manuscript items, the focus is on reproducing Peirce's written texts as accurately as possible and emending the text on the basis of the evidence contained within the manuscript itself. At times, of course, Peirce will misspeak himself or produce a passage that is confusing or vague. In such cases there is emendation only in public documents and when his intent is ascertainable. When such certainty is lacking, his eccentricities and anomalies are allowed to stand the way he wrote them, and when necessary explained in a note. The following is a discussion of the guidelines used in emending and restyling the texts of Peirce's private and public documents.

Peirce's writings are presented in a clear reading text. His published works are emended when there are errors or inconsistencies, or when the evidence suggests that previous editorial decisions have overturned Peirce's intentions. The unpublished writings require a different editorial approach. They are distinguished as either private documents or public documents, and then edited accordingly. Private documents include letters, drafts of letters, journals and notebooks, and a few manuscripts. None was ever intended for publication, nor did Peirce ever intend them for public presentation or consumption. Their content or ideas were for personal use only.[10] All other documents, even early drafts, work sheets, or personal working copies of writings that were later published, are considered public. Besides the universal italic printing of lowercase variables, private documents are emended only in the following four instances: spelling errors are corrected and periods are added at the ends of sentences except before the beginning of a new paragraph. Apostrophes are inserted in the plural forms of variables (but not in the possessive forms of proper names or other substantive words), and Peirce's incomplete revisions are completed. No other changes are made in private documents. Public documents include those that conform to genres normally intended for publication, whether or not they are so prepared, and whether they are working copies, early drafts, or outlines; lectures written, whether in outline or fair-copy form, for presentation in lecture courses or at public conferences; and all manuscripts or typescripts that contain topics and ideas that are related to those Peirce published and that he would have been willing to submit for public comment and criticism. Because they are considered public documents in the present edition, they are emended as follows: all spelling and grammatical errors are corrected, incomplete revisions completed, and lowercase variables emended with ital-

[10]Items 14, 22, and 60 are examples of private documents in the present edition.

ics. In addition, all missing punctuation is inserted, and inconsistencies within a document are corrected and noted. Slips of the pen and typographical errors are corrected.

The variant spellings of "premiss" and "premise" are allowed to stand according to Peirce's usage. Other inconsistencies within a public document are corrected. Occurrences of any form of the word "meter" are emended to Peirce's preference for the spelling as "metre" (except in item 13, where "German Normal Meter" is accepted as a proper name). Regarding the spelling of "seconds pendulum," during the period of *W*4 Peirce and his various publishers used alternate spellings: seconds' pendulum, second's pendulum, seconds pendulum, and seconds-pendulum. In Part XV of *The Century Dictionary* (p. 4371), Peirce clearly stated that "a *seconds pendulum* [is] (also written *seconds' pendulum* and *second's pendulum*)." Part XIX of *The Century Dictionary* (p. 5454) also has an entry for "seconds-pendulum." The editors have emended the variant spellings so that the form is consistent within the volume according to Peirce's preference for the spelling as "seconds pendulum."

Acceptable nineteenth-century spelling forms in all languages as well as acceptable variant spellings of proper names are retained. Our standard reference is the *Oxford English Dictionary.* However, Peirce's spelling of "coëfficient" with the dieresis, which is not cited in the *Oxford English Dictionary,* occurs frequently in the manuscripts included in *W*4. There are numerous occurrences in the published items without the dieresis. The "ë" occurs seven times in three published items.[11] The "e" appears six times in two manuscripts.[12] The fewer anomalous instances of "e" to "ë" are emended as Peirce preferred. In all MSS in this period, Peirce inscribed "st," "nd," "rd," and "th" in the superscript position; for convenience's sake, they are on the line in typewritten pieces. In published pieces the ordinals are superscripted to conform to Peirce's style; "2^{d}" and "3^{d}" are emended to "2^{nd}" and "3^{rd}." When Peirce typed abbreviated ordinals on the line, these mechanical exceptions attributable to his typewriter have been changed to superscript ordinals.[13] Peirce also regularly used the nineteenth-century calligraphic convention of double underlining superscript portions of abbreviations such as $M^{\underline{\underline{r}}}$ or $1^{\underline{\underline{st}}}$. In this edition such conventions remain above the line without underlining and without Peirce's inconsistent use of the period. Decimal points are lowered from mid-line to on-the-line; multiplication dots are raised to mid-line from the on-line position. Every ambiguous 3-em dash for an omitted word or phrase is placed mid-line. Ambiguously placed terminal punctuation is given with punctuation outside the single terminal quotation mark and inside the double quotation mark. As Peirce's placement of quotation marks (single *and* double) when used in combination with a comma, colon, or semicolon is not always precise enough to make his intention clear, such

[11]Items 13, 17, and 57 (where, in one of four instances, only one dot is visible).
[12]Item 37 has four, item 45 two.
[13]Items 7, 30, and 31.

combinations are interpreted by present standard punctuation practices. Superscripts and subscripts modifying the same character are stacked vertically rather than diagonally. These procedures are applied to conflicting publishers' styling also.

Peirce was a mathematician and logician, and knew that lowercase variables are to be expressed in italic form: and he insisted on that in publications, although he rarely underlined *all* lowercase variables in manuscripts. He was considerably less insistent on the practice for capital variables. Regarding all lowercase variables, when all of them in an item are in roman they are emended to italic and noted in the Textual Apparatus. When Peirce italicized some and left the rest to be changed from roman, they are italicized and noted in the Emendations. If Peirce was inconsistent in his underlining of lowercase variables, the roman is emended to italic and each instance noted as economically as possible. Exceptions regarding these variables, or any other cited readings, are recorded when all the rest are cited as emended in a document. Thus the original state of each variable can be reconstructed as it was in the copy-text. Capital variables in Peirce's manuscripts are left roman. In the published items, capital variables are allowed to remain consistently italic or roman as the case may be.

Every effort has been made to verify all of Peirce's quotations in editions he is known to have owned or had available, and to identify and verify quotations that are not marked as such in the manuscripts. A quotation is allowed to stand as he gives it, even if it differs from the original. Information on his quotations and their sources may be found in the Notes. Peirce's book and chapter titles and other bibliographical citations are emended to conform to the style of the present edition. When, in referring to his own published writings, Peirce gives specific page or volume numbers, these are replaced with the appropriate page numbers in the present volume. Page and volume numbers are retained when the publication does not appear in the present edition; they are also retained when, as in item 21, some but not all publications referred to appear.

To reduce the great variety of different publishing house styles and to make understandable Peirce's obscure references, title notations are printed to conform to the modern practice of italicizing book and periodical titles and placing chapter and article titles in quotation marks. Lengthy published quotations are restyled to conform to our way of printing extracts. Abbreviations are left as Peirce gave them, with periods added where needed. Periods are also added at the end of sentences where Peirce's pen skipped or he inadvertently omitted them.

Manuscript material requires further editorial intervention from time to time, in most cases because of Peirce's own revision. In the process of revising, he sometimes created grammatical errors or at other times failed to complete his intended revision by crossing out a necessary word or phrase but not going back to replace it. In such instances his original word or phrase is reinstated, along with a citation of each occurrence of it in the Emendations. When he added introductory clauses to already inscribed sentences but failed to lowercase the first word of the original sentence, his misplaced capitalization is emended.

Peirce meticulously collected offprints of his published papers and, as he often corrected and annotated them, they provide an important source of emendation of the text. Further sources for emendation of published items include Peirce's letters, other periodicals, and published errata lists in which he mentions oversights or printer's errors.

All emendations described thus far are listed, and the sources for each are cited, in the Emendations.

In one of Peirce's previously published scientific papers (item 76) and one manuscript (item 37) there are long tables with repeated column headings on successive pages. These headings are moved to accommodate different page breaks. Similarly, Peirce and his printers used ditto marks for repeated data within columns in these tables, with full text printed for the first entry on each page. Appropriate text is substituted for ditto marks, and these are used in place of text when page breaks in this edition differ from the original printing.

Titles of Peirce's previously published items are printed in italics, and unpublished manuscript titles in roman type. Untitled published reviews are given titles, italicized, and set within italic brackets. Titles of the reviewed works themselves in such titles are in roman. Descriptive titles of published letters are in italics, of unpublished letters in roman. All titles and heads appear without periods. Such phrases as "By Charles Peirce" or "C. S. Peirce," which appear at the beginning or end of a published paper or unpublished manuscript, are deleted and noted in the Textual Apparatus. The symbols or page-by-page numbering systems Peirce or his publishers used for footnotes are replaced by a single series of arabic numerals for each paper. In place of varying numbers of ellipsis points to mark omissions in quoted material, standard form is followed. Various printer's conventions for first lines of text or paragraph opening have been dropped, and all items except those that are letters begin with our usual paragraph indention.

Symbols

Within the Text

Italic brackets enclose titles and other text supplied by the editors.

Italic brackets enclosing three ellipsis points indicate one or more lost manuscript pages.

Italic brackets enclosing a part of a word indicate an editorial reconstruction of a damaged document.

Italic brackets enclosing a blank indicate that an incomplete discussion occurs before the end of the manuscript page.

Italic brackets enclosing page and line numbers before and after three ellipsis points indicate omitted published material.

Sets of double slashes mark the beginning and end of Peirce's undecided alternate readings; the single slash divides the original from the alternative inscription.

Within the Apparatus

All page and line numbers refer to the present edition. (Running heads do not count as lines.) Footnotes are counted separately from the text and are indicated by "n" following the page number. Parenthetical numbers following the line number indicate the first or any subsequent occurrence in that line of the keyed reading. If the same emendation occurs more than once in a given item, the additional instances are listed after the word *Also.* The abbreviation *et seq.* after a page and line number indicates that all subsequent readings within an item (except those noted) are like the cited reading.

An asterisk (*) preceding page and line numbers of an emendation indicates that the reading is discussed in a textual note.

A double dagger (‡) preceding page and line numbers of an emendation indicates that Peirce's quotation is corrected, completed, or further discussed in a note.

All readings to the left of the roman bracket are from the present edition, those to the right from the copy-text or other collated texts. In the Emendations, the bracket is followed by an abbreviation or number that identifies the source of the emendation. A semicolon precedes the rejected readings; if there are two or more such readings, they are separated by a bullet (•). The source for all readings in the Emendations and Rejected Substantive Variants, other than the editors (E), is given in the headnote for each item.

The phrase *not present* signifies the lack of corresponding text in the copy-text or other collated source texts.

The abbreviations *ital.* or *rom.* indicate that the reading to the left of the bracket was originally printed in italic type (underlined in manuscripts) or roman type (not underlined in manuscripts or in italic typescripts).

The word *reinstate* applies to manuscript material only and indicates that the reading to the left of the bracket was deleted by Peirce in the original, but that the editors have reinstated it.

The curved dash or tilde (~) stands for the same word or expression to the left of the bracket.

The caret (‸) signals the absence of punctuation, paragraph indention, operational signs (e.g., +, −, or the mid-line multiplication dot), and mathematical or scientific symbols (e.g., degrees, minutes, seconds).

¶ is the sign of a paragraph indention.

A vertical stroke (|) indicates a line-end break and is used to clarify an emendation or rejected substantive variant.

The abbreviations *num.* (numerator) and *denom.* (denominator) are used to distinguish variants in parts of complicated fractions. The word *formula* indicates a reading of an otherwise lengthy citation in which only a punctuation or some other careted variant occurs. The three words are enclosed by italic brackets when they appear to the left of the roman bracket in emendations.

Textual Apparatus

The Textual Apparatus provides (together with the Essay on Editorial Method) a nearly complete record of what has been done in the editing process, and it presents the necessary evidence for the editorial decisions that have been made in this critical edition. It consists of eighty sections, corresponding to the eighty items published in the present volume, and each section contains up to five separate subdivisions. Each of the eighty items begins with its identifying number in the volume and its (running-head) short title. It is followed by an untitled headnote, and by Textual Notes, Emendations, Line-End Hyphenation, and Rejected Substantive Variants. The last four are printed in reduced type, the last three in double columns.

The headnote describes the genesis of the text and the occasion for which it was written. It designates the copy-text and collated variant documents (and their sigla, in square brackets) and their pre-copy-text forms. It provides source information for unpublished manuscript items not deposited in the Harvard Peirce Papers, and information on their dating, and it gives a physical description of the manuscript (including number of leaves, paper size, watermarks, medium of inscription, and so on); it also describes fragmentary pages and other portions of the manuscript not included in the edition.

The Textual Notes discuss and explain in detail readings adopted in the edition that represent complex or interesting textual cruxes. They represent either an emendation or a retention of the copy-text reading and specify why, in certain problematical or anomalous instances, the copy-text has or has not been emended. They explain problematic restorations of missing words or portions of words, describe the manuscript page placement of revisions in the margins or on preceding or following versos that have been incorporated into the text or, when Peirce's final intention in those revisions is unclear, reproduce those revisions that cannot be incorporated.

Emendations provide a record of all changes, in both substantives and accidentals, made in the copy-text to produce the critical text. They record the change or correction of single letters or of words, of mathematical and scientific formulas, and of such accidentals as spelling and punctuation. The matter of emendations and their presentation is discussed in detail in the Essay on Editorial Method and in Symbols.

Line-End Hyphenation is a list of those compounds or possible compounds that are hyphenated at the ends of lines in the copy-text. They are resolved according to known Peirce usage and, consequently, are printed in this edition either as hyphenated words or as single unhyphenated words. (The list of compounds hyphenated at the ends of lines in this critical edition appears as a separate section before the Index.)

The list of Rejected Substantive Variants provides a record of the variant substantive readings in all authoritative versions of the given document. As the name implies, it records only the *rejected* variants, which are not adopted as emendations of the copy-text; *accepted* (and related rejected) variants appear in the emendations.

Two further lists are prepared in the editing process—(1) a full Historical Collations list, which includes all substantive variants and all accidentals (and which yields the published Rejected Substantive Variants lists), and (2) a complete List of Alterations in the Manuscripts—but neither is published in the edition. Both lists are available to interested persons for the cost of photocopying.

1. *Read's Theory of Logic, 1879*

Copy-text is P 148, the publication in the *Nation.* Haskell's *Index to the Nation* lists Peirce as the author of this unsigned review.

Emendations

1.27 all;] E; ~:
1n.1 ‸*The* . . . *Essay.*] E; 'The . . . Essay.
1n.1 Read.‸] E; ~.'

2. *Spectroscopic Studies, 1879*

Copy-text for this note is P 163, the publication in *Science News.* Internal evidence suggests that P 155, the printing in *Nature* 20 (29 May 1879): 99 [155], was abstracated directly from *Science News,* which has "BY CHARLES S. PEIRCE" below the title. The use of the third person resulted from the fact that the paper was "carefully prepared so as to serve as a permanent record . . . and report" of the meeting of the National Academy of Sciences (*Science News* 1 [1 May 1879]: 193), where Peirce presented four papers (P 151–154). All three rejected substantive variants are from P 155.

Emendations

4.4 Diffraction-Spectra] E; ~‸~
4.4 Wave-Lengths] 155; ~‸~
4.13 diffraction-plate] E; ~‸~
5.2 Ste.-Claire] E; St.‸Clair
5.29 glass-plate] E; ~‸~
6.13 millimetre] 155; millemetre

Line-End Hyphenation

6.19 one-millionth

Rejected Substantive Variants

4.6–15 For . . . brighter.] *not present*
4.26–5.11 We . . . finer.] *not present*
5.40–6.10 In . . . distortion.] *not present*

3. *Lecture on Logic and Philosophy, 1879*

Copy-text is MS 342, a fragmentary manuscript consisting of six leaves of medium-light, laid, and lined white paper measuring $7\frac{3}{4} \times 9\frac{3}{4}$ in. The leaves are separated halves of formerly single sheets of stationery that have a "CONGRESS P & P" embossing in the top left corner. Peirce carefully inscribed and moderately altered this manuscript in blue ink, and he may have used it for a lecture entitled "The Relations of Logic to Philosophy," given before the Harvard Philosophy Club on 21 May 1879. An earlier opening paragraph survives on a separate half sheet.

Textual Notes

8.25 that] What might be a period appears here in the MS. It is possibly a stray mark, false start, or even alteration with the rest of the phrase added as an afterthought, Peirce having neglected to delete the period.

9.1 For . . . a] Initially, Peirce inscribed "Even a" above deleted "Both reality and" but then added, without lowercasing the "E," "For" before "Even a."

Emendations

7.4–5 *Popular . . . Monthly*] E; *rom.*
7.25 some things] E; somethings
8.2–3 *Proceedings . . . Sciences*] E; *rom.*
8.3 Nov.] E; $\sim_\wedge$
8.9 Non-sensitive] E; $\sim_\wedge\sim$
8.11 etc.] E; $\sim_\wedge$
8.23 heaven,] E; $\sim_\wedge$
8.24–25 *Kritik . . . Vernunft*] E; *rom.*
*8.25 that$_\wedge$] E; $\sim$.
*9.1 even] E; Even
9.7 know] E; no

4. *Comparing Wave-length with Metre, 1879*

Copy-text for this article is Peirce's 23 May 1879 letter [CSP], which is discussed in the Essay on Editorial Method. The letter, which Peirce left untitled and which is inscribed in black ink on "U. S. Geodetic Survey Service" stationery, is now in Record Group 23 in the National Archives; it is emended by the pencil editing of the Coast Survey Office [CSO] and by P 136, the publication in the *American Journal of Science and Arts* [136], from which the title is taken. All rejected substantive variants are from P 136.

Textual Notes

10.1 In the 23 May 1879 letter, the date and inside address are: "Cambridge 1879 May 23|C. P. Patterson Esq.|Superintendent U. S. Coast & Geodetic Survey|Washington D. C." In P 136, "ART. VI" precedes the title, and after it is: "by C. S. PEIRCE. Communicated by the Superintendent of the U. S. Coast and Geodetic Survey."

10.1–2 *Note . . . Metre*] The spelling of "Metre" conforms to Peirce's usage throughout the letter. Also, the title that Patterson appended to the letter uses Peirce's *-re* spelling. The published form is *"Meter."*

11.20 to] This is a restoration of Peirce's word "to" over which he inscribed "first" in an incomplete revision.

11.25 respectfully] The deletion of the word "very," attributed here to AJSA only, may have had its origins in the Coast Survey Office. Peirce's closing appears on the right-hand half of the page, directly beneath two final, pencil-deleted paragraphs. The pencil deletion lines run diagonally across the paragraphs, and one of the lines extends through "very." It is uncertain whether this was intentional.

11.26 Cambridge . . . 23] The date line, added in pencil at the left following Peirce's signature, is actually a repositioning of Peirce's own date line which is pencil-deleted and in the usual position on the first page of the letter, directly beneath the printed letterhead on the right.

Emendations

10.5 To] 136; *not present*
10.5 PATTERSON,] 136; Patterson$_\wedge$ Esq
10.5 and] 136; & *Also* 10.15, 24; 11.18, 21
10.6 Survey:—] 136; $\sim_{\wedge\wedge}$|Washington D. C.
10.9 No.] 136; $\sim_\wedge$
10.16 Sep.] 136; $\sim_\wedge$
10.16 Aug.] 136; $\sim_\wedge$
10.17 Mean——] E; $\sim_\wedge$ CSP • $\sim$, 136
10.17 89° 54′ 19″.5] 136; 89$_\wedge$ 54$_\wedge$ 19$_\wedge$·5
10.20 coëfficient] E; coefficient CSP, 136 *Also* 11.20
*11.20 to] 136; first
11.24 long.] CSO, 136; I go abroad. ¶I propose to take the glass metre with me to compare directly with one of the authentic metres. ¶The preliminary report on the whole work will be ready to print before I leave so that I need not even read the proof.
*11.25 Yours respectfully,] 136; yours very $\sim_\wedge$
11.25 C. S. PEIRCE, *Assistant.*] 136; C$_\wedge$ S$_\wedge$ Peirce$_\wedge$|Ass't$_\wedge$
*11.26 Cambridge . . . 23] CSO; *not present* 136

Rejected Substantive Variants

10.18 0″.4] 0·4″
11.5 50.35] 90° 03′ 50·35

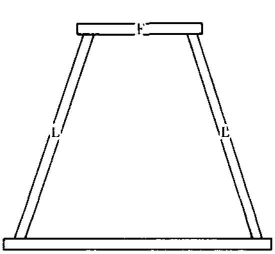
5. *Method of Swinging Pendulums, 1879*

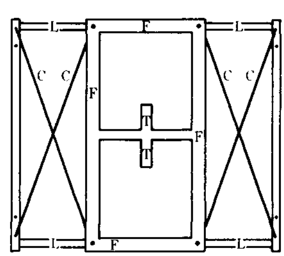
Copy-text for this report is Peirce's 17 February 1879 letter [CSP], written in black ink, which is discussed in detail in the Essay on Editorial Method. The letter-report, which Peirce left untitled, is emended by the Coast Survey Office pencil editing [CSO] and by P 137, the printing in the *American Journal of Science and Arts* [137], from which the title is taken. Peirce made alterations on all but the first and last pages of his letter-report. Unless otherwise noted, rejected substantive variants are from P 137.

Textual Notes

12.1–6 The first paragraph in the letter-report is preceded by: "U. S. Geodetic Service|Pittsburgh Pa, 1879 Feb 17.|C. P. Patterson, Esq.|Superintendent U. S. Coast and Geodetic Survey,|Sir:—." In P 137, "ART XVIII.—" precedes the title, after which appears: "by C. S. PEIRCE. [Read before the National Academy Academy of Sciences, April 17th, 1879, with authority of the Superintendent of the U. S. Coast and Geodetic Survey.]."

15.diagram The 17 February 1879 letter-report contains three roughly-drawn diagrams: the "wave" diagram, a top view of the suggested construction of a pendulum stand, and an accompanying side view of the latter, all of which are further discussed in the Essay on Editorial Method. Here are the last two as redrawn from Peirce's letter:





17.7 support-braces] Sometime between the redrawing of Peirce's original diagram and the later design for P 137, the projected construction for the legs of the stand was changed from cross-braces to support-braces. The text describing the stand was not changed because it referred to the original design, but the design change was not reflected in the text as printed. The text now describes the design accurately.

Emendations

12.6 Stuttgart,] CSO, 137; ~‸
12.6 1877,] E; *not present* CSP • 1878, CSO, 137
13.8 viz.,] 137; ~.‸
13.12 the supports] 137; the parts of the support
13.18 then] CSO, 137; in fact
13.23 ‸$\frac{d^2\varphi_1}{dt^2}$‸] 137; (~)
13.35 experiments] 137; experiment
14.9 φ_2] 137; φ_1
14.9 φ_1] 137; φ_2
14.11 constants)] 137; ~,
14.14 y] 137; *rom. Also* 15.24
14.15 $(t - t_2)\}$.] E; ~ } ‸ CSP, 137
14.19 starting.] 137; ~, and put $t_1 = \delta t$ and $t_2 = -\delta t$.
14.19–20 that instant] 137; the mean instant of starting,
15.1 And] 137; and
15.5 *[formula].*] E; ~‸ CSP, 137
15.19 figure,] 137; ~‸
15.19 time,] 137; ~‸
15.21 amplitude.] 137; ~‸
16.10(2) l] 137; *rom. Also* 16.11 (2, *num.*)
16.12 one-millionth] E; ~‸~ CSP, 137
16.14 one twenty-fifth] 137; $\frac{1}{25}$
16.15 the natural] 137; natural
16.16 four] 137; 4
16.18 π] 137; ∂
16.20 be only] 137; only be
*17.7 support-braces] E; cross-~ CSP • cross‸~ 137
18.1 pendulums] 137; two ~
18.1–9 Let . . . $\sin_2$,] 137; Then the phases of the harmonic constituents will be either exactly coincident or exactly opposed. The ratio of B to A is nearly as $\frac{\delta l}{y}$ to $2 - \frac{\delta l}{y}$. Assume, then, I as a quantity nearly unity such that

$$B:A = I\frac{\delta l}{y}:2 - \frac{\delta l}{y}.|$$

Then we may write|

$$\varphi_1 = C(2 - \frac{\delta l}{y}) \sin \left\{\sqrt{\frac{g}{l+x-y}} \cdot t\right\}$$

$$\pm CI\frac{\delta l}{y} \sin \left\{\sqrt{\frac{g}{l+x-y}} \cdot t\right\}|$$

$$\varphi_2 = -C(2 + \frac{\delta l}{y})\sin \left\{\sqrt{\frac{g}{l+x-y}} \cdot t\right\}$$

$$\pm CI\frac{\delta l}{y}\sin \left\{\sqrt{\frac{g}{l+x-y}} \cdot t\right\}$$

18.10 where the] 137; The
18.11 constituents . . . t] 137; components
18.12–17 Then . . . 0.586.] 137; *not present*
18.13 coëfficients] E; coefficients
18.17 0.586.] E; ~‸
18.18 ¶It] 137; ‸~ will be observed
18.18–19 of . . . constituents] 137; *not present*
18.28 the one] 137; one
19.4 ten-thousandths] 137; ~‸~
19.6 or] 137; an
19.10 $\frac{l+x+y}{g}$,] 137; ~‸
19.12 y.] 137; ~‸
19.34 suggestion of] 137; method proposed by
19.34 Faye,] 137; ~‸
19.34–20.2 though . . . hand,] 137; *not present*
20.2(1) as] 137; thoroughly
20.2 as it is brilliant,] 137; *not present*
20.3 offers] 137; that it ~
20.3–4 method . . . pendulums] 137; system
20.5 Feb. 17, 1879.] 137; yours very respectfully,|C S Peirce|Assistant

Rejected Substantive Variants

13.32 s_2] *s*
15.diagram [*diagram unnumbered*]] [*diagram numbered* "1." *and wrong diagram printed*]
17.diagram [*diagram unnumbered*]] [*diagram numbered* "2." *and wrong diagram printed*]

6. *Algebraic Principles of Logic, 1879*

Copy-text is MS 348, which consists of forty-one incomplete, discontinuous, and formerly folded sheets of slightly heavy and stiff, laid, lined white paper measuring $7\frac{3}{4} \times 9\frac{13}{16}$ in., with a "CONGRESS P & P" embossing in the upper left corner. The leaves are inscribed in faded black ink and are moderately altered. "By C. S. PEIRCE" follows the title. There are eight other leaves of related material, all of which are early attempts at "On the Algebra of Logic" (item 19).

Textual Notes

25.12 March 1867] Peirce misremembered when he wrote "May 1866." He is referring to the proof in his paper, "On an Improvement in Boole's Calculus of Logic," presented 12 March 1867 to the American Academy of Arts and Sciences and published in its *Proceedings* the following year (see *W*2:12–23).
26.25–26 These two lines, Peirce's apparent afterthought, were squeezed onto a single line between what precedes and follows.

Emendations

21.4 viz.] E; ~‸ *Also* 23.20
21.8 ¶The] E; ‸~
21.16 analogies] E; anologies
21.20 subjects.] E; ~‸
21.25 a proposition] E; proposition
21.28 expresses] E; expressed
24.6 *z*] E; *rom. Also* 24.8; 27.18(2); 28.5(2); 33.24; 34.21(5), 26(2)
24.8 $(x \mathbin{+\!\!,} y)$] E; *rom. Also* 24.12, 28.4
24.16 that] E; the
*25.12 March 1867] E; May 1866
25.15 +,] E; +‸ *Also* 27.18(1), 20(3), 21; 34.7(1), 10(3,4), 15(2), 23(1)
26.5(1) $\bar{x}$] E; *rom.*
26.10–15 $x \prec y$ . . . $n\text{-}x$] E; *rom. All lowercase variables rom.*
26.25 *Definition . . . Identity.*] E; *rom.*
26.29 *y*] E; *rom. Also* 34.10(7), 15(1), 21(4); 35.10(1), 26(3)
27.5 *Corollary*] E; *Corrollary*
27.6 *Corollary*] E; *Corrolary*
28.2 $x,y \mathbin{+\!\!,} x,z \prec x,$] E; *rom.*
28.4 $(x \mathbin{+\!\!,} z)$] E; *rom. Also* 35.14
29.3 $n\text{-}y \prec x,n\text{-}y$] E; *rom.*
29.3 $n\text{-}x$] E; *not present*
30.12 *x*] E; *rom. Also* 33.28, 34.15(1), 35.28(5), 37.5(6)
31.23 From] E; Fom
32.1 *Nothing.*] E; ~‸
32.19 5.] E; ~‸
32.23 Schröder] E; Schöder
33.14 $x,n\text{-}x$] E; *rom. Also* 37.3(1)
34.8(4) *z*] E; *x*
34.10 $y,‸x$] E; $y,,x$
34.15(2) *x*,] E; x‸
34.19 $n\text{-}y \mathbin{+\!\!,} z$] E; *rom. Also* 34.21
34.26 x,z] E; x,y
35.10(1) *n*] E; *rom. Also* 35.12, 13, 14(1), 28(4); 37.5(7)
35.24 *Theorem* V.] E; *Theorem* V. If $x \prec n\text{-}y$ then $y \prec n\text{-}x$ | *Proof.* | *Theorem* V‸
36.3 *Scholium.*] E; ~‸
36.21 VI.] E; *ital.*
36.28 *Theorem*] E; *rom.*
36.28 Schröder's] E; Schöder's

Line-End Hyphenation

29.14 everywhere
29.22 interchanged

7. *Of Thinking as Cerebration, 1879*

The copy-text, MS 350 [350], is an incomplete but reassembled italic typescript of ten numbered leaves, the first seven in purple and the last three in black ribbon. Pages 2–7 are on medium-weight, unlined, white paper measuring 8 × 10 in., pages 1 and 8–10 on medium-light, laid, unlined, white paper measuring $8\frac{3}{8} \times 10\frac{7}{8}$ in.; the last three are watermarked "THE AMERICAN LINEN PAPER." All of Peirce's alterations and his added handwritten paragraph before "The Settlement of Opinion" are in blue ink. There are pre-copy-text materials in MS 349, which consists of eight extensively altered, numbered leaves in italic typescript and four autograph leaves [349]. Although MS 349 lacks most of page two and "The Settlement of Opinion," it may have been used for preparation of a clean typescript of MS 350. Collation indicates that Peirce either typed MS 350 himself from MS 349, or he had it prepared by a typist from a cleaner copy that is not extant. Collation also shows that Peirce paid more attention to punctuation in MS 350 than in MS 349. "The Settlement of Opinion" was probably typed directly from P 107 in the *Popular Science Monthly* 12 (November 1877): 7–8 [107]. MS 349 and P 107 are used to correct typographical errors and to restore oversights in the manuscript. For example, two colons in MS 350 are probably the result of Peirce's not spacing back on the typewriter to form semicolons (by overtyping the colon with a comma), and missing necessary words are reinstated from pre-copy-text forms. The rejected substantive variants preceding "The Settlement of Opinion" (43.20) are from MS 349, and the rest are from P 107.

Textual Notes

39.18 ¶By] Only "be" appears on the last line of the second manuscript page, which leaves unclear whether Peirce meant a run-on or new paragraph at the top of page 3, where "By" is flush left. Nor is the equivalent extant in MS 349. The internal evidence of context and structure suggests the emended paragraph indention.

42.18 muscles,] In the revision process, a semicolon seems to have been typed inadvertently in the possible transcription of this extensively altered passage from MS 349. The context requires the emending comma.

Emendations

38.19 occasionally] E; occasionly 350, 349
39.6 accidentally] E; accidently 350, 349
39.9 *Habit and Intelligence*] E; *indistinguishable in italic typeface*
*39.18 ¶By] E; $_\wedge$~
39.22 other] 349; toher
39.33 of] 349; o
39.39 been] 349; be ~
40.23(1) it] 349; ~ ~
40.36 nerves;$_\wedge$] 349; ~:,
42.3 are] 349; *not present*
*42.18 muscles,] 349; ~;
42.28 *fainter*] 349; *indistinguishable in italic typeface*
42.29 product] E; pr ~
42.34 is] 349; *not present*
43.9 circumstances] 349; circ mstances
43.11 reaction] 349; action
43.28 not] 107; n t
43.31 not,"] 107; not,n"
43.34 led] 107; lead
43.35 doctrine;] 107; ~:
44.8 ingesta] 107; injest a
44.18 disappointment] 107; dissappointment
44.23 danger;] 107; ~:
44.29 impertinence] 107; *not present*

Rejected Substantive Variants

38.13 usefulness of them] part they play
38.15 glandular] *not present*
38.17 affected: . . . time] ~: but
38.26 intervals] inter-v
38.26–39.21 before . . . an] *not present*
39.34 that] which *Also* 39.37
40.4 the] *not present*
40.4 in order] *not present*
40.11 holds.] ~.* * I need not say that these essential principles in the formation of habits are aided by a number of secondary but still important causes.
40.13 perhaps] and
40.16 that] which
40.17 centre] ~ of
40.20 meet] ~ with
40.20 of a] of
40.22–23 soul . . . not] ~, as an independent substance, exist or no
40.23 that . . . it] ~, as we know it, intelligence
40.25 to] very closely with
40.29 a nerve-cell is] is a nerve-cell
40.30 without] ~ giving an particular
40.32 the genera,] ~ physiological ~‸
40.40(1) that] which
42.1 distinctions are] distinction is
42.3 there] ~ are
42.4 and] which are
42.5 nevertheless] still
42.5–6 reactions . . . itself.] ~, namely with a reaction upon a part of the brain.
42.13 that] ~ ~
42.15 as] *not present*
42.16 all] *not present*
42.16 action] ~ in every case
42.18 has, at last,] at last‸ has
42.19 say] speak
42.22 habit.] ~. We here have to take into account the singular phenomon of fancy. When sopething is imagined, the brain seems to be excited in very much the same way as if there had been a real perception similar to the that which is fancied. The energy of the excitation is generally of the excitation relatively feeble, but that does not seem to be essential: the main peculiarity of the case seems to be that muscular reaction is in some way inhibited. Now, in all speculative and prudential thinking, embracing all ela orate use of the brain reason, the doubt or first irritation of the brain comes from a fancy. the mind then proceeds to act just as if a real occasion had arisen, only with more deliberation. No muscular reaction t kes place, but a brain-habit or belief has been created from which we shall act without hesitation whenever a real occasion does arise. and then for the first time will the fact that a habit has been established be made manifest. Although in this speculati e thinking no muscular reaction takes place, yet in place of it a distinct judgment is formed, which seems to be wanting when we think for the pur ose of immediate action. This judgment is a sensation by which we are assured of the establishment of a belief-habit within us. The effect of this judgment is most important, [*four and one-half lines blank on rest of page*]
42.22–24 There . . . reaction.] *not present*
42.39 inhibitory] inhibiting
43.1 of] ~ the
43.10 which] *not present*
43.13–19 belief, . . . belief-habit.] belief. [*rest not present*]
43.20 THE . . . OPINION] V
43.21–22 Seeing . . . belief] If the settlement of opinion is the sole object of inquiry, and if belief is of the nature of a habit
43.24 that] which *Also* 43.25(1), 26
43.28 being once] once being
43.29 about] upon
43.33 on] upon
44.3 holds] ~ to
44.15 believes] ~ that

8. *Thinking as Cerebration, 1880*

The copy-text, MS 354, is an italic typescript of four numbered leaves (except for the second) of medium-weight, unlined, white paper measuring

8 × 10 in., prepared in purple ribbon. There are five alterations in black ink on the first two sheets only.

Textual Note

45.1–2 Because the typist (Peirce or someone else) neglected to space down before typing the second line, "Chapter I. Thinking as Cerebration" is typed over "LOGIC" on the verso of the leaf. When the typist realized the error, he restarted on what is now the recto, but made a new error in not capitalizing the last word of the title. The capitalized form has been adopted.

Emendations

45.11 The] E; the
45.15 cylinder] E; cylinders
45.18 of] E; o
45.22 to other vital] E; $\sim$ $\sim$ vit other vital
45.22 processes] E; porocesses
45.28 will] E; willl
46.3 efferent] E; eferent
46.22 species;$_{\wedge}$] E; $\sim$.:
46.24 have taken] E; have all the various actions which $\sim$ $\sim$
46.33 by inheritance] E; byHinheritance

9. *Rood's Chromatics, 1879*

Copy-text is P 149, the publication in the *Nation.* Haskell's *Index to the Nation* identifies Peirce as the author of this unsigned review. There are two additional paragraphs on questions of art, which were written by Russell Sturgis, an art specialist; they are not published here.

Emendations

47n.1 $_{\wedge}$*Modern . . . Industry*] E; 'Modern . . . Industry
47n.2 College.$_{\wedge}$] E; $\sim$.'
48.14 color-sense] E; $\sim_{\wedge}\sim$

10. *Ghosts in Diffraction-Spectra, 1879*

Copy-text is P 134, the publication in the *American Journal of Mathematics.* It is emended by MS 343, a single page corresponding to 62.27–31 and the table on p. 63. Sometime before 3 December 1879, Peirce wrote his father that he was mailing him his "paper on Ghosts which I desire to have printed at once," and he requested that he send it to the National Academy of Sciences publishing committee, "& see if they will print it at once. If not, please send it back to me & I will send it to the C. S. Office or to [Superintendent] Patterson. Others are working at the same matter & I wish to retain my priority." In the AJM publication, "BY C. S. PEIRCE. [Published by the authority of the Superintendent of the United States Coast and Geodetic Survey.]" follows the title.

Textual Notes

62.31 19.] The table that originally followed here has been moved, for typesetting reasons and as indicated in the italic brackets, to the next page.
62.46 C.] The value for the C line is omitted in the original article. It was probably some value other than $10\overset{r}{.}010$ because the third order Ghost, 0, occupies that position, and the dispersion calculated from that value ($2\overset{r}{.}956$) is higher than expected.
63.23 $1\overset{r}{.}044$] According to the data given, the mean for Ni – D_2 should be 1.042.

The text is not emended, however, because Peirce uses $1\overset{r}{.}044$ in a later calculation at 65.26.

64.8 $1\overset{r}{.}190$] The correct figure should be $1\overset{r}{.}199$. However, as it seems impossible to ascertain whether the figure $1\overset{r}{.}190$ or one of the other two from which it is derived is in error, the originally printed figure is allowed to stand.

66.19 If, as is probable, the observed value for G − 1,D_2 is $7\overset{r}{.}378$ (as it appears at 61.20), the difference between the observed and calculated values should be −.010. But in the absence of absolute certainty, the originally printed numbers are allowed to stand.

Emendations

50.5 diffraction-plate] E; $\sim_{\wedge}\sim$

52.17 $\frac{\alpha^M}{2^{M-1}}\cdot$] E; $\sim_{\wedge}$ *Also* 52.19

53.4 etc.] E; $\sim_{\wedge}$

53.10 $-\frac{1}{9216}$] E; $_{\wedge}\sim$

54.15 $\frac{W}{\omega}\cdot$] E; $\sim_{\wedge}$

54.16 even] E; *ital.*

54.19 odd] E; *ital.*

56.17 $\frac{\epsilon^m\omega^{m-1}}{m!2^{m-1}}\cdot$] E; $\sim_{\wedge}$

56.19 coëfficients] E; coefficients

58.16 coëfficient] E; coefficient *Also* 58.19, 26; 59.3, 8, 11, 13, 16

59.4 *[formula]*.] E; $\sim_{\wedge}$

59.17 $\frac{1}{4}\cdot$] E; $\sim_{\wedge}$ *Also* 59.19

60.24 millimetre] E; millimeter *Also* 60.32; 62.2, 28; 64.13, 14

60.28(1) D_1] E; D

61.6–8 $\left\{\begin{array}{l}D_2\\ D_1\\ \text{Mean}\end{array}\right.$] E; $\left\{\begin{array}{l}D_2\\ D_1\end{array}\right.$ Mean *Also* 61.14–16, 22–24, 30–32, 38–40, 46–48; 62.8–10, 16–18, 24–26

62.6 $12\overset{r}{.}496$] E; $13^{r}.496$

62.14 $6\overset{r}{.}482$] E; $2^{r}.482$

62.28 millimetre] 343; millimeter

63.6–8 $\left\{\begin{array}{l}D_2\\ D_1\\ \text{Mean}\end{array}\right.$] 343; $\left\{\begin{array}{l}D_2\\ D_1\end{array}\right.$ Mean

63.15–18 $\left\{\begin{array}{l}D_2\\ Ni\\ D_1\\ \text{Mean}\end{array}\right.$] 343; $\left\}\begin{array}{l}D_2\\ Ni\\ D_1\end{array}\right.$ Mean *Also* 63.25–28

63.35–37 $\left\{\begin{array}{l}D_2\\ D_1\\ \text{Mean}\end{array}\right.$] 343; $\left\}\begin{array}{l}D_2\\ D_1\end{array}\right.$ Mean

64.3 1.] E; $\sim$,

67.10 I] E; 1

Rejected Substantive Variants

63.1 of] *not present Also* 63.2, 9, 10, 11, 19, 20, 21, 29, 30, 31

63.12 $6\overset{r}{.}531$] 6.531

63.22 $10\overset{r}{.}693$] 10.693

63.30 Means.] *not present*

11. *Quincuncial Projection, 1879*

Copy-text is P 135, the publication in the *American Journal of Mathematics* [135], which is emended by the "Errata" [er] that appeared on the fifth page of the next volume of AJM; by P 183, the 1877 *Coast Survey Report* (Washington: Government Printing Office, 1880), pp. 191–92, and the map plate [183]; and by P 238, Thomas Craig's *A Treatise on Projections* (Washington: Government Printing Office, 1882), pp. 132 and 247 [238]. All rejected substantive variants are from P 238.

Peirce began thinking about map projections as early as 1876 in connection with a new edition of Ptolemy's catalogue he was preparing. He wanted to print a planisphere showing the stars and the figures of the ancient constellations. Early in 1877, he worked at developing the quincuncial projection and apparently submitted a now lost "paper" to Superintendent Patterson sometime before 23 February. He discussed the "revising" of this paper with various correspondents during the next two years, and on 4 February 1878 he first spoke of sending it to J. J. Sylvester, editor in chief of the *American Journal of Mathematics.* When Peirce informed Patterson on 26 April that Sylvester had accepted his paper for the forthcoming issue, Patterson agreed to the AJM publication, but later decided that it should also be published in the *Coast Survey Report.*

A copy of the projection was printed for Peirce's use at the General Conference of the International Geodetical Association at Stuttgart in the fall of 1877. But he continued to work on his paper throughout 1879–1880 to make, as he said in a 10 January 1880 letter to Patterson, "additions . . . to it & cut it down so as to give it a very special character so as to *unify* it, & make it say the particular thing I have particularly to say with clearness & force & say nothing else. . . . I have worked a good deal on this, . . . but writing these very short things is often more labor than would be supposed." By 26 March Peirce had already received an offprint of his paper and had noted his correction of the reversed words, which arrived in time for inclusion in the Errata for 1880. But when he sailed for Europe on 28 April, the hundred requested copies of his paper had not arrived. The Coast Survey Office later sent the remaining twenty-four copies for distribution in Europe.

Textual Note

68.1 In P 238, the title lacks the indefinite article; in P 183, it is preceded by "APPENDIX No 15," and "BY C. S. PEIRCE, ASSISTANT" follows it; in P 135, "BY C. S. PEIRCE.|[Published by the authority of the Superintendent of the U. S. Coast and Geodetic Survey.]" follows it.

Emendations

68.17 projection] er, 183, 238; potential

68.18 potential] er, 183, 238; projection

68.20 *Electricity and Magnetism*] E; *rom.*

69n.8 *per cent.*] E; ~ ~‸

71.14(2) line).] 183, 238; ~.)

Rejected Substantive Variants

68.4 ¶For meteorological] ¶This projection was constructed by Mr. C. S. Peirce, Assistant, United States Coast and Geodetic Survey. The brief description here given of the projection is extracted from the Coast Survey Report for 1877, Appendix No. 15, and was written by Mr. Peirce himself. ¶For meteorological

68.6–7 This . . . plate.] *not present*

69 [*map*]] *not present*

71.1–2 I. *Table of*] TABLE XXX.

71.25 II.] TABLE XXXI.

71.29 15°] 15 *Also* 71.31(2)

12. *Economy of Research, 1879*

Copy-text is MS 307, now in Record Group 23 in the National Archives. It is discussed in the Essay on Editorial Method. Its ten leaves of unlined, heavy, white paper that was formerly connected in single, large sheets measure $8\frac{1}{8} \times 10\frac{5}{8}$ in., and are inscribed in black ink. They are numbered in the upper right corner in bluish-green pencil, have pin holes in the top left corner where they were formerly fastened together, and are grayed and a bit fraying at the edges. *"By Ass't. C. S. Peirce"* appears after the title. There are relatively extensive alterations in this otherwise carefully inscribed manuscript. Non-editorial emendations and rejected substantive variants are taken from P 160, the publication in the 1876 *Coast Survey Report* [160].

Emendations

72.7 determinations] 160; determination
72.9 "probable error"] 160; $_\wedge\sim$ $\sim_\wedge$
72.12 diminishing] 160; diminutions of
72.19–20 The . . . symbols.] 160; *not present*
72.29 The . . . researches.] 160; *not present*
72.30 their] 160; the
72.30 properties] 160; ~ of the functions U and V
73.5 really] 160; *not present*
73.15 *r*] 160; *rom. Also* 76.11(5, 6)
73.18 non-quantitative] 160; $\sim_\wedge\sim$
73.21 observations, the] 160; $\sim_\wedge$ and the
73.21 being] 160; are
73.24 different] 160; *not present*
73.33 knowledge,] 160; ~;
73.34 improvement then] 160; that improvement
74.1 In our day] 160; Now
74.12 urgency,] 160; $\sim_\wedge$
74.12 or . . . cost] 160; *not present*
74.19 urgency-fraction] 160; fraction
74.21 exclusion] 160; eclusion
74.26–75.24 In . . . both.] 160; *not present*
75.20 $\frac{1}{4}\cdot$] E; $\sim_\wedge$ 160
75.27 to] 160; of
76.5 ρ_1^2] 160; ρ_1
76.5 ρ_2^2] 160; ρ_2
76.7 *[formula]*,] 160; $\sim_\wedge$
76.13(3) n_1] 160; *n*
76.16 *[formula]*.] 160; $\sim_\wedge$
77.4 knowledge] 160; determination
77.6 r_2^2,] 160; $\sim_\wedge$
77.8 *[formula]*.] 160;

$$\frac{\frac{dy}{dx_1}r_1}{\sqrt{\frac{dy}{dx_1}r_1^2+\frac{dy}{dx_2}r_2^2}}\,{}_\wedge$$

77.11 *[formula]*.] 160; $\sim_\wedge$
77.13 *[formula]*,] 160; $\sim_\wedge$
77.16–78.27 ¶The . . . investigation.] 160; ¶I propose now to make a practical application of these formulae to a problem connected with the determination of the force of gravity; which, in fact, suggested the whole speculation. ¶The error in the observed duration of an oscillation of a pendulum is due to three causes, 1[st] the irregularity in the motion of the pendulum, 2[nd] the error of the comparison with the clock, 3[rd] the error in the rate of the clock. These three sources of error are distinguished by their effects. An irregularity of the motion of the pendulum, if of rare occurrence, will produce an effect on the time of one oscillation which is independent of the duration of the experiment; but if it be continually occurring will produce an effect inversely as the square root of the duration of the experiment. If the irregularity be due to the mechanical effect of currents of air, it will be greater for small amplitudes; if it be due to jars, it will be the same for all amplitudes; and if it be due to changes of temperature it will be greater the faster the pendulum is moving through the air. All such irregularities will in the case of a reversible pendulum be inversely as the distance of the centre of mass from the point of support. An erroneous comparison of the pendulum with the clock will produce an effect on the time of one oscillation, which is inversely proportional to the duration of the experiment. An error in the rate of the clock will produce an effect which is independent of the duration of the experiment. ¶We find as a matter of fact that the errors are such as judged by these tests, should be produced by jars, with some effect from changes of temperature. The following table exhibits the probable error of the time of an oscillation under different circumstances,

each derived from eight experiments at Geneva, and compared with a calculation on the supposition that the error is wholly caused by jars. The units are 0.00000001 seconds.

						r Obs.	r Calc
Heavy end down							
Amplitude at beginning	2°;	at end	$1\frac{1}{2}$°.			320	312
"	"	"	$1\frac{1}{2}$°	" "	1°.	294	244
"	"	"	1°	" "	$\frac{1}{2}$°.	150	170
Heavy end up.							
Amplitude at beginning	2°;	at end	$1\frac{1}{2}$°			728	714
"	"	"	$1\frac{1}{2}$°;	" "	1°	552	558
"	"	"	1°;	" "	$\frac{1}{2}$°	366	388

¶This being the case, how many experiments should be made with the heavy end up as compared with the heavy end down. The cost is here the time occupied, and by the theory of the reversible pendulum

$$\frac{k_2}{k_1} = \frac{\frac{dy}{dx_2}}{\frac{dy}{dx_1}}. \text{ Hence } \frac{n_1}{n_2} = \frac{\rho_1}{\rho_2} = \frac{3}{7} \text{ That is, the heavy end up}$$

should have twice as many experiments or even more.

77.23 centre] E; center, 160 *Also* 78.19
77.29 (d T_d^2)] E; (d $T_d)^2$ 160
78.8 coëfficient] E; coefficient 160
78.12 *[formula].*] E; $\sim_{\wedge}$ 160
78.13 When] E; when 160

Line-End Hyphenation

73.38 tea-tray

Rejected Substantive Variants

74.6–7 have . . . life] sleep in death
74.13 $\frac{h}{k}\cdot\frac{r^4}{\sqrt{a+r^2}}$] $\sim_{\wedge}\sim$
76.16 1 *[num.]*] *not present*
76.21 n_2] n

13. *Gravity at Initial Stations, 1879*

Copy-text is P 161, the publication in the 1876 *Coast Survey Report.* It is emended by MS 384, Peirce's annotated offprint [384]; by P 290, in the 1883 *Coast Survey Report* (Washington: Government Printing Office, 1884), p. 476n. [290]; and by P 168, in the *American Journal of Science,* 3rd ser. 20 (October 1880): 327 [168].

On 13 January 1876 Peirce proposed to Superintendent Patterson "a Treatise on the Determination of the Force of Gravity" and sent him "a rough sketch of the plan for . . . [his] criticism," which sketch has not been located. By 8 October 1878 Peirce had sent his paper to the Coast Survey for inclusion in the 1876 *Report,* but he lamented in his letter to Patterson the next day that it

> was not all I could wish it to be and I shall have to send a few supplementary pages later. . . . I found . . . that none of the time observations made during my absence in Europe had been read from the fillets & others had not been worked up. . . . I then worked night and day & even after putting the matter into the hands of the copyist

succeeded in doing a great deal. The result was that I was yesterday so utterly exhausted by continuous work that it would have been useless to retain the paper a day or two longer in my hands. I, therefore, thought it best to forward it, in its somewhat crude state. . . . Meantime so much of the calculations of the last 3 weeks as have not already been done in duplicate will be done over so as to correct errors in the proof.

Peirce's paper was returned to him for further revision. By 24 March 1879 he had returned the paper to Patterson, but said:

I have . . . no desire to revise the matter further, except that it might be that a phrase might occur here and there which might be modified to avoid offending in any degree the French.

It appears to me essential that I should read the proof for the following reason.

It is known to be far more difficult to make a manuscript of mathematical work accurate than a printed proof. I believe that absolute accuracy in the MS. could not be attained without making three successive copies of it, occupying a good many months. Even when that was done the whole experience of mathematicians is that the proof would require the corrections of the author. It would therefore be far better to send the present MS., which was prepared very carefully, to the printer and revise the proofs by comparison with the original records and computations.

Since it will . . . be necessary for me to read the proof . . . to avoid the disgraceful character of a good deal of the Coast Survey printing, and since it is the universal custom throughout the civilized world, it appears to me better to make the very few and slight changes of phrase which may be advisable . . . in the proof . . .

I should think it far better not to print the report at all than to print it incorrectly.

Peirce received his requested proofs, and on 26 April he wrote to J. E. Hilgard that "On returning yesterday to Cambridge I found the enclosed proofs awaiting me. I suppose the printer will grumble about the number of changes. Please say to him therefore that these will not occur (to any extent) in the body of the paper, although there will be two or three long insertions to make." He continued to receive and revise proofs through July 1879, but on 11 August he wrote to Hilgard that "No proof remains now in my hands. Have decided not to make the long insertion I intended to make as it would further delay the press." When by 26 August he had received even further proofs of material he had submitted, he responded to Hilgard: "I regret that I cannot altogether approve of the illustrations of the pendulum work. I send corrected proofs of two of the most accurate. On these my mark *'out'* signifies that the dots referred to are *out of place*. . . . ¶I think that the divisions of the large squares into small ones should be shown. Without that, the illustrations are not sufficient." Peirce verified all the numbers in the proofs between late August and early September, and on 5 September wrote to Hilgard: "I return to you by today's mail the original drawings to accompany my paper on the comparison of gravity at European and American stations." Sometime after 15 April 1880 Peirce left for Europe and carried the first part of his pendulum report with him. He received the rest of his paper after 26 April.

As late as 12 April 1882 Peirce wrote to then Superintendent Hilgard that "On page 114 . . . it is stated that 39 centimetres on the staff of the balance used for determining the centre of mass of the pendulum are *too long* by 0.14 mm. But the correction is applied as if *too short* were meant." Although the matter was checked by Edward B. Lefavour, Peirce wrote to Hilgard on 1 December 1882: "A considerable part of my paper on the measurements of gravity at initial stations is rendered almost unintelligible by changes made after the last proof seen by me. These changes appear to have been made by the compositors running short of type and picking them out of the matter set up. But however made, I am desirous of avoiding a like accident in the papers now printing which contain a great many algebraical formulae, difficult to print."

Textual Notes

87 Because of photo-reproduction of the original drawings, the spelling of *"centimeters"* in drawing No. 26 has been allowed to stand.
122.19 table.] The table that originally followed here has been moved, for typesetting reasons and as indicated in the italic brackets, to the next page.
133.9 The occurrences of "Meter" in "German Normal Meter" are not emended because the phrase is taken to be a proper name.

Emendations

79.31 indispensible] E; indispensable
80.32 *et seq.* coëfficient] E; coefficient *All occurrences except* 102.21
81.14 *et seq.* metres] E; meters
81.37 *et seq.* metre] E; meter *All occurrences, including the compounds* pendulum-metre, metre-scale, *and* metre-scales
82.14 *et seq.* centre] E; center
82.14 cm‸] E; ~. *Also* 82.15, 17, 19, 20; 90.30; 111.18; 120.13; 121.24; 135.17
82.31 coordonnées] E; coordonées
83.18 General] E; Genera
83.29 stability.] E; ~‸
84.11 grammes] E; grams
84.12 *et seq.* Centimetres] E; Centimeter
86n.2 millimetre] E; millimeter *Also* 90.31, 134.28
89.22 sea-level] E; ~‸~
91.1 $\frac{1}{4}$'] 384; ~‸
91.2 Half-amplitude] E; ~‸~ *Also* 92.32
94.26 log‸ sin‸] E; ~. ~.
94.27 $(t-t_0)$.] E; (~)‸
95.35 $\mathrm{D}_t\varphi$] E; D_{t_φ}
96.3 Nat‸ log‸] E; ~. ~. *Also* 96.4, 5
96.3 C,] E; ~.
96.14 $\frac{p}{\tau}$.] 384; $\frac{\sqrt{\mathrm{p}}}{\tau}$‸
97.3 bell-glasses] E; ~‸~
98.27 2^{nd}] E; 2d *Also* 102.20
98.28 3^{rd}] E; 3d
99.29 $\frac{1}{g}$.] E; ~‸
100.28 *[formula].*] E; ~‸
101.18 *[formula].*] E; ~‸
101.20 *[formula].*] E; ~‸
102.5 *et seq.* coëfficients] E; coefficients
104.12 D_t] E; D_{t}
106.24 *et seq.* C‸] E; ~. *All occurrences representing "Centigrade" except* 122.18, 124.39
106.31 centimetre] E; centimeter *Also* 134.40
107.6 *et seq.* centimetres] E; centimeters
107.12 *Experimentalphysik*] E; *rom.*
107.14 60^{m}] E; 6^{m}
107.21 *et seq.* Centimetres] E; Centimeters
108.19 39.392] E; 39.292
109.2 $\frac{.001206}{6.35}=$] E; ~‸
109.17 .000580] E; .000380
109.18 $\mathrm{T}_u^2)=$] E; $\mathrm{T}_{\mathrm{u}})^2$‸
109.21 *[formula].*] E; ~‸
111.21 bob‸] E; ~.
111.22 $(\mathrm{cm})^2$] E; (~)
112.6 *et seq.* gr‸] E; ~.
117.27 COËFFICIENT] E; COEFFICIENT
120.5 a] E; *not present*
120.13 m‸] E; ~. *Also* 120.19, 132.25
120.27 mm‸] E; ~. *Also* 121.7
121.20 cm] E; (~)
121.24 $(\mathrm{s})^2$] E; ‸~‸^{2}
123.1 METRES] E; METERS
123.7 t_{A}] E; $t\mathrm{A}$
123.9 7.57] E; 7‸57
122.23 $24\overset{\mu}{.}4$] E; 24 .4
124.4 Pendulum-Metre] E; ~-Meter
124.22 $18\overset{\mu}{.}16$] E; 18 .16
124.24 $0\overset{\mu}{.}45$] E; 0 .45
124.31 T_d] E; T*d*
124.31 T_u] E; T*u*
125.3 $18\overset{\mu}{.}3$] E; 18 .3 *Also* 125.4
125.21 *[formula].*] E; ~‸
126.22 millimetres] E; millimeters
127.28 centres] E; centers

128.17 0.000128.] E; ~‸
131.35 seconds‸] E; ~' *Also* 132.23; 141.21, 24; 143.10, 12, 13, 15; 144.22
133.8 Pendulum-Metre] E; ~‸Meter *Also* 133.9
133.15 pendulum-metre] E; ~‸meter *Also* 133.22, 25–26; 135.22–23, 33, 36
133.29 pendulum-metres] E; ~‸meters
134.38 thermometer] E; thermometor
134.39 one-hundredth] E; ~‸~
137.7 CENTRE] E; CENTER
137.8 $h_d - h_u$] E; $\mathrm{h_d - h_u}$ *Also* 137.15
137.17 39.312] 290; 39.284
137.18 39.320] 290; 39.292
138.8 *C*] E; ~.
138.15 *off*] 384; *on*
140.3 *[formula]*.] E; ~‸
143.11 Inv.] 384, 168; Rev.
143.25 Seconds‸] E; ~' *Also* 143.32, 39
144.24 *seconds*‸] E; ~'

Line-End Hyphenation

84.7 knife-edges
86.3 spirit-level
88.8 equilibrium-point
91.27 knife-edges
109.4 knife-edges
120.31 thumb-screws
129.13 high-power
132.34 pendulum-support
134.28 one-tenth
138.20–21 interchanged

14. *Repetitions of Similar Trials, 1880*

The copy-text is MS 355, now in the Princeton University Library. It consists of five medium-weight, lined leaves of white paper measuring $7\frac{3}{4} \times 9\frac{7}{8}$ in., with a "Cumberland Mills" stamp in the upper left-hand corners. The leaves appear to have been torn from an exercise book, for they have slightly uneven left margins. The four leaves published here, and an additional leaf of notations and computations, are inscribed in blue-black ink and moderately altered. Published with permission of Princeton University Library.

Emendations

145.5 *et seq.* *p*] E; *rom. All lowercase variables rom.*
145.8 difference] E; Difference
145.11 is] E; ~ is

15. *Gravity at Paris, 1880*

Copy-text is P 256, the publication in the 1881 *Coast Survey Report.* On 14 June 1880 Peirce had "read . . . a paper before the French Academy on the value of gravity. It was very well received and the discussion of it was resumed" on 21 June, the date of his letter to Superintendent Patterson, in which he further said that "I had the satisfaction of receiving a vote of thanks for the research. The error which I have discovered in the old value excites so much interest that a new determination will at once be undertaken by the French. These gentlemen are anxious that I should also make a new determination with new apparatus, owing to the faults of that I previously used." Peirce sent his paper for publication in the 1881 *Report* on 19 July 1882, and he corrected proofs later in the year. "By C. S. PEIRCE, ASSISTANT" appears below the title in P 256 which, together with Peirce's three other Appendixes in the 1881 *Report* (P 253, 254, 255), was extracted, bound, and published—probably at the time of the printing of the *Report*—as *Methods and Results of Pendulum Experiments* (Washington: Government Printing Office, 1882). The original French version, "Sur la valeur de la pesanteur à Paris" (P 171), appeared in the *Comptes Rendus des Séances de l'Académie des Sciences* 90 (June 1880): 1401–3.

Emendations

148.20 metres] E; meters
149.17 2^{nd}] E; 2d
150.32–33 "Measurement . . . Stations,"] E; ‸~ . . . ~,‸
150.36 *et seq.* metre] E; meter
150.37 *American . . . Science*] E; *rom.*
151.8 seconds‸] E; ~'

16. *Science in America, 1880*

The copy-text is MS 363, which consists of thirteen leaves measuring $8\frac{3}{16} \times 10\frac{5}{8}$ in. of medium-light, unlined, white paper watermarked "Lacroix Frères." Their rough left edge suggests that they were torn from a pad. These notes for a July Fourth address are inscribed in black ink and extensively altered. The pages are numbered in the upper right corner in pencil in a hand probably not Peirce's.

Emendations

152.6 is usual] E; usual
152.12 fail to] E; ~ ~ fail to
152.19 had] E; has
152.25 two] E; too
152.26 men:] E; ~‸
152.26 Thompson] E; Thomson
153.4 magnificent] E; manificent
154.13 pedagogical] E; pedagoguical
154.21 science,] E; ~‸
154.21 research,] E; ~‸
154.23 a] E; I
154.27 simple.] E; ~‸
154.34 its] E; it
154.34 partisanship,] E; ~‸
155.3 and] E; *reinstate*
155.5 Saturn's rings] E; Saturns ring
155.6 Sun's] E; Suns
155.10 Astronomers] E; Astronomer's
155.13 governed] E; govererned
155.31 whatever] E; Whatever
155.33 Surveys,] E; ~‸
156.1 must,] E; ~‸

Line-End Hyphenation

152.8 shortcomings

17. *Peirce to Faye, 1880*

Copy-text is P 215, the publication in *Verhandlungen der Europaeischen Gradmessung.* The emendation at 159.15 is from the additional printing in the German section of the *Verhandlungen,* pp. 30–32 [G].

Emendations

157.16 ça] E; ce
158.19 chacun] E; chaqu'un
158.27 résumé] E; resumé
159.7 dont] E; que
159.15 l'enregistrement] G; l'enrégistrement
159.24 périodes] E; periodes
159.35 établi] E; etabli

18. *Colours of Double Stars, 1880*

Copy-text is P 179, the printing in *Nature,* signed "C. S. PEIRCE" at the end.

Emendations

161.8–9 ‸*Modern Chromatics.*‸] E; "Modern Chromatics."

162.6–7 double‸stars] E; ~-~ *Also* 162.16

Line-End Hyphenation

162.2 differently-coloured

19. *Algebra of Logic, 1880*

Copy-text is P 167, the publication in the *American Journal of Mathematics,* which is discussed in the Essay on Editorial Method. It is emended by MS 366, Peirce's "Corrections not final" offprint [366], inscribed in black ink; by MS 367, his "Working Copy" offprint [367], inscribed in black ink; by MS 369, his "Lecture Copy" offprint [369], also inscribed in black ink, and by MS 368, his printed letter of corrections [368]. In P 167, "BY C. S. PEIRCE" is below the title.

Textual Notes

178n.1–6 The wording of this footnote is from the MS 368 letter, but its placement as a footnote is given only in MS 367, where the note reads: "An oversight has here been committed. For from $A = (\bar{A} \prec x)$ follows not merely (16) but also (19), (20), and (21), and therefore all the properties of the negative which concern syllogistic. But this does not affect the general view here taken of the subject for all the forms here stated to be deducible from $\bar{A} = (A \prec x)$ are in fact deducible from (16)."

180.21 In MS 367 Peirce has added the additional formula in the bottom margin with an arrow showing where it is to be inserted. In MS 366 he has done the same but with this additional notation: "Here, I should have noticed (3′) . . . $\overline{\prec} y$ } Formula (3) has the same relation to (14) of chapter II that (3′) has to (34)."

183n.1 The editors are responsible for the placement of this notation as a footnote. The wording is taken from MS 367 (which corresponds to MS 368 but without directive), where Peirce has written the notation in the left margin beside lines 6–9. MS 366 contains a similar notation: "After each of the lettered propositions I give its proof."

197.2–3 The MS 368 letter contains the same directions as apply to the deletions of six lines in MSS 366 and 367, but with the additional comment, "the reasoning being inadmissible."

201.36 hyphen (–)] In his annotated copies Peirce did not remark on the discrepancy between the word "hyphen" and the en dash that followed in parentheses. The printer's setting of the dash instead of the hyphen is allowed to stand.

Emendations

166.10 *et seq.* P_i] E; *subscript rom. All subscript i's rom.*

169.6 2nd] E; 2d *Also* 205.14

169n.1–2 "On . . . Arguments,"] E; ‸*On . . . Arguments,*‸

169n.4 "Logic of Relatives,"] E; ‸*Logic of Relatives,*‸ *Also* 182n.2, 195n.1

171n.2–3 "because . . . like."] E; ‸~ . . . ~.‸

172.13 "On the Syllogism,"] E; ‸~ ~ ~,‸

172.13 I‸] E; ~.

173n.1 "On . . . No. V‸," 1863] E; ‸*On the Syllogism,* ~. V.,‸ 1862

173n.2 "Calculus . . . Statements] E; ‸*Calculus . . . Statements*

173n.2 [Second Paper],"] E; ‸~ ~‸,‸

174n.1 "On . . . No. II‸,"] E; ‸*On the Syllogism,* ~. ~.,‸ *Also* 174n.11–12

175.3 P),] E; ~)‸

175.4 *x;*] E; ~‸ *Also* 175.9

175.8 P,] E; ~‸

176.8 P] 367, 368; $\overline{\mathrm{M}}$
176.30 *Bokardo*] E; *Bocardo*
*178n.1–6 An . . . 16.] 367, 368; *not present*
180.15–16 ¶The inference|(2′)] E; ¶(2′) The inference
*180.21 (3′) . . . $\overline{\prec}\, y\}$.] 366, 367, 368; *not present*
180.26 $x\overline{\prec}$] E; ∼ $\prec$
182n.7–8 "On . . . No. III$_\wedge$,"] E; $_\wedge$*On the Syllogism,* ∼. ∼.,$_\wedge$
182n.10 *Pure*] E; *Formal*
182n.14–15 "On . . . Logic,"] E; $_\wedge$*On . . . Logic,*$_\wedge$
182n.18 "Ausdehnungslehre,"] E; $_\wedge$*Ausdehnungslehre,*$_\wedge$
182n.19 $_\wedge$*Die . . . Mathematik,*$_\wedge$] E; '∼ . . . ∼,'
182n.34 Bain's] E; *ital.*
182n.34 "Calculus . . . Statements,"] E; $_\wedge$*Calculus . . . Statements,*$_\wedge$
*183n.1 The . . . enunciations.] 366, 367, 368; *not present*
183n.2 "Logic of Relatives" (§4)$_\wedge$] E; $_\wedge$*Logic of Relatives*$_\wedge$ (§4);
184.6 (6),] E; (∼)$_\wedge$
185.2 *b*).] E; ∼)$_\wedge$
185.10 + *b*)$_\wedge$] E; + ∼),
185.10 *[formula]*$_\wedge$] E; ∼.
185.11 *c*)$_\wedge$] E; ∼),
185.16 = *q*$_\wedge$] E; = ∼, *Also* 185.17
185.19 $\prec b)_\wedge$] E; $\prec$ ∼),
185.20 *q*)$_\wedge$] E; ∼),
187.3 ∞] 366, 367, 368; 1 *Also* 194.23, 26
187.3 *x*,] E; ∼.
188.1 $\overline{a \times b}$] E; $\overline{\sim + \sim}$
188.10(2) =] 366, 367, 368; $\prec$
189.30 *c*),] E; ∼)$_\wedge$
191.19 *z*] E; $\bar{z}$
191.19 ×] 367, 368; + *Also* 191.20, 21
191.19 $\bar{w}$] E; *w*
191.20 $\bar{z}$] E; *z*
191.20 *w*] E; $\bar{w}$
192.1 *w*$_\wedge$] E; ∼,
193.28–29 "Appendix . . . Dialectic,"] E; $_\wedge$*Appendix . . . Dialectic,*$_\wedge$
194.6 Africa,] E; ∼$_\wedge$
195.9 from———] E; ∼$_\wedge$
*197.2–3 + etc.|§3.] 366, 367, 368; + etc.|Here we have evidently|(A:C):B = A:(B:C).|In the same way we find|(A:D):(B:C) = (A:C):(B:D)| = A:[B:D):C] = A:[B:(C:D)]| = [A:(C:D)]:B = [(A:D):C]:B.|§3.
197.22 *[formula]*,] 366, 367, 368; ∼$_\wedge$ = Σ[A:(C:B)] *Also* 198.3
197.24 *[formula]*] 366, 367, 368; ∼ = Σ[B:(C:A)]
197.25 J*b* =] 367, 368; ∼ = Σ[(A:(B:C)] =
197.26 K*b* =] 367, 368; ∼ = Σ[C:(A:B)] =
197.27 *[formula]*] 366, 367, 368; ∼ = Σ[C:(B:A)]
197.28 M*b* =] 367, 368; ∼ = Σ[B:(A:C)] =
201.10 zero] E; first
204.20 ^{l}*s*] E; *ls*
204.21 servant of———] E; servant
204n.1–2 "On the Syllogism, No. IV$_\wedge$"] E; $_\wedge$*On the Syllogism,* ∼. ∼.$_\wedge$
204n.2 *external*] 366, 367, 368; *internal*
204n.3 "Logic of Relatives."] E; $_\wedge$*Logic of Relatives.*$_\wedge$
205.1 indispensible] E; indispensable *Also* 205.22
205.17 3rd] E; 3d
207.8 × $\overline{ac}$] 366, 367, 368, 369; + ∼
207.29 good.)] E; ∼.$_\wedge$
208.2 $_\wedge{}^{(a^b)}c$] E; (∼
209.1 PAGES 198–99] E; PAGE 47

Line-End Hyphenation

188.18 non-relative *Also* 196.11
196.25 (*m* + *n*)-fold
199.18 alio-relative
200.35 non-occurrence

20. *Logic of Plural Relatives, 1880*

The copy-text, MS 371, consists of three unnumbered leaves torn from a pad, with remains of glue evident at the top of each leaf. The leaves of white paper, lined on one side only, with a "HURLBUT" watermark, measure $7\frac{7}{8} \times 10$ in. and are inscribed in light blue ink. There are numerous alterations.

Textual Note

211n.3 p. 78] Peirce left space before the closing parenthesis, but forgot to insert the page number.

Emendations

210.20 196] E; 45
211.9 two,] E; ∼‸
211.10 *x*,] E; ∼‸
211n.1 "Logic of Relatives"] E; ‸*Logic of Relatives*‸
211n.1–2 "New . . . Categories."] E; ‸*New . . . Categories*‸‸
211n.3 *Jour.*] E; ∼‸
211n.3 vol. 1,] E; ∼‸ ∼.
*211n.3 78] E; *not present*

Line-End Hyphenation

211.3 non-relatives

21. *Results of Pendulum Experiments, 1880*

Copy-text is P 168, the publication in the *American Journal of Science and Arts* [168], which is emended by MS 365, Peirce's ink-inscribed, pre-copy-text document in the National Archives [365]. "ART. XXXVI" precedes, and "by C. S. PEIRCE, Assistant Coast and Geodetic Survey. [Published by authority of C. P. Patterson, Superintendent.]" follows the title in AJSA. MS 365, received 1 September 1880 by the Survey office, lacks the last paragraph, which suggests a missing page. The penultimate paragraph was originally set as a single-page footnote to a deleted passage at the end of the previous paragraph. The P 174 republication in the *London, Edinburgh, and Dublin Philosophical Magazine and Journal of Science*, 5th ser. 10 (November 1880): 387 [174], has also been collated, and the results appear in the Rejected Substantive Variants. MS 377a, an offprint of P 168 among the Harvard Peirce Papers [377a], has the two marginal ink inscriptions that are noted in the Rejected Substantive Variants. Manuscript 377b, another offprint, is bound into a volume in the Boston Public Library (call no. 5920a.52) with "PEIRCE|LOGIC OF|RELATIVES" on the spine of what was a gift to the library from T. S. Perry, a friend of Peirce. Below " + 2$^{s}_{\cdot}$89" in blue ink is "243" or "233" probably in Peirce's hand. Page references within this item refer to the original publications.

Emendations

212.11 0$^{m}_{\cdot}$9932052] 365; 0•9932052^{m} 168•0$^{metre}_{\cdot}$9932052 174 (*the terminal superscript is moved to above the decimal point as in* 365 *at the relevant places in lines* 9–13, 15–6, 18–9, 32 *and is always spelled out and placed in the comparable position in* 174)
212.19 *Geodesy*] E; *rom.* 168• ∼‸ 365•'Geodesy,' 174
213.1 Inv.].] 365, 174; ∼.]‸ 168
213.3 metre] E; meter *Also* 213.6, 8

Line-End Hyphenation

212.7 sea-level

Rejected Substantive Variants

212.4 the] my 365
212.4–5 obtained . . . Survey,] for the lengths of the seconds pendulum 365
212.6–7 for . . . pendulum,] *not present* 365
212.8–9 to be = 1 : 293] $\frac{1}{293}$ 365

212.12 Paris] −2439 ~ 377a
212.13 Berlin] +623 ~ 377a
212.15–16 a . . . mean,] *not present* 365
212.21 = 1 : 292.2] *not present* 365
212.22 experiments] ~ themselves 365
212.22–23 This . . . important.] *not present* 365
212.24 It . . . result] The value 365
213.3–10 The . . . used.] *not present* 365
213.10 used.] ~ .—Silliman's *American Journal,* October 1880. 174

22. *The Logic Notebook, 1880*

The copy-text, MS 375, consists of seven very thin and light, grayish blue, and unlined leaves measuring $8\frac{13}{16} \times 9\frac{15}{16}$ in. They are taken from a notebook whose covers are missing, are chipped and fragile, and are inscribed in blue ink. There are numerous alterations. The pages are numbered in the hand of Don Roberts.

Emendations

214.9 *et seq.* *x*] E; *rom. All lowercase variables rom.*
214.18 C.] E; ~‸
214.21 *y*'s.] E; y's‸
214.22 *y.*] E; y‸
214.24 *y*'s] E; ys *Also* 214.25
215.8 unknowns.] E; ~‸
215.26 $\bar{x} \times y =$] E; $\bar{\mathrm{x}} \times \mathrm{y}$ ‸
215.27 $\bar{x} \times \bar{y} =$] E; $\bar{\mathrm{x}} \times \bar{\mathrm{y}}$ ‸
216.23 *each,*] E; ~‸
216.30 be:] E; ~‸
217.3 proposal.] E; ~‸

23. *A Boolian Algebra, 1880–81*

Copy-text is MS 378, which consists of seven numbered white pages, measuring $8\frac{3}{8} \times 10\frac{13}{16}$ in., of thin, white, laid, and unlined paper watermarked "THE AMERICAN LINEN PAPER." The seven leaves, which are inscribed in blue ink, are in poor, brittle condition. There are many alterations.

Emendations

219.5 'If . . . P'] E; ‸~ . . . ~‸
219.21 accordingly] E; according
219.27(1) X's] E; Xs *Also* 219.28, 220.3
220.4 *et seq.* $\frac{\varphi}{x}$] E; *rom. All lowercase x's rom. except* 220.10, 11(1), 22(1)
221.8 ∴] E; *not present Also* 221.19
221.11 S's] E; Ss
221.11 M's] E; Ms *Also* 221.12
221.12 P's] E; Ps

24. *Axioms of Number, 1880–81*

The copy-text, MS 380, is a black-ribbon, italic double-spaced typescript consisting of three leaves of medium-light, laid, unlined white paper measuring $8\frac{1}{2} \times 10\frac{7}{8}$ in., with "THE AMERICAN LINEN PAPER" watermark. The top right corners are torn off. "By C. S. PEIRCE" appears below the title, and the second page is corrected in black ink. Pre-copy-text restorations are from MS 379, which measures $7\frac{13}{16} \times 9\frac{15}{16}$ in. and has a "J. C. BLAIR PA." watermark on its four unnumbered leaves, inscribed in faded black ink [379]. The rejected substantive variants are from MS 379.

Textual Notes

223.7 Peirce wrote "[Insert here XI]" in MS 379.
223.19 Peirce wrote "[Insert here XV]" in MS 379.
223.28 *et seq.* In this italic typescript the lowercase variables are not underlined.

Emendations

223.17 than] 379; then
223.27 *Definitions* . . . *Multiplication*] E; *rom.*
*223.28 *et seq.* *m*] E; *rom. All lowercase m's and n's rom.*

Rejected Substantive Variants

222.16 or as less] or less
223.2 any] that is, ~
223.3–4(2) smaller . . . smaller] greater . . . greater
223.5 smaller] greater
223.7 IX] XI
223.10 X] IX
223.13 XI] X
223.19 XIII] XV
223.22 XIV] XIII
223.23 The . . . a] In any counting, the final number of the
223.24 XV] XIV
223.28 numbers, *m* and *n*,] ~‸ n_1 *and* n_2‸
223.29 count of] ~ ~ a lot of
223.30 *m* and *n*] n_1 *and* n_2
224.1 *n*] n_1
224.3 *n*] n_1
224.5 *m*] m_2

25. *Associative Algebras, 1880–81*

The copy-text, MS 381, consists of four unnumbered leaves measuring $7\frac{7}{8} \times 9\frac{15}{16}$ in., with a "HURLBUT PAPER CO." watermark. The white paper, which is lined only on one side, is inscribed in blue ink. There are relatively few alterations.

Emendations

225.4 *et seq.* *pq*] E; *rom. All lowercase variables rom. except i* 226.2
225.5 q′‸] E; ~.
225.5 0.] E; ~‸ *Also* 225.23, 226.9
225.13 $(a_m A^m)$] E; ‸~‸
225.23(2) and] E; *not present*
226.8 as] E; *not present*
226.8 or] E; *not present*
226.9 then] E; *not present*
226.24 etc.] E; ~‸ *Also* 226.25

26. *Associative Multiple Algebra, 1880–81*

The copy-text, MS 382, is inscribed in faded blue ink on nine unnumbered leaves measuring $7\frac{7}{8} \times 9\frac{15}{16}$ in., with a "HURLBUT PAPER CO." watermark. The white paper is lined only on one side. There are many alterations in the manuscript, which has "By C. S. Peirce" below the title.

Emendations

228.15 *Linear Associative Algebra*] E; *rom.*
228.28 Vol. 1, p.] E; ~‸ 1, p‸
229.3 *et seq.* *i*] E; *rom. All lowercase variables rom. except* 229.8, 9, 12, 13, 14(2,3,4), 20; 230.16–18, 23; 232.3, 6, 10, 18, 21, 22
229.14 etc.] E; ~‸ *Also* 229.15, 16, 17, 21, 22, 23, 24
229n.2 *Proceedings*] E; *rom.*
229n.2 body,] E; ~.
231.22 aspects‸] E; ~.
231.30 III.] E; ~‸
231n.1 B.] E; ~‸
231n.1 remarks,] E; ~‸
231n.1 *Proceedings . . . Sci.*] E; *rom.*
231n.2 1875.] E; ~‸
231n.3 *Proceedings . . . Sciences,*] E; *rom.*
231n.3 Oct.] E; ~‸

27. *Division of Finites, 1880–81*

The copy-text, MS 383, consists of seven numbered leaves measuring $7\frac{7}{8} \times 9\frac{15}{16}$ in., with a "HURLBUT PAPER CO." watermark. The white paper, which is lined only on one side, is inscribed in black ink. There are many alterations.

Emendations

233.5 *et seq.* *p*] E; *rom. All lowercase variables rom. except* 233.15, 20, 23; 234.2, 15(7), 16(4); 235.8; 237.8
233.5(4) $\neq$] E; =
235.13 scalar] E; salar
235.14 remainder] E; remaineder
235.29 1] E; *i*
235.30 0,] E; ~‸
235.32 1‸] E; ~.
235.32 *v*.[E; v‸

28. *Jevons's Deductive Logic, 1881*

Copy-text is P 198, the publication in the *Nation.* Haskell's *Index to the Nation* lists Peirce as the author of this unsigned review.

Emendation

238.5 ¶Some] E; ‸— ~

Line-End Hyphenation

239.15 wherein

29. *Rutherfurd's Rulings, 1881*

Copy-text is MS 392, a typescript now in Record Group 23 in the National Archives which Peirce himself corrected [CSP]. It is discussed in the Essay on Editorial Method. Readings cited in the Emendations and Rejected Substantive Variants are from the Coast Survey Office changes in the typescript [CSO] and P 204, the printing in *Nature* 24 (21 July 1881): 262 [204]. "C. S. PEIRCE" is typed at the end of MS 392.

Emendations

240.1 *Rutherfurd's*] CSO, 204; Rutherford's
240.9 Rutherfurd] CSO, 204; Rutherford
240.12 Rutherfurd's] CSO, 204; Rutherford's

Rejected Substantive Variants

240.3(1) of] ~ C.P. Patterson, CSO, 204
240.7 my work,] this ~, CSO • this ~‸ 204
240.8(1) that] *not present* 204
240.9 Mr. Rutherfurd] ~ Lewis ~ CSO, 204
240.13 2nd] second 204
240.17 I shall] are CSO, 204
240.17 publish] to be published CSO, 204

30. *Methods of Science, 1881*

The copy-text, MS 396, consists of three purple-ribbon, unnumbered italic typewritten leaves of light, laid, thin white paper in deteriorating, chipped condition. They measure $8\frac{3}{8} \times 10\frac{7}{8}$ in. and are watermarked "THE AMERICAN LINEN PAPER." "By C. S. PEIRCE" follows the title, and Peirce's corrections are in blue-black ink. All lowercase variables in this italic typescript are, according to Peirce's intentions, represented in italics. An accidentally typed "Z" appears at the end of the text.

Emendations

242.10 neither] E; neirher
242.11 to] E; ~ to
243.1 hear] E; here
243.11 B,] E; ~.
243.14 equation] E; e5uation
243.21 susceptible] E; suscepti ~
244.2 proposition] E; proposirtion
244.6 occurring] E; occuring

31. *Methods of Reasoning, 1881*

Copy-text is MS 397, a blue-ribbon, italic typescript of twenty leaves of light, laid, unlined white paper measuring $8\frac{3}{8} \times 10\frac{7}{8}$ in., with "THE AMERICAN LINEN PAPER" watermark. Peirce's revisions on pages 1–5 are inscribed in black ink. There are nine additional typewritten and autograph pages of earlier versions of the manuscript. Three emendations correct the mistyping, due to the slipping of paper, at the bottom of three pages.

Textual Note

248.16 fishing. In] It is uncertain whether Peirce intended to delete "It . . . proposition," which he may have partially rewritten while typing from another draft, or whether it resulted from an eye skip in transcription from a no longer extant draft.

Emendations

245.27 one] E; on ~
246.40 however,] E; ~‸
247.27 termed] E; terned
248.7 know] E; k ~
*248.16 fishing. In] E; fishing. It is easy to see that every hypothetical proposition|In
248.21 possibilities] E; posssibilities
248.23 every] E; everq
248.24 categorical] E; categoriocal
248.28 1890] E; 1980
248.29 did] E; di ~
248.32 States] E; Stetes
248.38 compared] E; compeared
249.15 may be] E; may
249.26(2) of] E; ~ of
250.7 Very] E; ~ Very
250.9 is,] E; ~‸
250.20 on] E; ofn
250.30 say,] E; ~‸
250.36 inferential] E; infere-|tial
251.3 indispensible] E; indispensable
252.33 P,] E; ~. *Also* 252.36
255.9 Baroko] E; Bocardo
255.14 Bokardo] E; Baroko
255.17 propagated] E; propogated *Also* 255.18
255.26 passed] E; assed
255.31 ponens] E; monens

Line-End Hyphenation

246.13 cannon-balls

32. *Mouse-Trap Problem, 1881–82*

Copy-text is MS 398, which consists of two numbered leaves of medium-weight, laid, white paper, blue-lined only on one side and measuring $7\frac{7}{8} \times 9\frac{15}{16}$ in. They are inscribed in blue ink, have a "HURLBUT PAPER CO." watermark, and were folded as if for mailing. The author line is crossed out, possibly by Peirce, who may have considered sending off this item for publication, but perhaps changed his mind. It is also possible that it was submitted, but then rejected and returned to him. There are relatively few alterations.

Emendations

257.3 1857] E; 1855
257.9 *a*] E; *rom. Also* 257.17
257.9 *b*] E; *rom. Also* 257.17
257.18 *r*] E; *rom.*

33. *Note on 0^0, 1881–82*

Copy-text is MS 399, a black-ribbon, italic typescript consisting of three unlined leaves of medium-light, laid, white paper. The first and third leaves, watermarked "THE AMERICAN LINEN PAPER," measure $8\frac{1}{2} \times 10\frac{7}{8}$ in.; the second leaf, which is unwatermarked, measures 8×10 in. Pages two and three are numbered by typewriter in the upper right corner, and Peirce's few alterations are in black ink. Two additional fragmentary leaves of related material and notations also survive.

Emendations

258.5 *et seq.* *x*] E; *rom. All lowercase variables rom.*
259.1 minus 1,] E; minus,
259.3 $^{\cos}{}_\wedge$] E; $^{\cos.}$
259.3 $E^{r't'\cdot}$] E; $E^{r'\cdot t'}{}_\wedge$
259.3 $^{\sin}{}_\wedge$] E; $^{\sin.}$
259.4 $\cos_\wedge$] E; $\sim.$
259.4 $\log_\wedge$] E; $\sim.$
259.4 $\sin_\wedge$] E; $\sim.$
259.5 2EC] E; 2NC
259.8 ∞,] E; $\sim_\wedge$ *Also* 259.19
259.26 1.4444.] E; $\sim_\wedge$
259.30 1,] E; $\sim_\wedge$
259.32 1.4444,] E; $\sim_\wedge$

34. *Propositions and Syllogisms, 1881–82*

Copy-text is MS 400, which consists of six numbered, medium-weight leaves of white paper, lined only on one side, measuring $7\frac{13}{16} \times 9\frac{13}{16}$ in., with a "MASSASOIT COMPANY" watermark. They are inscribed in faded blue ink, and there are relatively few alterations. In the top right margin of the first page, "1883" is pencil-inscribed in a hand other than Peirce's.

Textual Note

262.12 but the other] The manuscript reads "but for the other" but Peirce deleted "the" by mistake. The sense requires the deletion of "for" and the retention of "the."

Emendations

260.4 order.] E; $\sim_\wedge$
260.18 *et seq.* *n*] E; *rom. All lowercase variables rom.*
261.3 quality).] E; $\sim$.)
261.4 distinction.] E; $\sim_\wedge$
261.5 Order.] E; $\sim_\wedge$
261.28 $\text{first}_\wedge$] E; $\sim$,
261.28 $\bar{p}\gamma$).] E; $\sim$.)
262.3(1) $_\wedge\breve{A}$] E; ($\sim$
*262.12 the] E; for
263.5 C)}] E; $\sim)_\wedge$

35. *Boolian Algebra, 1881–82*

Copy-text is MS 401, which consists of four numbered leaves of white paper measuring $7\frac{3}{4} \times 9\frac{7}{8}$ in., with a "HURLBUT PAPER CO." watermark.

They are lined only on one side, are inscribed in blue-black ink, and are generally soiled, browned, and brittle. There are relatively few alterations.

Emendations

264.9 *et seq.* $(x - f)$] E; *rom. All lower-case x's, y's, and z's in display formulas rom. except* 266.5
264.22 signifies‸] E; ~.
264.24 etc.] E; ~‸ *Also* 265.2, 3
266.3 in] E; it
266.4 *x, y, z,*] E; *rom.*

36. *Fundamental Proposition of Arithmetic, 1881–82*

The copy-text, MS 402, is inscribed in faded blue ink on four numbered leaves of white paper measuring $7\frac{3}{4} \times 9\frac{7}{8}$ in., with a "MASSASOIT COMPANY" watermark. There are relatively few alterations.

Emendations

267.5 in] E; is
267.12 A's] E; As *Also* 267.16, 21, 22; 268.13, 20
267.24 will] E; with
268.9 λ'd] E; λd
268.13 granted).] E; ~.)
268.20 α's] E; αs
268.21 line).] E; ~)‸

37. *Comparison of Metre with Wave-Length, 1881–82*

The copy-text, MS 403, consists of sixty-seven leaves of medium-weight and stiff white paper measuring 8×10 in., with a "MASSASOIT COMPANY" or a "Westons Linen Record 1881" watermark. Except for two of the "MASSASOIT" leaves, which are lined on both sides, all leaves are lined only on one side, and all are inscribed in blue-black India ink; seven of the leaves also contain pencil corrections and the marginal notations that certain figures and numbers are either "right" or "wrong." The sixty-seven leaves of this incomplete manuscript are numbered 1–4, 6–25, and 41–83, with alterations on virtually every page. There are three additional leaves, not published here, of rough and incomplete computations.

Textual Notes

272.38 pointings,] The asterisk that follows the comma suggests that a footnote was either on a now lost page, or that Peirce did not inscribe the citation. The bottom third of manuscript page 11 is blank.
278.2–3 Corr. for judgment.] This head does not appear in the MS. It is reproduced here (as Peirce often did when a table continued onto the next page) for the reader's convenience—as are those in similar instances at 287.1, 288.1, and 289.1–2.
282.21 After 385″.7, Peirce wrote at mid-page the sentence that is emended out. However, he left the statement incomplete and the rest of the page blank. He then started over at the top of the next manuscript page, without having crossed out the incomplete sentence. What appears as an "I" is probably the beginning of an "F."
290.1 Preceding this line, at the top of p. 65 in the manuscript, Peirce has written a bracketed note to himself: "Change all the +s to −s." All numbers for 5th through 10th centimetres originally appeared with plus signs (or no sign, meaning plus). The editors have removed the bracketed note and have inserted minus signs for all numbers in the table.

Emendations

269.14 seconds‸] E; ~'
269.23 Rutherfurd] E; Rutherford *Also* 273.12
269n.1 Mayer] E; Meyer
269n.1 *New . . . Cyclopaedia*] E; *rom.*
270.1 pallet] E; palet *Also* 270.3

270.4 centimetre] E; centiimetre
270.7 No.] E; ~‸ *Also* 270.10; 276.22, 28; 278.6; 282.34, 35; 283.7, 32; 288.9, 12; 297.26, 28, 31, 35, 39; 298.1, 2, 3, 14, 15, 16, 18, 20, 23
270.10 pallet] E; palette
270.30 diffraction-plates] E; ~‸~
270.38 diffraction-plate] E; ~‸~ *Also* 274.31
271n.1 "On . . . Rutherfurd's] E; ‸*On . . . Rutherford's*
271n.1–2 Diffraction-Spectra." *American . . . Mathematics*] E; *Diffraction-Spectra.*‸ American . . . Mathematics
271n.2 Vol.] E; ~‸
271n.2 p.] E; ~‸
271n.2 330.] E; ~. Reprinted in this volume.
272.8 one-millionth] E; ~‸~
273.3 two-millionth] E; ~‸~
273.7 three-millionth] E; ~‸~
273.11 Messrs.] E; ~‸
273.11 Brothers] E; Bother
273.17 screw-heads] E; ~‸~
273.18 micrometer] E; mircometer
273.23 mahogany] E; mahogony
274.18 focus] E; focuss
274.40 latter.‸] E; ~..
275.18 cross-wire] E; ~‸~ *Also* 275.22
275.28 eye-piece] E; eyepiece
276.22 54.5.] E; ~‸
276.25 Corr.] E; ~‸ *Also* 278.27
276.27 red.] E; ~‸
276.33 32″4] E; 32.4
277.4 pointing‸] E; ~.
277.10 corr.] E; ~‸ *Also* 278.27
277.12 39″5] E; 39.5
277.28 green.] E; ~‸
277n.1 Microscope] E; Microsope
278.4 18″7] E; 18.7
278.7 2^{nd} pointing] E; *not present*
278.8 3^{rd} pointing] E; *not present*
278.10 55°0.] E; ~‸
278.27 Temp.] E; ~‸ *Also* 292.3
278.28 barom.] E; ~‸
278.29 53″3] E; 53.3
279.8 Feb.] E; ~‸ *Also* 279.11
279.12 Mar.] E; ~‸
279.32 Telescope‸] E; ~.
279.41 *et seq.* $3^{r}776$] E; $3^{r}776$ *All superscript lowercase r's rom.*
280.12 Ther.] E; ~‸ *Also* 281.3
280.12 above‸] E; ~.
280.32 Micrometer.] E; ~‸ *Also* 281.1, 23, 34
281.5 readings‸] E; ~.
281.14 61°8‸] E; ~. *Also* 281.36
281.25 63°3‸] E; ~.
281.25 63°4] E; 63.4
*282.21 385″7] E; ~|The following is the *resumé* of measures with Gitter I
282.22 résumé] E; resumé *Also* 283.6
282.29 1879,] E; ~‸
282.30 ‸ Dec.] E; , ~‸
282.34–35 Rutherfurd's] E; Rutherfords *Also* 284.14
283.9 Dec.] E; ~‸
283.14 double‸centimetres] E; ~-~
283.26 microscopes] E; micro|copes
283.28 magnifying] E; manifying
284.21 $\frac{14}{681}$·] E; ~‸
284.23 ☞.] E; ~‸
284.25 stated.] E; ~‸|25.3
284.30 Temp‸] E; ~. *Also* 284.33; 285.11, 14, 17, 20, 28, 29, 30, 31, 33, 34, 40; 286.7, 12, 13, 14, 16, 22, 24, 27; 287.37; 289.23; 294.3, 14; 295.2
284.30 65°] E; ~‸
284.36 72°.] E; ~‸
285.1 No‸] E; ~. *Also* 285.29
285.2 67°.] E; ~‸
285.14 69°.] E; ~‸
285.28 76°.] E; ~‸
286.7 70°.] E; ~‸
286.21 23‸] E; ~.
286.21 63°.] E; ~‸
286.30 June 10.] E; June 10 June. 10.
286.33 Eighth] E; Eigth
286.38 1879,] E; ~‸
286.39 4‸] E; ~. *Also* 287.36
287.5 3‸] E; ~. *Also* 287.37
287.6 comparisons.] E; ~‸
287.37 position.] E; ~‸
288.14 Diff.] E; ~‸
288.28 lines,] E; ~.
289.19 coëfficient] E; coefficient *Also* 298.22, 23
289.23(1) Difference‸] E; ~.
290.1 Centimetre‸] E; ~, *Also* 290.16(2)
290.3–22 −] E; +
290.32 Cm.] E; ~‸
291.2 3.] E; ~‸ *Also* 295.24
291.4(2) Temp] E; *not present*
292.1 H.] E; ~‸
292.4 73°9] E; 73‸°9
292.21 I.] E; ~‸
295.26 Temp‸] E; *Temp.*
295.26 Excess] E; *ital.*
296.1 4.] E; ~‸
296.2 Temp] E; *ital.*

296.11 0.25] E; ~|3
296.16 mean.] E; ~‸ *Also* 296.25
297.1 Nos.] E; ~‸ *Also* 297.38
297.3–4 By addition of . . . cm] E; *ital.*
297.23 Decimetre] E; Decimete
298.4 coëfficients] E; coefficients

Line-End Hyphenation

273.22 three-legged
273.35 well-corrected
284.10 screw-revolutions
284.24 otherwise

38. *Logic of Number, 1881*

Copy-text is P 187, the publication in the *American Journal of Mathematics* [187], which has "By C. S. PEIRCE" following the title. It is emended by MS 393, a pre-copy-text italic typescript with Peirce's ink inscriptions [393], and by his annotated offprints, MSS 411 and 412 [411, 412]. Unidentified citations in the Rejected Substantive Variants are from MS 393. The italic typeface of MS 393 is represented in roman as Peirce intended, except for lowercase italic variables.

Emendations

301.30 2^{nd}] E; 2d *Also* 302.5, 35; 307.20, 29, 40; 308.13
305.19 x':] 393; ~‸
306.6 addition:] E; ~. 187 • ~; 393
306.23 relation,] 411, 412; ~‸
306.23 being c'd by] 411, 412; c, with
306.25 c of] 411, 412; c'd by *Also* 306.26
307.22 3^{rd}] E; 3d *Also* 307.27, 308.15
309.15 but by] E; by but

Rejected Substantive Variants

299.1 *On . . . Number*] *not present*
299.4–300.22 Nobody . . . quantities.] *not present*
300.24–301.14 A . . . untrue.] In a discrete system, a proposition is proved to be true, if of any quantity then of every greater (or less) quantity, as soon as it has been shown to be true, if of any quantity then of the next greater (or less.) ¶A system of discrete quantity is either limited, semilimited, or unlimited. A limited system is one which has an absolute maximum and an absolute minimum quantity; a semilimited system has one (generally considered a minimum) without the other; an unlimited has neither.
301.15 Semi-infinite] ~-limited
301.15 *Quantity.*] ~. ¶A proposition is shown to be true of every quantity in a semi-limited system, as soon as it is proved of the minimum and of every quantity greater.
301.16 semi-infinite] ~-limited *Also* 304.25
301.17 and] *not present*
301.21 other cases,] general,
301.23 other cases‸] general,
301.25–27 It . . . themselves.] *not present*
302.7(1) +] −
302.17 $x = n$] $x = 1$
302.26 associative principle:] definition of addition;
303.31 $1 \cdot yz$] $y(1z)$
304.11 $1 + n$] $1 \cdot 1 + 1n$
304.12 definition of multiplication] distributive principle
304.22 *Discrete . . . directions*] *Unlimited Discrete Simple Quantity*
304.23 number . . . directions] unlimited quantity
304.24 but] ~ in unlimited number
304.33 have] *not present*
305.12 semi-infinite] semilimited *Also* 306.24
305.13 doubly infinite] unlimited
306.21–22 In . . . $\prec$ 1.] *not present*
306.24 simple] single
306.27–29 In . . . $\prec cs$.] *not present*
306.32–33 Let . . . [$\bar{s}$].] *not present*
307.1 "positive . . . number] *not present*

307.15 simple correspondence] count
307.17–308.17 Since . . . *x.*] *not present*
308.33–308.27 every . . . for 1.] *not present*

39. *On an Algebra, 1881*

Copy-text is P 188, the publication in the *American Journal of Mathematics,* which is emended by the printed errata on the verso of the Index at the front of the AJM volume [er]. Like Peirce's other notes to his father's *Linear Associative Algebra,* this one has "[C.S.P.]" at the end. *Linear Associative Algebra* first appeared in 1870 (Washington City), when Benjamin Peirce distributed lithograph copies among his friends. Following the AJM publication in 1881, the book was republished by D. Van Nostrand in New York in 1882; its title page indicates that this is a "New Edition, with Addenda and Notes, by C. S. Peirce, Son of the Author" and that the text is "Extracted from *The American Journal of Mathematics.*" The P 188 errata are completely reset in the Van Nostrand edition, where the present note appears on page 36.

Emendations

312.6–7 *Memoirs . . . Sciences.*] E; *rom.*
312.22(1) j_i] er; j
312.22 $(1 + \mathfrak{J}i_1)$] er; $(1 - \mathfrak{J}i)$
312.23 *Proceedings . . . Society*] E; *rom.*
312.24 "Memoir . . . Matrices"] E; ‸ ~ . . . ~‸

40. *On a Class of Algebras, 1881*

Copy-text is the publication in the *American Journal of Mathematics,* which has "[C. S. P.]" at the end of the note. It appears on pages 94–98 in the Van Nostrand edition.

Emendations

313.6 e_3] E; a_3
314.11 $b_{53}j + d_{53}l$] E; $b_{35}j + d_{35}l$
318.21 $(2q + 1)j$] E; (~)
318.22 coëfficients] E; coefficients

Line-End Hyphenation

317.19 intersection

41. *Relative Forms of Algebras, 1881*

Copy-text is the publication in the *American Journal of Mathematics,* which is emended by pre-copy-text restorations from MS 407 [407]. "By C. S. PEIRCE" follows the title in AJM. The note appears on pages 125–29 in the Van Nostrand edition. Roman variables in MS 407, both lowercase and uppercase, are listed in the Rejected Substantive Variants only when part of another substantive variant.

Emendations

319.13 coëfficients] 407; coefficients
319.18 of] 407; o
319.23 etc.] 407; *not present*
320.7 *aI*] E; *aJ*
320.27 c_{34}] E; c_{34}
321.9 *Brief*] E; *A brief*
321.21 0,] 407; ~.
321.22 – ɪz,] 407; – ~‸

Line-End Hyphenation

319.15 *non-relative*

320.20 non-relative *Also* 320.23

Rejected Substantive Variants

319.8 etc.,] *not present*
319.11 except] but
319.24–25 in . . . term] one term of which
319n.1 *[footnote]*] *not present*
320.2 taken to be] *not present*
320.5 We] Let us
320.11 *[formula]*] $i' = (I:A) + a_1(I:I) + b_1(J:I) + c_1(K:I) + \text{etc.}$
320.15 *[formula]*] $+ a_2(I:J) + b_2(J:J) + c_2(K:J) + \text{etc.}$
320.19 ultimate] *not present*
320.26 *[formula]*] $k'l'A = k'L = a_{14}I + b_{14}J + c_{14}K + \text{etc.}$
320.27 $a_{12} = a_{34},\ b_{12} = b_{34},\ c_{12} = c_{34}$, etc.,] $a_{12} = a_{14},\ b_{12} = b_{14},\ c_{12} = c_{14}$, ∼.‸
320.30–32 *[formula]*] $ijm = (a_{12}i + b_{12}j + c_{12}k)\ m| = (a_{12}a_{15} + b_{12}a_{25} + c_{12}a_{35} + \text{etc}_\wedge)i| + (a_{12}b_{15} + b_{12}b_{25} + c_{12}b_{35} + \text{etc.})j + \text{etc.}$
320.34–35 *[formula].*] $i'j'm'A = (a_{12}a_{15} + b_{12}a_{25} + c_{12}a_{35} + \text{etc.})\ I| + (a_{12}b_{15} + b_{12}b_{25} + c_{12}b_{35} + \text{etc.})\ J| + \text{etc}_\wedge$
321.7 Thus,] It will be seen that
321.8 *i.e.* (see] or (See
321.11 It] In
321.15 is . . . the] may be otherwise stated thus. The
321.22 $u, \mathfrak{r}z, 0, x - v, 0, -\mathfrak{r}z$] $u, rz, x, v, 0, -rz$
321.33 *[formula]*] $i_1 = i + m,\ j_1 = \mathfrak{r}^2 j,\ k_1 = k + \frac{1}{r}i + \frac{1}{r}m,\ l_1 = rl + j,\ m_1 = -m$
321.34 where] when
321n.1 A . . . above] This proof, in an abridged form
322.10 a vector] *not present*
322.17 corresponding to] *not present*

42. *Algebras with Unambiguous Division, 1881*

Copy-text is the publication in the *American Journal of Mathematics*, which is emended by pre-copy-text restorations from MS 408 [408]. "By C. S. PEIRCE" follows the title in AJM. The note appears on pages 129–33 in the Van Nostrand edition. All readings to the right of the bracket in the Rejected Substantive Variants are from MS 408, whose roman lowercase and uppercase variables are listed only when part of another substantive variant.

Textual Note

326.28 The "s'''" appears with only one or two primes in some printings, but with spacing for three primes. In the printing of the bound volume of AJM, the first two primes are clearly visible, the third is discernible, and the spacing is for three primes.

Emendation

322.21 coëfficients] 408; coefficients *Also* 323.5

Rejected Substantive Variants

323.23–24 irresoluble] irresolvable
323.27 $\left(\frac{A-s}{t}\right)^2$] $\frac{\mathrm{A}-\mathrm{s}}{\mathrm{t}}$
323.32 here] *not present*
324.4 cannot] ∼ so
324.26 them such] this
324n.1–2 *[footnote]*] *not present*
325.27 *i.e.*] Then,
327.11 quotient.] ∼. [C.S.P.]

43. *Algebra of Relatives, 1882*

Copy-text is P 220, Peirce's six-page brochure privately printed in Baltimore. It is dated 7 January 1882, with a 16 January 1882 postscript regarding Cayley's *Memoir on Matrices.* Although there is no indication in P 220 where the brochure was printed, its format, paper, and untrimmed page size clearly match that of the *American Journal of Mathematics.* It appears that, because of possible objections by Cayley and J. J. Sylvester, Peirce decided not to distribute the brochure.

Emendations

328.7 coëfficient] E; coefficient *Also* 328.15, 16, 26; 329.10, 17, 18; 332.4; 333.5
328n.1 coëfficients] E; coefficients *Also* 329.18
329.2 $0 \cdot C_{\wedge}$] E; ~.
330.13 $(\bar{\mathrm{I}})_{ii}$] E; $(\bar{\mathrm{I}})_{ij}$
330.20 $(y)_{ij}$.] E; ~$_{\wedge}$
330n.1 "On ... Processes,"] E; $_{\wedge}$~ ... ~,$_{\wedge}$
330n.1 *Am. ... Math.,*] E; *rom.*
331.38 $\neq 0$] $\nleftrightarrow 0$
332.10 *C.*] E; ~$_{\wedge}$
333.25 "Memoir on Matrices,"] E; $_{\wedge}$*Memoir on Matrices,*$_{\wedge}$
333n.1 "Description ... Relatives."] E; $_{\wedge}$~ ... ~.$_{\wedge}$
333n.1 *Memoirs,*] E; *rom.*
333n.2–3 "On ... Algebra."] E; $_{\wedge}$~ ... ~.$_{\wedge}$
333n.3 *Proceedings*] E; *rom.*
333n.3–4 "Note on Grassmann's ... Extension."] E; $_{\wedge}$~ ~ Grassman's ... ~.$_{\wedge}$
333n.4 "On ... Logic."] E; $_{\wedge}$~ ... ~.$_{\wedge}$
333n.5 *Am. ... Mathematics,*] E; *rom.*

44. *Relative Forms of Quaternions, 1882*

Copy-text is P 226, the publication in the *Johns Hopkins University Circulars,* where "by C. S. PEIRCE" follows the title. Peirce presented this paper before the Mathematical Seminary of the Johns Hopkins in January 1882.

45. *Logic of Relatives, 1882*

The copy-text, MS 413, consists of twelve reassembled, slightly heavy, stiff leaves of white paper lined only on one side, with a "MASSASOIT COMPANY" watermark. They measure $7\frac{3}{4} \times 10$ in. and are inscribed in faded blue ink. There are numerous alterations.

Emendations

336.7 *et seq.* *m*] E; *rom. All lowercase variables rom. except m* 336.10, 11; 338.5; *l* 336.17, 338.17; *ls* 338.23, 25; 339.1(1), 4; *l* † *s* 339.10; *a, b, c* 341.5
336.24 coëfficient] E; coefficient *Also* 339.7
337.2 $x_{\wedge}$:–] E; x.:–
337.8 use] E; used
338.4 etc.] E; ~$_{\wedge}$ *Also* 338.7, 8
338.27 modes of] E; mode
339.15 *l*(*sb*)] E; l(sw)
340.2 I's ... J's] E; Is ... Js
340.8 then] E; Then
341.28 † $\breve{\bar{s}}$] E; † $\breve{\bar{\mathrm{b}}}$

Line-End Hyphenation

336.9 coëfficient

46. *Relative Terms, 1882*

The copy-text, MS 414, consists of six leaves measuring $7\frac{3}{4} \times 9\frac{7}{8}$ in., with a "MASSASOIT COMPANY" watermark. The slightly heavy, stiff white paper is lined only on one side, and is inscribed in black ink. The first leaf is smudged because of water damage, and there are many alterations on the following pages.

Textual Note

343.20–27 Peirce's marginal directions to "Insert Copy A," a new paragraph inscribed on a separate leaf designated "A," have been adhered to. Opposite his directions is the following crossed-out sentence which ended the preceding paragraph: "The relative product of two alio-relatives (under the same limitation) never vanishes, unless one of the factors vanishes; a proposition obviously very important. The relative sum of two negatives."

Emendations

342.7 and] E; as
342.23 self-relative] E; $\sim_{\wedge}\sim$ *Also* 343.21, 27, 28; 344.5
342.25 *et seq.* $(a)_{ii}$] E; *rom. All lowercase variables rom. except a* and *b* 343.4, 8; $j = i$ 342.27; *m* 343.29
343.19 self-relatives] E; $\sim_{\wedge}\sim$
343.23 alio-relative] E; $\sim_{\wedge}\sim$ *Also* 343.25, 27
343.33 non-relative term.] E; nonrelative $\sim_{\wedge}$
343.33(2) non-relative] E; $\sim_{\wedge}\sim$ *Also* 344.1, 4
344.6 self-part] E; $\sim_{\wedge}\sim$
344.6 alio-part] E; $\sim_{\wedge}\sim$

Line-End Hyphenation

342.18 *self-relatives*
343.1 alio-relatives

47. *Remarks on Gilman, 1882*

Copy-text is P 230, the publication in the *Johns Hopkins University Circulars,* which has "by C. S. PEIRCE" after the title. Gilman's and Peirce's papers, which were presented before the Johns Hopkins Metaphysical Club in April 1882, appear on the same page in the *Circulars.* Roman brackets are used in the title of this item to separate the part of it that has been modified by the editors.

Textual Note

347.2 There is space for the *l* at the end of the line in the *Circular,* but apparently the type was dropped.

Emendations

345.1 *Remarks . . . Syllogism"*]] E; Remarks on the above paper
346.3 β.]] E; $\sim$].
346.8 δ.]] E; $\sim$].
346.30 middle.]] E; $\sim$].
346.37 formulae] E; formulæ
*347.2 *l*] E; [*blank*]
347.17 $-\!\!<$] E; $>\!\!-$
347.19 yields] E; yield

48–55. *Report of a Conference on Gravity Determinations, 1882*

Copy-text is P 260, the publication in the 1882 *Coast Survey Report* [260]. It is emended by MS 492, Peirce's corrected offprint [492]; by MS 410, which consists of Peirce's autograph "Six Reasons" and "Opinions" [410P] and of amanuensis copies of everything but Peirce's "Opinions" and Herschel's "General Remarks" [410Am]; and by Peirce's "Errata" [er] tipped in at page 503 of P 260. All these are discussed in the Essay on Editorial Method.

Textual Notes

350.14–28 consisted . . . Resolutions.] Peirce inscribed this passage at the top of the first page of the offprint in MS 492, with a direction for its insertion where it now appears. He did not, however, give instructions to replace the original titles with the new ones listed here.

351.1–2 *Letter . . . Herschel.*] This is not the original title, but the one adopted from Peirce's new table of contents in MS 492. The word "Copy" is inscribed at the top center of the first page of Hilgard's letter in MS 410.

352.6 *Reply . . . Herschel*] This is not the original title, but the one adopted from Peirce's new table of contents in MS 492. The underlined word "Copy" is inscribed in the top left of the first page of Herschel's letter in MS 410.

355.34 support. . . .] The ellipsis replaces nine asterisks spread across the whole width of the printed page between the body and complimentary close in P 260; they indicate three deleted paragraphs, which are reproduced below in the Rejected Substantive Variants.

356.1 Peirce wrote in the top margin of MS 492, preceding the title, "COMMUNICATIONS. Comments upon the following papers by the different gentlemen present at the conference are printed along with them; but are distinguished from the text by being placed in square brackets."

368.28 The] Though Peirce inserts quotation marks here and at the end of this paragraph (369.10) in MS 492, on several following pages he removes all such marks. The confusion is probably due to the quotation marks printed in the paragraph on the bottom half of the copy-text page (369.28–370.30) where they are needed because of dialogue between Peirce and Herschel. Peirce's apparently inadvertent failure to delete his added quotation marks has been corrected.

369.14–15 The . . . questionable.] In MS 492 Peirce has written *"Note,"* probably as a reminder to himself to add something like the introductory statement in MS 410, which is incorporated here. That statement parallels another at the head of Herschel's remarks at 365.21–23.

377.1 *Resolutions*] This is not the original title, but the one adopted from Peirce's new table of contents in MS 492.

Emendations

*350.14–28 consisted . . . Resolutions.] 492; were as follows: 260 • *not present* 410Am

350.19 1.] E; $\sim_{\wedge}$ 492

350.20 2.] E; $\sim_{\wedge}$ 492

350.21 Experiments;] E; ~. 492

350.23 Determinations of Gravity;] E; determinations ~ ~, 492

350.23 Assistant] E; Ass't 492

350.24 Major] E; *not present* 260 • Maj. 492

350.26 6.] E; *not present* 260 • $\sim_{\wedge}$492

*351.1–2 *Letter . . . Herschel*] E; *TWO LETTERS.* | [Read to the conference to explain the immediate cause of the meeting.] | No. 1. 260 • *not present* 410Am

351.11 astronomy,] 492; ~; 260 • Astronomy; 410Am

351.12 geology,] 492; ~; 260 • Geology; 410Am

352.3 Yours$_{\wedge}$] 410Am, 492; ~,

*352.6 *Reply . . . Herschel*] E; No. 2. 260 • *not present* 410Am

353.4 Américains] 410Am; Americains

353.4 à] E; á 260, 410Am
353.4 Suisses.] 410Am; ~,
353.6 réversible] E; reversible 260, 410Am
353.7 me paraît] E; ~ parait 260 • meparaît 410Am
353.25 *know*] 410Am; *rom.*
356.2 *Experiments*‸] E; EXPERIMENTS.|By C. S. PEIRCE. 260 • Experiments‸|By C. S. Peirce 410P • Experiments‸|By C. S. Service 410Am
356.14–16 as . . . to it] er; *not present* 260, 410P, 410Am
357.12 seconds] E; second's 260 • seconds' 410P, 410Am *Also* 357.14, 37, 39; 358.4
357.13 reasons.] 410P, 410Am, 492; ~:
357.23 Ste.-Claire] E; ~.‸~ 260 • ~‸‸~ 410P, 410Am
357.34 wave-length] 410P, 410Am; ~‸|~
358.20 62°] 492, er; 60° 260, 410P, 410Am
358.37 Consequently,] 410P; Conse-|sequently‸ 260 • ~‸ 410Am
359.5 less] 492, er; greater 260, 410P, 410Am
359.5 the mountain] 492; it 260, 410P, 410Am
359.14 receptacles] 492; the receptacle 260, 410P, 410Am
359.16 from] 492; *not present* 260, 410P, 410Am
359.24 foot-pounds,] 410Am, 492; ~‸~, 260 • ~-~; 410P
360.13–14 *Gravity,* by] E; GRAVITY.|By Assistant
361.35 seconds] 410Am; second's *Also* 362.1
361n.1 [Mr. . . . completed.]] 492; ‸~ . . . ~.‸ 260 • *not present* 410Am
362.21 viz.,] E; ~‸, 260 • ~‸: 410Am
362.21 Pamlico] 410Am; Pamplico
362n.1–5 [Major . . . great.]] 492; ‸~ . . . ~.‸ 260 • *not present* 410Am
363.6 sea-level] E; ~‸~ 260, 410Am *Also* 363.21, 25 (Sea‸level 410Am)
363.23 Mississippi] 410Am; Missis.|sippi
363n.1–10 [Major . . . disappear.]] 492; ‸~ . . . ~.‸ 260 • *not present* 410Am
364.18 twenty-four] E; 24 260, 410Am *Also* 376.8
364.22 six] E; 6 260, 410Am
365.16–17 submitted. . . . 1882.] E; ~.|CHAS. A. SCHOTT.|MAY 13, ~. 260 • ~,|by Chas. A. Schott|May 13, ~. 410Am
365.18–19 *General* . . . Herschel] E; COMMUNICATIONS. | GENERAL REMARKS UPON GRAVITY DETERMINATIONS.|By Major (now Lieut. Col.) JOHN HERSCHEL, R. E.
365.25 sea-level] E; ~‸~
366.33 *seconds*] E; *second's*
367.8 seconds] E; second's
368.27 accuracy.] E; ~.|J. HERSCHEL
*368.28–369.10 [Mr. PEIRCE.‸ The . . . conference.]] E; ‸~. ~. ~ . . . ~.‸ 260 • [~. ~. "~ . . . ~."] 492
368.29 of] 492; for
369.12 *Gravity Work*‸] E; GRAVITATION WORK.|By C. S. PEIRCE
*369.14–15 The . . . questionable.] 410P; *not present*
369.23–25 [Major . . . Foster.] 492; ‸~ . . . ~.‸
369.28–371.15 [Major . . . streams.]] 492: ‸~ . . . ~.‸ — J. H.|JULY 4, 1883. 260 • *not present* 410P
370.11 coëfficient] E; coefficient
371.19–33 [Mr. . . . matter.]] 492; ‸~ . . . ~." 260 • *not present* 410P
371.20–26 That . . . generalization.] 492; *not present*
371.27–29 ‸The . . . matter.‸] 492; "~ . . . ~."
371.38–372.1 [Major HERSCHEL. The . . . represented.]] 492; ‸~ ~. "The . . . ~." 260 • *not present* 410P
372.4 [Major . . . concurred.]] 492; ‸~ . . . ~.‸ 260 • *not present* 410P
372.7 [Mr. . . . concurred.]] 492; ‸~ . . . ~.‸ 260 • *not present* 410P
372.14–375.10 [Major . . . series.]] 492; ‸~ . . . ~.‸|J. H.|JULY 4, 1883. 260 • *not present* 410P
372n.1 Gehler's *Physikalisches*] E; *Gehler's Physikal.*
372n.1 "Pendel."] E; ‸*Pendel.*‸
373.33 2nd] E; 2d *Also* 375.17
373.35 3rd] E; 3d *Also* 375.22
374.3 be] E; he
374.36(2) vibration-number] E; vibration No.
375.15 coëfficient] 410P; coefficient
375.30 Further] 492, er; ¶XI. ~ 260 • X.|¶~ 410P

375.33 XI. Small] 492, er; Small 260, 410P
375.34–376.4 [Major . . . 1′.]] 492; ‸ ~ . . . ~.‸ 260 • *not present* 410P
375.35 magnitude] E; magni|tude
376.11 *in*] E; *rom.* 260, 410P
376.12 [Major . . . dissented.]] 492; ‸ ~ . . . ~.‸ 260 • *not present* 410P
376.26–28 [Professor . . . sort.]] 492; ‸ ~ . . . ~.‸ 260 • *not present* 410P
*377.1 *Resolutions*] E; CONCLUSIONS PROPOSED BY PROFESSOR NEWCOMB, AMENDED AND ADOPTED BY THE CONFERENCE. 260 • ¶After an extended discussion, the following propositions, formulated by Mr. Newcomb as the conclusions arrived at by the participants in the conference, were unanimously adopted, subject however to verbal amendment: 410Am

Line-End Hyphenation

350.7 following-named
363.16 forty-ninth
364.28 twenty-four
372.24 knife-edges
374.35 superposition

Rejected Substantive Variants

349.1–3 *REPORT . . . MAY*] CONFERENCE ON GRAVITY OBSERVATIONS.|Washington D.C.,|May 13 410Am
350.3 a] *not present* 410Am
350.3–4 (now . . . Col.)] *not present* 410Am
350.5 and Geodetic] *not present* 410Am *Also* 350.9
350.8 May 13, 1882] *not present* 410Am
350.9 Herschel] J. ~ 410Am
350.10 astronomy),] Astronomy); 410Am
350.12 Survey‸] ~, (invited to be present 410Am
350.14–28 The proceedings . . . Resolutions.] ¶The conference began at half past twelve o'clock, Mr. Hilgard presiding. ¶The Chairman read the following letters which were the immediate cause of the present meeting. 410Am
351.8 &c.] ~ ~ 410Am
351.10 beside] besides 410Am
351.23 party] parties 410Am
351.25 day] ~ that 410Am
352.9 Prof. . . . HILGARD:] *not present* 410Am
352.15 days'] day's 410Am
352.20 city] *not present* 410Am
352.27 interpreted] misinterpreted 410Am
353.7 déplorables] déplorable 410Am
353.15 school] schools 410Am
353.16 the] ~ exceedingly 410Am
353.33–34 great room for] room for great 410Am
354.27 imagine,] ~‸ that, 410Am
354.40 and] ~ of 410Am
355.34 support.] ~. ¶Now if I have not worn out all your patience, I will add one concluding personal explanation. ¶I take for granted that in a "conference" some attempt at orderly discussion will be necessary. I am unhappily *quite* unused to speaking otherwise than colloquially, and if it all falls to me to speak at any length I cannot expect to be able to do so with any effect, unless by some accident upon some detail which I thoroughly understood. If on the other hand the discussion takes the form of an *examination,* I think it likely that I might do better. I say this in the interest of the subject solely; my personal inclination being much rather in the direction of *listening* and learning. ¶With a thousand apologies for the length of this letter, I remain 410Am
355.36 J. HERSCHEL.] (Signed) J. Herschel.|Prof. J. E. Hilgard. 410Am
356.1 *Six*] The following papers submitted by Mr. Peirce and Mr. Schott were read and discussed:|Six 410Am
356.5 Now,] ~‸ in my report on the *Measurements of Gravity at Initial Stations* (C. S. Report for 1876, p. 202), I remarked that it may very well yet turn out 410P, 410Am (Measurements . . . Stations *not underlined*)
356.7 earth; for] ~. But subsequent studies have shown that this prediction might have been made more positively. For I have shown, in a memoir

read before the National Academy of Sciences, that 410P, 410Am
356.14 affected,] ~‸ by two latitudes of suspected accuracy; 410P, 410Am
357.13–14 reasons. ¶First] ~. ‸Firstly 410P • ~. [*most of line blank*]|Firstly 410Am
357.25 cubic] solid 410P, 410Am
357.26 determined] ascertained 410P, 410Am
358.2 way.] ~.* [*asterisk refers to the asterisk preceding the following text written vertically in the left-hand margin*] *See my memoir read to the French Academy of Sciences, *Comptes Rendus* [*rest lost in binding but reconstructed from amanuensis copy*] 1870, June 19, with the comments of M. Faye. 410P, 410Am
358.9 a] *not present* 410P, 410Am
358.19 swung,] placed‸ 410P, 410Am
358.20 above] of 410P, 410Am
358.21 nearly] *not present* 410P, 410Am
358.26 bar] *not present* 410P, 410Am *Also* 358.27, 28
358.27 with its] and 410P, 410Am *Also* 358.28
358.29 it is believed] *not present* 410P, 410Am
358.30 present] *not present* 410P, 410Am
358.33 inferences] information 410P, 410Am
359.17 further] farther 410Am
359.30(3) the] *not present* 410P, 410Am
360.7 magnetical] magnetic 410Am
360.8–9 measurements] measures 410Am
360.13–14 *Notes* . . . Schott] *not present* 410Am
361.22 the] *not present* 410Am
362.13 statute] *not present* 410Am
362.32 to be] thus 410Am
363.6 mean] *not present* 410Am
363.7 would] ~ then 410Am
363.24 lower] *not present* 410Am
363.32 cross-] *not present* 410Am
365.14 research] *not present* 410Am
369.11–12 *Opinions* . . . *Work*] *not present* 410P
369.15 questionable.] ~.|C S Peirce 410P
369.19 for the present,] *not present* 410P
369.20–25 But . . . Foster.]] *not present* 410P
369.27 should] ~ either 410P
369.27 omitted] ~‸ or only $\frac{1}{12}$ of it applied. 410P
372.5–6 VII. . . . accordingly.] *not present* 410P
372.8 VIII] VII 410P
372.10 IX] VIII 410P
375.11 X] IX 410P
375.16 and . . . causes] etc. 410P
375.20–21 or . . . causes] etc. 410P
376.5 XII] XI 410P
376.8 XIII.] XII‸ 410P
376.11 XIV] XIII 410P
376.13 XV] XIV 410P
376.15 flexures] flexure 410P
376.19 XVI] XV 410P
376.21 XVII] XVI 410P
376.22 XVIII] XVII 410P
376.23 XIX] XVIII 410P
376.24 total] ~ error 410P
376.29 XX] XIX 410P
377.3–6 1. . . . determined.] *not present* 410Am
377.7 2. A] 1. That a 410Am
377.9–11 within . . . globe] as possible 410Am
377.12–15 3. . . . areas] 2. That a complete gravème metric survey of some limited region is at present of such interest as to justify its execution, while more extended series of determinations covering long lines on the earth's surface are also desirable 410Am
377.16–18 5. . . . apparatus] 3.—That each series of such determinations should be made with the same apparatus, so as to refer the results to the intensity at some central station 410Am
377.19–21 6. . . . part] 4.—That the probable error should in order of magnitude not many times exceed the probable error of local deflection. Since an arc of one second corresponds to a ratio 1:200,000, we may take that as a desirable degree of precision 410Am
377.22 7.‸] 5.— 410Am
377.23 be . . . station.] ~ carefully compared from time to time . . . ~. |¶6.—That all questions of detail, especially those which relate to the means of avoiding error, can best

be settled by individual experiment and research. [*remaining half-page blank*] ¶At half past three o'clock the members of the Conference separated with the understanding that each would read, verbally amend where necessary, and sign the propositions agreed upon. It was further understood that the conclusions arrived at should be given to the public in the form of a communication to the Journal of Science. 410Am

56. *Lecture on Logic, 1882*

Copy-text is P 225, the publication in the *Johns Hopkins University Circulars,* excluding the bracketed subtitle: "Outline of the Remarks made by Prof. C. S. Peirce, at the beginning of his Course, September, 1882." On 4 October 1882, Peirce wrote to his brother James Mills Peirce from Washington:

> I have a most splendid class of eleven in the University. I only wish I could give my whole time to them. I made quite an effective address to persons who might be thinking of joining my class. I spoke of our time as the age of method and said that the highest honors could no longer be paid to the mere scientific specialist but to those who adapted the methods of one science to the uses of another. That a liberal education so far as it regards the intellect means *logic,* considered as the method of methods,—the via ad principia methodorum. That the student ought to feel from the beginning to the end of his course, that in whatever lecture room he is, it is *logic* he is studying.

Textual Note

378.6 The following two paragraphs, not by Peirce, precede the text of the "Introductory Lecture":

> Professor C. S. Peirce began his instruction for the current session by a lecture in Hopkins Hall, on the underlying methods of modern logic. It was attended by instructors as well as students. In compliance with a request for an abstract of his address, which was delivered without notes, the speaker has given the following outline.
>
> Mr. Peirce said that he had requested the instructors to do him the favor to listen to his observations, because he thought that a clear understanding of the purpose of the study of logic might remove some prejudices by leading to a true estimate of its nature.

Emendations

380.6 indispensible] E; indispensable

382.22 *Motibus*] E; *Motu*

57. *Multiple Algebras, 1882*

Copy-text is P 224, the publication in the *Johns Hopkins University Circulars* which, in a bracketed subtitle, is described as an "Abstract of a paper read before the University Mathematical Society, October 18, 1882." The phrase "by C. S. PEIRCE" follows the title of the paper. The *Circulars* printing is emended by MS 426, which consists of two pages of pre-copy-text material [426].

Emendations

383.16 so] E; $\sim$ so

384.12 ζ.] E; $\sim_\wedge$

385.3 twenty-four] E; $\sim_\wedge\sim$

385.19 υ] 426; ν

385.26 substitutions] 426; substltutions

385.37 θ).] E; $\sim)_\wedge$

58. *Oscillation of Pendulums, 1882*

Copy-text is P 218, the publication in the *American Journal of Science.* Peirce's title is preceded by "ART. XXVIII" and followed by the notation, "Communicated by the authority of the Superintendent of the U. S. Coast and Geodetic Survey." The phrase "by C. S. PEIRCE" appears after the title in the printed article.

Emendations

389.7 *Journal*] E; *rom.*
390.4 centre] E; center

59. *Junctures and Fractures, 1882*

Copy-text is MS 427, a fragmentary manuscript consisting of four medium-weight leaves of white paper lined only on one side and measuring $7\frac{7}{8} \times 9\frac{15}{16}$ in., with an "E H OWEN EXTRA SUPERFINE" watermark. The four sheets, as well as five other fragmentary sheets of related material, are inscribed in blue ink. There are many alterations.

Emendations

391.23 numbers,] E; number‸
392.7 *et seq.* *w*] E; *rom. All lowercase variables rom. except c* 393.3, 7

60. *Peirce to Mitchell, 1882*

The copy-text of this letter, L 294, consists of three leaves of medium-weight, white paper, measuring 8×10 in., with a "CONGRESS" embossing in the upper left corner of each recto. The leaves are inscribed in blue ink on both rectos and versos, and are folded as if for mailing. Page numbers are by someone other than Peirce. There are four additional pages of logical notations. Peirce's numerous alterations and the fact that it survives among his papers at Harvard suggest that this is the draft of a letter, rather than an actual letter, to Mitchell.

Emendations

394.9 *et seq.* *b*] E; *rom. All lowercase variables rom.*
396.17 everything] E; everthing
397.2(1) of ——,] E; ~ ——‸
397.17 *[diagram 1].*] E; ~‸
398.7 split] E; slit
398.15 *l* + *b*.] E; l + b‸

61. *Beginnings of a Logic Book, 1883*

The copy-text, MS 443, consists of five folded raglan-edged leaves of medium-weight, unlined, white paper measuring $7\frac{1}{4} \times 9$ in. when unfolded, with a "PARKINS & GROTTO'S PAPYRUS" watermark. They are inscribed in blue ink. There are alterations on all but one page. Graphite and red pencil inscriptions are in a hand other than Peirce's.

Textual Note

401.26–27 See . . . *Logic.*] The sentence added here appears on the lower third of an otherwise blank verso. Though Peirce draws no line for insertion, the sentence is interpreted as a replacement for six deleted lines on the lower third of the opposite recto leaf.

Emendations

400.28 parts:] E; ~‸
401.27 *Syllabus of Logic*] E; *rom.*
401.28 *Logic*] E; *rom. Also* 401.29
401.31 Whewell,] E; ~‸

62. *On Propositions, 1883*

The copy-text, MS 444, consists of three folded raglan-edged, medium-weight leaves of unlined white paper measuring $7\frac{1}{4} \times 9$ in. unfolded; the watermark is "PARKINS & GROTTO'S PAPYRUS." The three-page fragment is inscribed in blue ink, and there are nearly twenty alterations.

Textual Note

402.24–25 "Hamlet's . . . world] Peirce had in part written, '"Hamlet was mad," I refer to a world not to be', then deleted all but "I refer." The sense requires that the first "to" and the word "world" be retained.

Emendations

402.9 to speak] E; speak
402.12 business,"‸] E; ~,",
*402.25 undecided,] E; ~.
*402.25 to] *reinstate*
*402.25 world] *reinstate*

63. *Preface, 1883*

Copy-text is MS 429, which is discussed in detail in the Essay on Editorial Method. It consists of five leaves of medium-weight white paper lined only on one side and measuring $7\frac{7}{8} \times 9\frac{7}{8}$ in., with an elaborate "H 1876" crown device watermark. It is inscribed in blue ink, is signed "C. S. Peirce" at the end, and has many alterations. Pencil inscriptions on the last page are in a hand other than Peirce's. The copy-text [429] is emended by a pre-copy-text leaf [L], corresponding to lines 406.22–407.9, which is part of a reassembled, superseded version of the "Preface" included in the MS 429 folder; by P 268a, the Preface printed in *Studies in Logic* [S]; and by the Preface printed in the *Johns Hopkins University Circulars* [C].

Textual Note

406.22 Venn] In a copy of *Studies in Logic* at the Johns Hopkins University, there is the following note, not in Peirce's hand, in the top margin of page iv, with a lightly drawn line to "Venn": "But see Venn. Symbolic Logic, 2d ed., 1894."

Emendations

406.7–8 (now . . . Franklin)] S, C; *not present*
406.9 article] S, C; paper
406.14 Peirce] S, C; C. S. ~
406.23 Non-European] L, S, C; ~‸~
406.25 Non-Republican] S, C; ~‸~
407.21 The . . . is] S, C; We are
407.29–30 in . . . way] C; *not present* 429 • in an exceedingly interesting way S
408.5 modes] S, C; moods
408.9 σημείων] S, C; σημειῶν
408.9 σημειώσεων] C; σημειωσειων
408.18–19 I . . . publication.] S, C; *not present*
408.20 BALTIMORE, Dec. 12, 1882.] S; *not present* 429 • ~, December 1, ~. C

Rejected Substantive Variants

406.1 *Preface*] PREFACE. 429 • PREFACE S • Preface to Contributions to Logic by Members of the Johns Hopkins University. C. S. Peirce, Editor. (Little, Brown & Co., Boston, 1882) C
406.8 and of Dr.] ~ ~ Mr. S • and Mr. C
407.11–12 fire, i.e.] ~; that is S, C
407.21 Dr.] Mr. S, C *Also* 407.29
407.33 and of Dr.] and Mr. S, C
408.12 appear] appears S, C

64. *Probable Inference, 1883*

Copy-text is P 268b, the publication in *Studies in Logic* [S]. It is emended by MS 498a, Peirce's blue ink and graphite pencil inscriptions in a volume now in the Houghton Library [498a], which he inscribed as *"Copy No 1"* below his signature on a front endpaper; by MS 498b, his annotated copy formerly in the possession of Irwin C. Lieb [498b], some annotations of which correspond to the corrections noted in a 24 March 1883 letter-draft to T. S. Perry; and by MS 388, pre-copy-text fragments of material related to the present article [388].

Emendations

408.27 liver:] E; ~;
409.4 man:] E; ~;
409.19 *M:*] E; ~; *Also* 409.24
410.31 proportion] 498a, 498b; proposition
410.35 one-eighth] E; ~‸~
410.39 picquet] E; piquet
411.36–37 ‸*Essay . . . Understanding,*‸] E; "Essay . . . Understanding,"
414.5 *[formula]*.] E; ~‸
415.1 males:] E; ~;
416.8 III.] 498a; *not present* S • §3. INDUCTION. 498b
416.9 two] 498a, 498b; these ~
416.25 nine-tenths] E; ~‸~
417.21 V‸] E; ~. *Also* 419.1, 38
417.31 II‸] E; ~. *Also* 418n.13
418.8 IV‸] E; ~. *Also* 424.25, 426.5, 434.25
420.15 *e*'s] E; *e's*
420.30 for] E; fer
421.33 man:] E; ~.
422.13 innervation] E; inervation
422n.1 Make Our Ideas Clear] E; *no caps*
423.3 results] E; result
424n.1 ἀπαγωγὴ εἰς τὸν ἀδύνατον] E; *apagoge* S • ~ ἐις ~ ~ 498b
427.36 peculiarities‸] 388; ~,
428.6 roulette‸] 388; ~,
428.37 two-thirds] E; ~‸~
430.31 indispensible] 388; indispensable
431.5 centre‸] 388; ~,
432.24 comprizing] 388; comprising
434.17 *M*'s] E; *Ms*
435.20 70] E; 76
437.31 3rd] E; 3d
437.32 2nd] E; 2d *Also* 437.34, 38
441.22 Quételet] E; Quetelet
441.26 one-half] E; ~‸~ *Also* 441.28, 443.31
441n.1 ‸*Théorie Analytique des Probabilités,*‸] E; "Théorie Analitique des Probabilités,"
443n.1 ‸*Laws of Thought,*‸] E; "Laws of Thought."
443n.1 p. 370.] E; *not present* S • ~ ‸ ~‸ 498b

Line-End Hyphenation

410.28 countervailing
411.19 lottery-wheel
445.25 overwhelming
447.20 non-*M*'s

Rejected Substantive Variants

408.21 *Inference*‸] INFERENCE.|§1. PROBABLE DEDUCTION AND PROBABILITY, IN GENERAL. 498b
408.24 The . . . the] Take an example of the 498a
408.25 inference:—] ~, such as this:‸ 498a
409.1–2 say . . . following] as follows 498a
409.6 the . . . of] applying 498a

409.10–11 the . . . of] *not present* 498a
409.22 *Probable*] Quantitative 498a *Also* 413.19
409.24 is] ~ a random instance of 498a
409.30 of] ~ proportional 498a
411.7 perhaps] no doubt 498a
411.10 is . . . determined] may further be ~ 498a
411.12 but] while 498a
411.13 preference; and the] ~‸ The 498a
411.29 other] further 498a
413.14 II.] §2. STATISTICAL DEDUCTION. 498b
413.20 *M*'s, the] ~, objects drawn at random the the 498a
414.1 *M*'s,] ~‸ generally 498a
414.6 number‸] ~, *n* 498a
414.7 *n* =] to be 498a
416.7 1.4] 1.6 498a
417.20 IV.] §4. HYPOTHETIC INFERENCE. 498b
420.10 V.] §5. GENERAL CHARACTERS OF DEDUCTION, INDUCTION, AND HYPOTHESIS. 498b
423.24 VI.] §6. INDUCTION AND HYPOTHESIS, INDIRECT STATISTICAL INFERENCES|GENERAL RULE FOR THEIR VALIDITY. 498b
427.15 VII.] §7. FIRST SPECIAL RULE FOR SYNTHETIC INFERENCE. SAMPLING MUST BE FAIR. ANALOGY. 498b
433.20 VIII.] §8. SECOND SPECIAL RULE FOR SYNTHETIC INFERENCE, THAT OF PREDESIGNATION. 498b
439.19 IX.] §9. UNIFORMITIES. 498b
441.10 X.] §10. CONSTITUTION OF THE UNIVERSE. 498b
446.13 XI.] §11. FURTHER PROBLEMS. 498b

65. *Universe of Marks, 1883*

Copy-text is P 268c, the publication in *Studies in Logic.* There are no emendations, no manuscript drafts, and no corrections in Peirce's copies of *Studies* or in his letters.

Line-End Hyphenation

452.27 non-existence

66. *Logic of Relatives, 1883*

Copy-text is P 268d, the publication in *Studies in Logic* [S]. It is emended by MS 498a, Peirce's annotated copy of *Studies* in the Houghton Library [498a]; by L 344, Peirce's 24 March 1883 letter-draft to T. S. Perry [344]; and by L 339, Peirce's 30 June 1903 letter to his brother James Mills Peirce [339]. See the headnote to item 64 for further relevant sources of emendation. The final paragraph is an addendum, which is represented in reduced type as in the original publication.

Textual Notes

463.22 In L 339, Peirce corrected the incorrectly printed formula by crossing out $(l \dagger 0) \infty \prec l \dagger 0 \infty \prec$; in MSS 498a and 498b, his corrections are incomplete because he failed to delete the final sign of the copula.
466.30 liederliche] Peirce misread his copy when, in his letter to his brother, he gave the following direction: "For leiderliche read liederliche."

Emendations

454.15 coëfficient] E; coefficient
455.31 *[formula].*] E; ~‸
456.30 $\bar{b}$.] E; ~‸
459.18 n] E; u *Also* 462.2, 8, 9
463.3 ‸0] 498a, 498b, 339; (~
*463.22 *[formula].*] 339; $l \dagger 0 \prec (l \dagger 0) \infty \prec l \dagger 0 \infty \prec l \dagger \mathfrak{n} \prec l$
464.3 $0 \dagger \breve{g}$] 498a, 498b, 344, 339; $0 f \breve{g}$

464.19 coëfficients] E; coefficients *Also* 464.21, 465.22
464.19 454] E; 187
464n.1 pages 454–55] E; page 188
465.4 of ——."] E; ~ ——.‸
465.27 latter] 498a, 498b, 344, 339; unaccused
465.27–28 of all not] 498a, 498b, 344, 339; of all to whom he is not
465.28 to him] 498a, 498b, 344, 339; *not present*
466.25 *[formula].*] E; ~‸
466.27 "Algebra of Logic,"] E; ‸*Algebra of Logic,*‸
*466.30 liederliche] 498a, 498b, 339; lederliche

Line-End Hyphenation

455.24 *non-existence*
466.17 non-benefactors

67. *Communication from Mr. Peirce, 1883*

Copy-text is P 245, the publication in the *Johns Hopkins University Circulars,* which has "C. S. PEIRCE" at the end and which follows J. J. Sylvester's "Note" of 30 March 1883. The rejected substantive variants are Peirce's ink inscriptions in the galley proofs, which also contain Sylvester's comments. The galleys are deposited in the Johns Hopkins Libraries. When Peirce deleted a passage in the galleys, it is noted in the Rejected Substantive Variants as *not present.*

In the February 1883 Circulars, Sylvester had published an "Erratum" to his article, "A Word on Nonions," which Peirce had viewed as a personal attack on his intellectual integrity regarding the application of logic to algebra. On 18 February, Peirce sent a letter to President Gilman of the Johns Hopkins in which he addressed Sylvester's article and erratum: "I am quite clear that he has cast an imputation upon my honor, but I wish to make my reply as simple explanatory and good-natured as possible. Will you not give me your friendly judgment of what I have written & if it meets your approval insert it in the next circular?" And on 26 March he informed Gilman that he had

> been a number of times to try to see [him] . . . in order to submit . . . my piece in reply to Professor Sylvester. I do not think his attack on me ought to have gone forth with the approbation of the board of trustees. But since it *did,* the University is committed to the principle & must publish my reply. If that is refused, I shall be forced to go into print on my own account. I now send you my reply, which I cannot modify in any essential particular, unless it be to add *facsimiles* of letters & other documents. But I prefer to reserve that for a rejoinder.

Gilman noted on the back of that page of Peirce's letter: "wrote ack^{g} this & saying that I would shew the MS. to Prof. Sylvester." And on 27 March, Peirce wrote Gilman that he recognized that the latter would "follow . . . [his] own judgment in regard to showing my manuscript to Professor Sylvester. It is not quite fair, because his was not shown to me. Still, if it were possible to avoid further dispute, I should heartily consent. I do not think, however, that it is possible; because he is blinded by arrogance & I shall not go without having the imputations that have been made upon my conduct, completely refuted." Gilman noted on Peirce's letter that he had informed Sylvester of Peirce's communication and wished to discuss it with the former. On 29 March Peirce wrote to Gilman:

> I cannot consent to my statement being modified unless Professor Sylvester will say that my conduct was correct in regard to the proof-sheets. I have no objection to

this being qualified by his saying that it was correct *if the oral message was delivered to me as I say it was;* but clearly if such a qualification is to be inserted, everything depends upon how it is put.

On the other hand, I do not care how strongly he says that he did not authorize the reference to me.

I would suggest that my reply be modified by making it say, near the beginning, not 'the purpose of Professor Sylvesters *Erratum* is' but 'the object . . . has seemed to some persons to be.' Instead of saying 'My answer is' put 'But it cannot be that Professor Sylvester intended to find fault with my conduct, for an answer to such a charge has been already furnished by Professor Sylvester himself who says.' Then quote the sentence from page 203. Then about the oral message & my understanding that he would look at his proof-sheets again. Then my narrative, omitting what took place between him & me. Then this *"Note by Professor Sylvester.* If Mr. Peirce understood the message in regard to the proof-sheet as he says, he was undoubtedly justified in making the insertion he did. But I never authorized such a reference to him, and should have preferred to have it omitted."

That would suit me well enough—

Sylvester apparently saw the proofs of Peirce's "Communication" just before mid-April, for on 14 April 1883 he wrote, probably to Gilman, in whose papers this note survives:

I return the two proofs which I found on my table last night. There was no accompanying note. The corrections of my own proof [of his "Note"] are for the press. The running commentary on the other prompted by natural indignation can be made the basis (in a modified form and reduced within the bounds of social convenance) of a counter "personal explanation."

Gilman sought the advice of G. W. Brown, Johns Hopkins Trustee, on the matter, and was persuaded to publish Sylvester's and Peirce's pieces. Sylvester then replied to Gilman on 18 April:

I am astonished at the proposition contained in your note of the 18$^{\text{th}}$ that it should be proposed to allow Mr. Peirce's virulent and disingenuous statements to be made in the circular without giving me an opportunity of replying thereto. If that course is adopted, self-respect will render it imperative for me to withdraw from all future participation in the circulars. . . . My few lines proposed to be inserted in the circular bear no reference to Mr. Peirce's most improper and unprovoked attack and insinuations.

Gilman recorded in his diary on 20 April 1883 that he had had almost daily interviews with Peirce regarding the difficulties Peirce was undergoing with Sylvester. Peirce quite likely saw Sylvester's mid-April comments on the galley proofs, and perhaps altered them accordingly.

Textual Note

471.20 p. 53] This is Peirce's citation for the page number in the newly paginated offprint. In the original publication in *Memoirs of the American Academy of Arts and Sciences,* it was page 369.

Emendations

467.4–5 *"Erratum"*] E; $_\wedge\sim_\wedge$ *Also* 467.7
467.7 on] E; upon
468.5 proof-sheet] E; $\sim_\wedge\sim$
468.28 $w_\wedge$] E $\sim$, *Also* 469.11
468.28 $k_\wedge$] E; $\sim$,
469.11 $l_\wedge$] E; $\sim$,
‡470.7 algebra of] E; *not present*
471.22 coëfficients] E; coefficients

Rejected Substantive Variants

467.1 *A . . . Peirce.*] A PERSONAL EXPLANATION.|BY C. S. PEIRCE
467.5 Circulars$_\wedge$] ~, published with the approbation of the Board of Trustees,
467.16–17 "knows . . . whatever"] $_\wedge$~ . . . ~$_\wedge$
468.3 relating . . . work] *not present*
468.7–12 At . . . it?] *not present*
469.35 etc. . . . etc.] *not present*
470.1(3) u_a] u^a
471.7 whichever] whoever
472.10 narrative.] ~. I prefer not to go into what may have passed between Professor Sylvester and me concerning Nonions.
472.11–14 Professor . . . much.] him. When he published this, he had certain knowledge that I had reached the same result many years before in connection with my theory of the logic of relatives; and this being so, I think that if he was unwilling to insert into his paper giving the group any allusion to what I had not printed, at least it would be reasonable for me to expect that he would consult my memoir on the logic of relatives, in order to be able to make a suitable reference to me.* I certainly did not dream, when his proof-sheet was submitted to me, that he would find it too much to say that the group in question could be derived from my algebra, or that if he did I should first learn of his objection from a public protest.
*Professor Sylvester's statement however, is that he not only has not read that memoir, but he has no knowledge of what it contains. Few of my acquaintance escape so easily.

68. *Construction of Reversible Pendulum, 1883*

Copy-text is MS 450, a reassembled manuscript consisting of two medium-weight leaves of white paper lined only on one side and measuring $7\frac{3}{4} \times 9\frac{5}{8}$ in., with an "E H OWEN EXTRA SUPERFINE" watermark, and one heavy and stiff leaf of white paper lined only on one side and measuring 8×10 in., with a "MASSASOIT COMPANY" watermark. The three leaves are inscribed in blue-black India ink. Four additional fragmentary pre-copy-text pages also survive. There are relatively few alterations in the entire manuscript.

Emendations

473.17 *et seq.* $I_1 D_t^2 \varphi_1$] E; *rom. All lowercase variables rom.*
475.14 $I_1 D_t^2 \varphi_1$] E; $\mathrm{I_1 D_t \varphi_1}$
475.20 displaced] E; displacd

69. *Sixty Lectures on Logic, 1883*

The copy-text, MS 459, consists of twenty-five leaves of medium-weight, ruled, white paper measuring $7\frac{13}{16} \times 9\frac{7}{8}$ in., with an "E H OWEN EXTRA SUPERFINE" watermark. They are inscribed in blue-black ink. Eighteen pre-copy-text pages of some of these syllabus entries also survive. There are many alterations.

Emendations

477.15 *et seq.* $\Pi_i u_i$] E; *rom. All lowercase variables rom. except i* 483.33, 484.2; *j* 484.1; *s* 484.15, 485.19; *g* 484.27; *c* 485.19
478.9 0.] E; ~$_\wedge$
483.27 $\Pi_i \Sigma_j$] E; $\mathrm{\Pi_i \Sigma_i}$
483.31 v_j] E; $\mathrm{v_k}$
484.9 etc.] E; ~$_\wedge$
485.1 quantity$_\wedge$] E; ~.
486.15 fictitious] E; fictititious
488.13 reformed,$_\wedge$] E; ~„
489.6 *Studies in Logic*] E; 'Studies in Logic'

Line-End Hyphenation

477.32 Anti-spurious

70. *Lecture on Propositions, 1883*

The copy-text, MS 462, consists of ten leaves of medium-weight, laid, unruled, white paper measuring $5\frac{1}{16} \times 7\frac{7}{8}$ in. They are numbered in the upper right corner and inscribed in blue ink; the red adhesive on their top edges indicates that they were torn from a tablet. There are relatively few alterations.

Emendations

490.4 *Studies in Logic*] E; *rom.*
490.4 p.] E; $\sim_\wedge$
490.8(1) *et seq.* *x*] E; *rom. All lowercase variables rom. except x* 490.26; 491.1, 3(1), 7, 14(2); 492.15(1), 18, 19, 20; *i* 490.26; 491.3, 13; 492.12, 13, 14(2), 15, 17, 18, 20, 22, 23
491.5 $\Sigma_i \bar{x}_i$] E; $\Sigma_\mathrm{i} \bar{\mathrm{x}}$
491.14 non-*x*'s.] E; $\sim$-x's$_\wedge$ *Also* 491.15
491.14 *x*'s.] E; x's$_\wedge$
491.16 non-*x*'s] E; $\sim$-xs
491.17 *i*'s] E; *is*
491.19 abbreviation] E; abbrievation
492.12 exists.] E; $\sim_\wedge$
492.14 non-*x*] E; $\sim_\wedge$x
492.15 non-*x*'s] E; $\sim_\wedge$x's *Also* 492.24
492.17 non-*x*.] E; $\sim$-x$_\wedge$
492.19 non-*x*'s] E; $\sim$-*xs*
492.21 non-*x*'s.] E; $\sim_\wedge$x's$_\wedge$ *Also* 492.24, 26
492.22 *x*'s.] E; x's$_\wedge$
492.23 *i*'s] E; *is*
492.24 11th] E; *not present*
492.25 12th] E; *not present*
492.25 *i*'s] E; is *Also* 492.26
492.25 *x*'s.] E; xs$_\wedge$
492.26 13th] E; *not present*
492.26 non-*x*'s] E; $\sim_\wedge$xs
492.27 14th] E; *not present*
492.27 *i*'s.] E; i's$_\wedge$

71. *Lecture on Types of Propositions, 1883*

The copy-text, MS 463, is a fragment consisting of twenty-seven numbered leaves of coated, unruled, white paper measuring $5\frac{1}{8} \times 7\frac{15}{16}$ in.; the red adhesive on their top edges indicates that they were torn from a tablet. They are inscribed in blue ink, although the dots in the quadrants of the circle are colored in red and blue pencil; it is uncertain whether these colorings are Peirce's. Item 72 also has an additional, pre-copy-text leaf with orange-red pencil inscriptions in Peirce's hand. MS 463 has six additional pre-copy-text leaves, and there are many alterations.

Textual Notes

493.20 Peirce originally inscribed "false,—" but changed the comma to a period. Since he frequently used the comma-dash and colon-dash, occasionally the semicolon-dash, but rarely the period-dash, he likely would have deleted the dash had he been more attentive to his incomplete alteration.

498.3 the . . . b_i)] Rather than rewriting the same formula, which appears only four lines above this point, Peirce drew an arrow pointing from the blank space toward the already inscribed formula. This arrow is interpreted as his note to himself to repeat the appropriate formula, which consequently is repeated here.

Emendations

493.7(1) *et seq.* *x*] E; *rom. All lowercase variables rom. except i* 497.19
*493.19–20 continuity—false.$_\wedge$] E; $\sim_\wedge\sim$.—

493.21 *Studies in Logic,*] E; *rom.*
493.21 p. 61,] E; ~‸ ~.
493.22 "On . . . Universe."] E; ‸~ . . . ~.‸
494.2 black.] E; ~‸ *Also* 497.29, 31
494.3 non-black.] E; ~‸
494.26 non-black] E; ~‸~ *Also* 494.31, 32; 495.20, 24
494.35 non-blackness] E; ~‸~
495.7 non-black] E; ~‸Black
495.9 ones.] E; ~‸
495.12 others.] E; ~‸
495.20 thing] E; *not present*
496.7 one] E; *reinstate*
496.8 breathe] E; breath
496.13 white.] E; ~‸
497.13 *American Journal of Mathematics* Vol.] E; Am. mathematical Journal ~‸
497.14 "On . . . Logic"] E; '~ . . . ~'
497.23–24 applicable.] E; ~‸
497.27 non-existent] E; ~‸~ *Also* 497.30
497.30 crow's] E; crows
497.30 or] E; of
*498.3 $b_c = \Pi_i\ (\bar{c}_i + b_i)$] E; *not rewritten*
498.3 put] E; but
498.5 (or] E; ‸~
498.5 c_4,] E; ~‸
498.6 c's)] E; ~‸
498.16 $\times \bar{b}_i$] E; + ~
498.25 non-lover] E; ~‸~
499.10 (c_a)] E; $(\mathrm{a_c})$
499.11(2) *c*] E; a
499.12 *a*] E; c
499.32(1) etc.] E; ~‸ *Also* 499.33; 500.2, 3, 6, 13
499.33 $\times (p_{11}$] E; ‸ $(\mathrm{p}_{11}$
500.3 $\times (l_{12}p_{21}$] E; ‸ $(\mathrm{l}_{12}\mathrm{p}_{21}$
500.4 $\times (l_{13}p_{31}$] E; ‸ $(\mathrm{l}_{13}\mathrm{p}_{31}$
500.18 class.] E; ~‸
500.23 black‸things] E; ~-~

Line-End Hyphenation

493.26 non-black *Also* 493.27

72. *Lecture on Logic of Relatives, 1883*

The copy-text, MS 464, is a reassembled fragment consisting of fifteen leaves of slightly heavy and stiff, plain white paper measuring $5\frac{1}{8} \times 7\frac{15}{16}$ in.; the red adhesive on the top edge of some of the leaves indicates that they were torn from a tablet. All fifteen leaves are inscribed in blue ink, and the verso of another leaf is inscribed in orange-red pencil. Nine additional leaves of pre-copy-text material also survive. There are extensive alterations.

Emendations

501.5 *et seq.* $\Pi_i\Pi_j$] E; *rom. All lowercase variables rom. except i* 504.24; *k* 504.24
501.9 character.] E; ~‸
501.13 possesses] E; possess
501.18(2) is] E; *not present*
502.1 *syllogism*‸] E; ~.
502.22 Π's] E; Πs
502.22 Σ's] E; Σs
502.25 etc.] E; ~‸
503.4 century,] E; ~.
503.10 celebration.] E; ~‸
504.11 $\Sigma_i u_i v_i$.] E; $\Sigma_i \mathrm{u_i v_i}$‸
504.26 thing;] E; ~‸
504.27 0.] E; ~‸
504.27 vanishes] E; vanishs
505.14 formulae‸] E; ~,
505.28 454] E; 196
506.2 n.] E; ~‸
506.5(3) is] E; *not present*
506.8 mark.] E; ~‸

73. *Lecture on Logic, 1883*

The copy-text, MS 491, consists of nine numbered leaves of medium-weight, white paper, lined on one side only and measuring $7\frac{13}{16} \times 9\frac{7}{8}$ in., with an "E H OWEN EXTRA SUPERFINE" watermark. The manuscript is inscribed in blue ink, and there are many alterations.

Textual Note

507n.1 Peirce originally inscribed this note in the left margin beside the lines reading *"Man* is . . . two premises" (507.19–23).

Emendations

507.6 *Summulae*] E; *rom.*
507.9 Hispanus] E; Hipanus
*507n.1 *suppositiones.*] E; ~‸
508.24 don't] E; dont
508.26(1) Would] E; Wou
508.26(1) many?] E; ~.
508.27 twenty-four] E; ~‸~
508.27 twenty-five] E; ~‸~
509.7 it] E; It
509.15 *Discussions*] E; *rom.*
509.16 *Commentary*] E; *rom.*
509.36 an] E; a
509.40(2) as] E; a

74. *Division in Arithmetic, 1883*

Copy-text is P 266, the publication in *Science.* MS 465 is a pre-copy-text version, which Peirce titled "Note on a ["Method of Arithmetical Division"—*deleted and*] New Rule for Division in Arithmetic" [*inscribed above*]. It is signed "C. S. PEIRCE" at the end.

75. *Flexure of Pendulum Supports, 1883*

Copy-text is P 253, the publication in the 1881 *Coast Survey Report.* On 20 February 1880, Peirce wrote Superintendent Patterson from Baltimore that "a paper on the flexure of the pendulum support will soon be ready." By 2 June he was able to report to Patterson that his work was "in active progress, but I absolutely need the results of Mr. Farquhar's swings on different supports at York. I hope it will come soon." In January 1882 he was about to prepare his "Memoirs on the effect of flexure of the pendulum support . . . for publication." Sometime between 6 and 19 July, he submitted his "Flexure report" to Superintendent Hilgard, and by 1 December he reported that he was "now correcting the proofs," which he completed sometime in early 1883. See the headnote to item 15 for the separate publication of this and three other papers.

Emendations

515.6 "Tides"] E; ‸~‸
515.6–7 *Encyclopædia Britannica*] E; *rom. Also* 516.4–5
515.12 centre] E; center *Also* 516n.1, 518.9, 521.4
515.25 seconds‸] E; seconds'
516.1 Basevi] E; Bassevi
516.7 Cellérier] E; Cellerier *Also* 516n.2
516.11 millimetres] E; millimeters
517.17 knife-edge] E; ~‸~
518.7 centimetres] E; centimeters
520.1–2 *Coast Survey Report*] E; *rom.*
520.3 diminishing] E; diminish|ing
520.14 modulus] E; modules
520.37 seconds] E; second's
521.18 India-] E; india-
521.38 metre] E; meter
523.25 micrometer‸scale] E; ~-~
524.37 Means. . . .] E; *not present*
525.7 metres] E; meters
525.22 60^{cm}‸] E; ~ cm.
526.12 *Am. Jour. Sci.*] E; *rom.*
526.31 *[formula]*.] E; ~‸
526.34 $\frac{1}{\gamma}\cdot$] E; ~‸
527.3 *[formula]*.] E; ~‸
528.6(2) D_t^2] E; D_t
528.21 one-tenth] E; ~‸~ *Also* 528.22
528.21 one-thousandth] E; ~‸~ *Also* 528.23

Line-End Hyphenation

520.16 semi-elastic
521.17 blotting-paper
521.35 screw-feet
521.37 binding-screws

76. *Ellipticity of the Earth, 1883*

Copy-text is P 254, the publication in the 1881 *Coast Survey Report.* Peirce worked on the proofs for this paper in the latter part of 1882, and by 1 November of the following year he reported that his office work comprised "Five papers put through the press," which included the present paper. See the headnote to item 15 for the separate publication of this and three other papers.

Textual Notes

532.29 sea bed] The hyphen is deleted to conform to the first occurrence without a hyphen (531.4) and to the OED.
534.1 "Fathoms" is reproduced here (as Peirce would often do when a table continued onto the next page) for the reader's convenience.

Emendations

529.8–9 coëfficients] E; coefficients *Also* 529.12
529.27 coëfficient] E; coefficient *Also* 530.10, 12, 19
530.34–35 ‸*Comptes Rendus,*‸] E; "Comptes Rendus,"
531.2 centre] E; center *Also* 531.14, 18; 532.2, 7
531.20 *[formula].*] E; ∼‸
531.27 *[formula].*] E; ∼‸
*532.29 sea‸bed] E; ∼-∼
533.graph *Sea-level*] E; ∼‸∼
533.13 *Thalassa*] E; *rom.*
534.4 Guahan] E; Guaan
534.17 *[formula].*] E; ∼‸
534.21 *Geodesy*] E; *rom.*

Line-End Hyphenation

529.14 large-sized

77. *Coincidence of Vibration, 1883*

Copy-text is P 255, the publication in the 1881 *Coast Survey Report,* which is without the "A," identifying the wall, and "G," pointing to the eye-piece. For further information on the preparation and separate publication of this paper, see the headnotes to items 76 and 15.

Textual Note

538.18–19 The formula, which in the *Report* was broken before the plus sign, is broken differently here to fit the space on the page. To indicate the continuity we have inserted the multiplication dots.

Emendations

535.diagrams A] E; *not present*
G] E; *not present*
535.11 millimetres] E; millimeters *Also* 540.18
536.6 D, D] E; ∼‸ ∼
536.23 millimetre] E; millimeter
537.1 *[formula].*] E; ∼‸
537.16 $\frac{b_1 + b_2}{2} t$] E; $\frac{b_1 + b_2}{2}$ *Also* 537.17
537.17 0.] E; ∼‸

537.19 *[formula].*] E; ∼‸
538.6 *[formula].*] E; ∼‸
*538.18 · sin] E; ‸∼ *Also* 538.19
539.7 *[formula].*] E; ∼‸
539.9 *[formula].*] E; ∼‸
540.1(denom.) (1) $a_1 b_1$] E; ab_1
540.1(denom.) (3) $a_1 b_1$] E; $a_i b_1$
540.2(2) $(a_1 - a_2)$] E; ‸∼)
540.2 *[formula].*] E; ∼‸
540.8 $\frac{1}{1000}$th] E; $\frac{1}{1000}$

Line-End Hyphenation

536.7 knife-edges

78. *Additional Note on Coincidences, 1883*

The copy-text, MS 445, is a fragment consisting of five heavy, stiff leaves of white paper, ruled on one side only and measuring $7\frac{3}{4} \times 9\frac{7}{8}$ in., with a "MASSASSOIT COMPANY" watermark. A separate leaf, representing a discrete version of the first two paragraphs of the note, also survives. All six leaves are inscribed in blue ink, and there are numerous alterations.

Emendations

541.15 Fig.] E; ∼‸ *Also* 542.19
542.19 2‸] E; ∼.
543.8 clock's] E; clocks

79. *Design and Chance, 1883–84*

The copy-text, MS 494, is a fragment consisting of thirty-three reassembled, unnumbered leaves of white paper measuring $7\frac{13}{16} \times 9\frac{7}{8}$ in., with an "IMPROVED" watermark. They are inscribed in blue ink, but as Peirce had difficulty with his pen, the inscription is light and dark and has many retracings and frequent marginal ink spots and pen scratches. Moreover, there are many alterations. Inscriptions in orange pencil on the first leaf are in a hand other than Peirce's.

Textual Notes

548.29–30 general *[. . .]* ¶It]The last word on the leaf is "general." At least one leaf seems to be missing. Preceding the next paragraph, at the top of the next leaf, is the editorially deleted "to be explained."

549.24 This paragraph is followed by the three-word paragraph "A million players," which is the fourth line of leaf 19; the rest of the leaf is blank. The phrase is interpreted as Peirce's reminder to himself that this is the example he wishes to cover later. It is removed from the text because the detailed example appears two paragraphs later.

552.19 Although the ellipsis in italic brackets indicates that a page or more of manuscript may be missing, it is possible that only the word "chance" is missing.

Emendations

544.14 Crookes] E; Crooks
544.17–18 withhold] E; withold
546.18 non-Euclidean] E; ∼‸∼
546.28 smallest] E; smalles
546.29 can be] E; be
546.35 that] E; the
546.39 would‸] E; ∼,
547.22–23 unreasonable] E; unreanable
547.30 governs] E; govens
547.40 Clifford's] E; Cifford's
548.4 proceeded] E; proceded
*548.29–30 general *[. . .]* ¶It] E; general|to be explained.|¶It
548.32 agencies] E; acencies
549.17 self-contradictory] E; ∼‸∼
*549.24–25 is not. ¶Now] E; is not. ¶A million players. ¶Now

549.26 are] E; a
549.33 million] E; millin
549.40 that.] E; ~‸
550.1 slightly] E; slghtly
550.12 another.] E; ~‸
550.15 solutions to] E; solutions
550.19 will be] E; will
551.25 example—] E; ~‸
551.26 element,‸] E; ~,—
551.29 find‸] E; ~—
551.37 chance.] E; ~‸
551.40 further] E; futher
553.9 have shown] E; shown
553.15 some of] E; some
553.17 others] E; other
553.25 destroyed.] E; ~‸
553.37 one] E; on

80. *Teaching of Mathematics, 1884*

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Emendations

555.5 improved] E; impoved
555.8 Wolffian] E; Wolfian
555.15 element] E; elelement
555.16 others.] E; ~‸
555.23 feeling,] E; ~‸
555.24 learning] E; leaning
556.2 exercise] E; exercis
556.4 mathematician] E; math|ematician
556.5 second] E; secon
556.28 relationship‸,] E; ~.,
556.31 if] E; ~ if
557.1 general] E; heneral
557.4 oblivious] E; obliviouus
557.11 for at] E; fortat
557.14 indeed.] E; ~‸
557.15 to be] E; to
557.24 understands] E; inderstands
557.28 fault‸,] E; ~.,
557.29 really] E; realy
557.31 understand] E; *not present*
557.31 999,999] E; 999,99
557.37 don't] E; dont
557.40 predilection] E; predeliction
558.5 that] E; thaF

Line-End Hyphenation in the Edition Text

The following lists those compound words hyphenated at the ends of lines in the critical text of the present edition that, in being quoted or transcribed from the text, must retain their hyphens. All other possible compounds hyphenated at the ends of lines should be transcribed as single words.

4.4 Wave-Lengths
16.14 twenty-fifth
50.5 diffraction-plate
60.12 ten-thousandths
72.5 non-quantitative
81.18 pendulum-expeditions
86.25 knife-edge
88.10 chronometer-breaks
111.18 knife-edge
120.23 water-tight
125.7 knife-edge
130.37 spectacle-lens
135.17 cross-section
135.22 pendulum-metre
152.6 self-glorification
178.2 minor-particular
203.12 sixty-four
270.30 diffraction-plates
300.24 semi-limited
301.16 semi-infinite
312.28 idem-factor
313.11 self-consistent
343.25 alio-relative
343.29 self-relative
344.4 non-relative
357.30 platin-iridium
358.39 sea-level
372.21 knife-edge
381.5 lecture-room
387.26 four-fold
424.27 well-drawn
440.32 non-uniformity
443.18 non-occurrences
457.3 *alio-relatives*
467.30 proof-sheet
471.11 square-roots
471.12 cube-roots
473.6 knife-edge
522.4 knife-edge
532.32 three-fourths
545.3 weak-minded
546.30 non-Euclidean
552.16 self-consciousness

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