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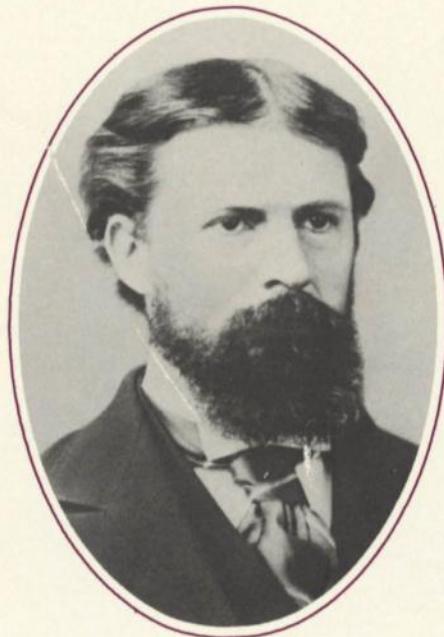
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# Writings of Charles S. Peirce

A CHRONOLOGICAL EDITION

*Volume 3*  
1872-1878

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**Volume 3**



An official Coast Survey photograph, ca. 1875

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*Volume 3*  
1872–1878

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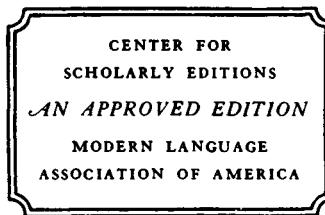
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*Indiana University Press Bloomington*

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## Preface

Editions differ in what they select and how they arrange and edit their texts. Our selecting, arranging, and editing 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, experimental psychology, 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, and in October 1973 some twenty-five Peirce scholars gathered at "The Arisbe Conference" in Milford, Pennsylvania, to discuss the relative merits of several alternative plans for such an edition. The first result of that discussion was that, under the auspices of the Texas Tech University Institute for Studies in Pragmatism, a small group of scholars spent the summer of 1974 checking an electroprint copy of the Peirce Papers against the originals in the Houghton Library of Harvard University and recording everything evident in the originals but not in the copies, such as watermarks, size and quality of paper, and faint pencil notations, with particular attention to whatever might assist in dating the large number of undated manuscripts. Early in 1975, Indiana University assumed responsibility for the preparation of a new edition. A Center for American Studies was established at Indiana University in Indianapolis, and the Peirce Edition Project was set up under its auspices. Two xerox copies of the electroprint copy were acquired from Texas Tech University—one to remain as arranged in Richard S. Robin's *Annotated Catalogue of*

*the Charles S. Peirce Papers*, the other to be gradually rearranged and renumbered in chronological order. Supporting grants from the National Endowment for the Humanities and the National Science Foundation began in July 1976, and the Project now had a director and a full-time staff of three. A Board of Advisors and a group of Contributing Editors were appointed and, after a meeting with the former in November 1977, general policies and procedures were adopted. Since 1984, the Project has had a full-time staff of six.

When work toward a new edition began in 1975, the only edition of Peirce's writings in more than one volume was the eight-volume *Collected Papers* (1931–1935, 1958). But in 1976 there appeared the four volumes of *The New Elements of Mathematics*. By that time the first part of Peirce's *Contributions to THE NATION* had been published, and parts 2 and 3 followed in 1978 and 1979. And in 1977 there appeared the *Complete Published Works, Including Selected Secondary Materials*, a 149-microfiche edition accompanied by a printed *Bibliography and Index*. These are all valuable editions, but none conveys a comprehensive sense of Peirce's entire work. Peirce's known writings, published and unpublished, would fill over a hundred volumes if the several thousand manuscript pages of discarded computations and scratch-sheet calculations were included. But any edition in fewer than sixty-five volumes might fairly be called "Selected Writings."

The present edition will consist of twenty volumes. It will include every philosophical and logical article that Peirce published during his lifetime, and those of his scientific and mathematical articles that shed most light on the development of his thought and that remind us of the immediate scientific and mathematical background of the work he was doing in philosophy. 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. Not less than a third, and if possible a half, of the writings included will be from so far 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 letters that are relevant to Peirce's work during the period. (Two volumes of correspondence are planned as supplements to the edition proper.) Except for some long technical-scientific papers, we publish no excerpts in the main text.

Recently a growing number of readers of Peirce have come to him from semiotics, the theory of signs, and they often 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 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 to be entitled "A System of Logic, considered as Semeiotic."

Our edition will facilitate the tracing of other developments of Peirce's thought as well, 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? Or 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—that is the primary goal of our edition.

Each volume contains several distinct sections. The largest and most important, the text of Peirce's writings, is preceded by a brief chronology and historical introduction and followed by editorial notes. The latter are frequently preceded by an appendix consisting of some fragmentary text by Peirce, or text by someone else, that sheds important light on writings in the main section. The editorial

notes are followed by a bibliography of Peirce's references and by a chronological list of every paper that he is known to have written, whether published or not, during the period covered by the volume. The historical introduction and the chronological list thus frame the writings that appear between them, and they provide a comprehensive sense of Peirce's work in mathematics, the sciences, and philosophy. Textual apparatus and index make up the final sections of each volume. (A comprehensive index and bibliography is planned for a separate later volume.)

The writings included in our edition have been prepared according to the standards of the Center for Scholarly Editions, and they appear in "clear" text: that is, excepting a few editorial symbols that represent physical problems in the manuscripts, everything in the main section is Peirce's own. In some instances, we have supplied titles. Each title is followed by a source note or identifying number—published items are identified by P number and the bibliographic information given in the *Bibliography and Index*; unpublished items by MS number and the date of composition. (Further information concerning manuscript or publication appears in the first editorial note for each item.) MS numbers refer 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; the bulk of that work has been completed. If further papers turn up too late to appear in their chronological places, they may be included in later supplements.

Finally, restraint and accuracy have been the guiding principles in our editing, and the published text represents what Peirce actually wrote, not what we think he should have written. We correct typographical errors, but retain his 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. All editorial changes are listed in the Emendations, and difficult emendations are explained in the Textual Notes. These notes and emendations, like the Editorial Notes, are keyed to page and line numbers. As further aids to the reader of Peirce's text, we have adopted four sets of symbols.

Supplied titles appear in italic brackets; ellipsis points in italic brackets indicate the loss of at least one full manuscript page; words or parts of words in italic brackets represent editorial reconstructions of damaged manuscript portions; and sets of double slashes signal the beginning and end of Peirce's unresolved alternative readings, with the single slash dividing the two alternatives. Also, the blank spaces appearing in three of the selections in the present volume represent the same spaces in manuscripts written by an amanuensis. For more detailed discussions of our editorial principles and practices, the reader should consult the Essay on Editorial Method, the introductions to the Editorial Notes and Chronological List, and the Explanation of Symbols.

#### NOTE TO THE SECOND PRINTING

The following nine corrections to the first printing have been incorporated in this second printing; the original readings are given in brackets. (1) vii.14, 21.9, 547.30 Enlarged [Enlarge], (2) ix.3 202 [203], (3) the entries for 1865–1867 and 1872 have been corrected at xix.14–25, (4) 24.22–23 “I . . . wholesome.” [‘I . . . wholesome’.], (5) 38.31 complex [incomplex], (6) three dots have been removed from the first and one dot each has been added to the first and second figure on p. 264, (7) 348.19 universel [universelle], (8) 490.2 page 481 [page 000], and (9) 620.49 *Review* [*Review*]. Emendations for (1), (4), (5), and (7), which represent Peirce’s errors, have been added on pages 581, 582, 583, and 603.

Indianapolis, May 1993

#### NOTE TO THE THIRD PRINTING

The following eight corrections to the second printing have been incorporated in this third printing; the original readings are here given in brackets. (1) 37.10 analyzed [analized], (2) 37.14 unanalyzed [unanalyzed], (3) three dots have been removed from the first and one dot each has been added to the first and second figure on p. 264, (4) 292.30 incorrectly [correctly], (5) 308.16 this much [thus much], (6) 329.33 in it is found [in it is a found], (7) 490.2 page 481 [page 000], and (8) 549.39 Observations [Observation]. Emendations for (1), (2), (4), and (5), which represent Peirce’s errors, have been added on pages 583 and 602.

Indianapolis, February 1999

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## Acknowledgments

We are indebted to Indiana University, the National Endowment for the Humanities, and the National Science Foundation, 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, where the Charles S. Peirce Papers are kept, for their cooperation; to the Interlibrary Loan department of Indiana University–Purdue University at Indianapolis for continued good service; to James A. Moore for invaluable past services as research associate in the Project; to Webb Dordick for his research assistance in the Harvard libraries; and to all those scholars who have given us expert help at particular points, especially to Arthur W. Burks, Glenn Clark, H. William Davenport, Allen G. Debus, Peter L. Heath, Kenneth L. Ketner, Thomas G. Manning, Alexander P. D. Mourelatos, Marc Rothenberg, Rosalie Vermette, and Shea Zellweger.

For permission to use duplicates of its annotated electroprint copy of the Harvard Peirce Papers, we are indebted to the Institute for Studies in Pragmaticism at Texas Tech University.

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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

## *1. The U.S. Coast Survey—and Darwin!*

There was no more intensively scientific seven-year period of Peirce's life than that of the present volume. He had no academic employment and gave no lectures at Harvard or at the Lowell Institute or elsewhere. As an Assistant in the Coast Survey his duties had so far been astronomical, and his concurrent assistantship in the Harvard College Observatory (1869–72) had been arranged with a view to those duties. But from late in 1872 onward his duties became increasingly geodetic.

The Coast Survey, with help drawn from the Hydrographic Office of the Navy, was—and was recognized as—the chief scientific agency of the United States federal government. It had been founded in 1811. Its first Superintendent was Ferdinand Rudolph Hassler; its second was Alexander Dallas Bache (1843–67); its third, Benjamin Peirce, Charles's father (1867–74); and its fourth, Carlile P. Patterson (1874–81).

Until the creation of the Bureau of Standards in 1901, the Office of Weights and Measures was part of the Washington Office of the Coast Survey, and the Assistant in Charge of the Washington Office was in charge of the Weights and Measures Office also. Throughout the period of the present volume that Assistant was Julius E. Hilgard. In the summer of 1872 there was a conference in Paris looking toward the creation of an international bureau of weights and measures there. Hilgard was given an extended leave of absence to attend that conference and for other purposes. From 15 April to 23 August, Charles Peirce was Acting Assistant in Charge of the Survey's Washington Office. The records of his “photometric researches” show that he was in Washington for several further months in that and succeeding years.

The present volume includes a number of chapter drafts of what we have called “Toward a Logic Book, 1872–73.” Concerning these, he wrote to his mother from Washington on 20 April 1872: “On clear nights I observe with the photometer; on cloudy nights I write my book on logic which the world has been so long & so anxiously expecting.” The book was never finished. Neither were the related “Illustrations of the Logic of Science” of 1877–78, which were advertised as a book in preparation for the International Scientific Series. The six papers he did finish and publish would not have made much more than half the intended book.

The Coast Survey was the chief scientific agency of the federal government not only in its own researches but also, especially during the superintendencies of Bache and Benjamin Peirce, in the help it gave to scientists not in its employ. The most widely known example of this began in December 1871 and continued into the fall of 1872. Peirce had named two of the Survey’s new vessels after Hassler and Bache and had assigned the *Bache* to the Atlantic coast and the *Hassler* to the Pacific. To get to our Pacific coast, the *Hassler* had to travel around South America. Dear to the heart of Benjamin Peirce’s friend, Louis Agassiz, was the Museum of Comparative Zoology at Harvard, and the *Hassler*’s voyage would be a great opportunity to add to its collections. But Agassiz was the chief American opponent of Darwin’s theory of evolution, and the voyage of the *Hassler* would take him, late in life, on the nearest approach to Darwin’s early-in-life five-year voyage of the *Beagle* that could be carried out in less than a year. He gladly accepted Superintendent Peirce’s invitation, as did his wife and ex-president Thomas Hill of Harvard.

The *Hassler* sailed on 4 December 1871. In *The American Naturalist* for January 1872 there appeared a letter from Agassiz to Superintendent Peirce “Concerning Deep-Sea Dredgings” dated 2 December 1871, in which he discussed the questions he hoped the voyage would help to answer, including Benjamin’s own theory of “continental drift.”

The Survey’s own scientific representative on the voyage was Agassiz’s former pupil, Assistant Louis François de Pourtales. The few books they took along were chiefly by Darwin, including of course his *Voyage of the Beagle*. Darwin was informed of the plan well in advance and sent his best wishes. The captain of the *Hassler* was Philip C. Johnson, and there were occasional comparisons between him and Captain FitzRoy of the *Beagle*. The deep-sea dredging equipment of the *Hassler* proved defective, and that was a great

disappointment; but dredging at moderate depths resulted in numerous and important additions to the collections of the Harvard Museum of Comparative Zoology. Johnson, Agassiz, and Pourtalès sent reports to Superintendent Peirce along the way. Mrs. Agassiz sent the *Atlantic Monthly* an article which appeared in its October 1872 issue, and she had two further articles in the January and March 1873 issues. The best day-by-day account was by Pourtalès, in Appendix 11 of the 1872 *Coast Survey Report*.

Under Agassiz's direction, his wife kept a journal of scientific and personal experience which was nearly ready for publication at the time of his death in 1873. She drew upon it for the account of the expedition which she included in her *Louis Agassiz: His Life and Correspondence*, published in 1885.

Chauncey Wright, the most vigorous defender of Darwin among the members of the Metaphysical Club in Cambridge, had had a long article on "The Genesis of Species" in the *North American Review* for July 1871, which had pleased Darwin so much that, with Wright's permission, he had it reprinted in pamphlet form in England. Wright visited Darwin at Down, 4–5 September 1872, four days after the *Hassler* reached San Francisco.

That was the end of the *Hassler*'s voyage. The captain and crew remained, and the passengers returned by the recently completed transcontinental railway. The Agassizs first lingered for a while, and he addressed gatherings of the California Academy of Sciences; but they were back in Cambridge by October. And by November, Chauncey Wright was back from his European travels and his visit with the Darwins.

It is quite likely, therefore, that the Metaphysical Club in Cambridge had already devoted some of its meetings to the Darwinian theory of evolution, and not altogether unlikely that Peirce at the meeting he addressed in November 1872 presented his pragmatism as the lesson in logic taught by Darwin's *Origin of Species*, as he certainly did in "The Fixation of Belief" in 1877 and in "Comment se fixe la croyance" in 1878.

## 2. *The Coast and Geodetic Survey*

The most decisive single step of Benjamin Peirce's superintendency of the Coast Survey had already been taken in March 1871, when he obtained an act of Congress authorizing a transcontinental

geodetic connection along the 39th parallel between the Atlantic and Pacific coastal surveys, along with a small initial appropriation. The fundamental problem of geodesy was that of the figure of the earth, and the chief instruments for its determination were gravity pendulums.

The first international scientific association was geodetic. Its founding conference had been at Berlin in 1864. In the French form of its name, it was called international from the beginning. In the German form, it was called at first middle-European, then European, and only in 1886 did it begin to be called international. Conferences were held every third year, but there was a "Permanent Commission" or standing executive committee that met annually. There was also a Special Committee on the Pendulum. By 1872 the association was settling on the Repsold-Bessel reversible pendulum as the best research instrument for its principal purpose.

On 30 November 1872 Superintendent Peirce wrote Assistant Peirce a letter of instructions beginning: "You are hereby directed to take charge of the Pendulum experiments of the Coast Survey, and to direct and inspect all parties engaged in such experiments. . . . In combination with the pendulum experiments you will investigate the law of the deviations of the plumb line and of the azimuths from the spheroidal theory of the earth's figure."

Since this assignment would involve spending most of his time away from Cambridge, Charles resigned his assistantship in the Harvard College Observatory on 2 December 1872.

Ten days later Charles wrote to A. & G. Repsold and Sons in Hamburg, Germany, ordering for the Coast Survey one of their reversible pendulums suitable for absolute determinations of gravity. The Repsolds replied that there would be a delay in filling the order because they had such an accumulation of still unfilled orders for other instruments to be used in observing the transits of Venus in 1874 and 1882. (The last previous transits had been in 1761 and 1769. The next would be in 2004 and 2012.) The pendulum was finally ready in the spring of 1875.

Meanwhile, in 1873 and 1874, Charles conducted parties making observations of gravity with nonreversible, invariable pendulums with conical bobs, on Hoosac Mountain and in the Hoosac Tunnel in northwestern Massachusetts, and at Northampton and Cambridge. During the same extended periods, and for the most part with the same aides, he continued the photometric researches which he had

already begun in Cambridge and in Washington earlier in 1872, using a Zöllner astrophotometer attached to a telescope inside a portable observatory, with an aide outside recording his observations. He had also, but under conditions too unfavorable, tried the experiment of "weighing the earth" at the top and bottom of the central shaft of the Hoosac Tunnel.

By 1875, the greater part of the photometric researches was completed, but he wanted still to make a more thorough study of earlier star catalogues. During his second Coast Survey assignment in Europe (1875–76), he examined medieval and renaissance manuscripts of Ptolemy's star catalogue in several libraries. He also made inquiries as to the methods used in the preparation of the most recent star catalogue, the *Durchmusterung* of Argelander and Schönfeld at the Bonn Observatory. Peirce's book, *Photometric Researches* (1878), included his own edition of Ptolemy's catalogue, as well as a long letter from Schönfeld concerning the methods of the *Durchmusterung*.

The chief purpose of this second sojourn, however, was to accept delivery from A. & G. Repsold and Sons in Hamburg of the reversible pendulum, and to make such determinations at so-called "initial stations" in Europe; namely, those at Berlin, Geneva, Paris, and Kew. In April 1875 at the new Cavendish Laboratory in Cambridge, England, he consulted Maxwell about the theory of the pendulum. At Hamburg in late May and early June, he took possession of the Repsold pendulum and made preliminary tests of it. He then conferred in Berlin with General Baeyer, founder and president of the Royal Prussian Geodetic Institute, who questioned the stability of the Repsold stand. Peirce went next to Geneva. By arrangement with Professor Plantamour, Director of the Observatory, he swung his new pendulum there, and detected and measured the flexure of the stand that General Baeyer had suspected.

In September 1875, the Permanent Commission of the International Geodetic Association met for ten days in Paris. On one of those days there was also a meeting of the Special Committee on the Pendulum, at which Peirce reported his Geneva findings. The Special Committee reported to the Permanent Commission. Peirce took part in the discussion of its report. He thus became the first invited American participant in the committee meetings of an international scientific association.

Later in 1875 and in 1876, Peirce swung his new pendulum for

extended periods in Paris, in Berlin, and at Kew; and after his return to the United States, at the Stevens Institute in Hoboken. The *Coast Survey Report* for the year 1876 (not published until 1879) contained 145 pages by Peirce on “Measurements of Gravity at Initial Stations in America and Europe,” on the second page of which he said: “The value of gravity-determinations depends upon their being bound together, each with all the others which have been made anywhere upon the earth. . . . Geodesy is the one science the successful prosecution of which absolutely depends upon international solidarity.”

(Making the Stevens Institute at Hoboken the “initial station” for the United States involved months of pendulum swinging there and, for that purpose as well as for readier access to Washington and other sites, Peirce took up residence in New York City. His wife Zina had her own commitments in Cambridge and Boston, and declined to accompany him. They were never reunited. By far the fullest and best account of her, and of Charles in his relations with her and with other members of her family, is Norma P. Atkinson’s 1983 doctoral dissertation, “An Examination of the Life and Thought of Zina Fay Peirce, an American Reformer and Feminist.”)

The next general conference of the International Geodetic Association was held at Stuttgart in late September and early October of 1877. By invitation, Peirce had sent well in advance a memoir in French on the effect of flexure of the Repsold stand on the oscillations of the reversible pendulum. This memoir, lithograph copies of which had been distributed in advance of the conference, and papers by Plantamour and his colleague Cellérier confirming Peirce’s findings were published as appendixes to the proceedings of the conference. Peirce attended the conference as accredited representative of the United States Coast Survey. That was the first formal representation of an American scientific agency in the sessions of an international scientific association. During the discussions, Hervé Faye, president of the Bureau of Longitudes in Paris, suggested that swaying of the stand could be prevented by swinging from the same stand two pendulums with equal amplitudes but in opposite phases. Peirce later made an analytic mechanical investigation of Faye’s proposal, concluding that it was as sound as it was brilliant. Copies of this investigation were distributed at the 1879 meeting of the Permanent Commission.

Peirce was active in still other fields that called for international cooperation. One of these was metrology. In 1872 when Peirce was

Acting Assistant in Charge of the Coast Survey's Washington Office, he had control of the United States Office of Weights and Measures, a department of the Coast Survey until 1901. The American Metrolological Society had been founded in 1873, and two years later, Peirce had become a member of its Committee on Units of Force and Energy. When he was elected to the National Academy of Sciences in April 1877, he was immediately made a member of its Committee on Weights, Measures, and Coinage.

Before his election to membership, he had received grants from the Bache Fund of the National Academy for the experiments reported in his "Note on the Sensation of Color," which was published in 1877 both in this country and in England, and which made him the first modern experimental psychologist on the American continent.

Of the thirty-four papers that Peirce presented to the National Academy of Sciences in the thirty-three years from November 1878 to November 1911, the first was geodetic: "On the Acceleration of Gravity at Initial Stations."

There was in Washington, besides the National Academy of Sciences, what called itself "The Philosophical Society of Washington." In its name, as in that of the American Philosophical Society in Philadelphia (established in 1743), "Philosophical" meant "Scientific." Benjamin Peirce had been one of the founders of both the Academy and the Society. Charles was elected a member of the latter on 1 March 1873. From 1871 to 1874 he presented the following papers:

- 16 December 1871: On the Appearance of Encke's Comet as Seen at Harvard College Observatory
- 19 October 1872: On Stellar Photometry
- 21 December 1872: On the Coincidence of the Geographical Distribution of Rainfall and of Illiteracy, as shown by the Statistical Maps of the Ninth Census Reports
- 17 May 1873: On Logical Algebra
- 3 January 1874: On Quaternions, as Developed from the General Theory of the Logic of Relatives
- 14 March 1874: On Various Hypotheses in Reference to Space.

Charles continued to make presentations to the American Academy of Arts and Sciences in Boston (some of which were published in its *Proceedings*), but they were now predominantly scientific:

- 12 March 1872: On Stellar Photometry (exhibiting the Zöllner astrophotometer)
- 9 March 1875: Photometric Measurements of the Stars
- 11 May 1875: On the Application of Logical Analysis to Multiple Algebra [At this meeting his father presented a paper "On the Uses and Transformations of Linear Algebra."]
- 11 October 1876: On a new edition of Ptolemy's Catalogue of Stars
- 10 October 1877: Note on Grassmann's Calculus of Extension
- 13 March 1878: On the Influence of Internal Friction upon the Correction of the Length of the Second's Pendulum for the Flexibility of the Support
- 11 June 1879: On the Reference of the Unit of Length to the Wave-Lengths of Light.

We return now to the theme of this section of the present introduction. What opened the way to the breadth and intensity of Peirce's scientific work in the period of the present volume? His father's initiative in beginning the transcontinental geodetic survey, and that of Superintendent Patterson in continuing it. Patterson in 1878 obtained an act of Congress changing the Survey's name to: The Coast and Geodetic Survey. The transcontinental survey was finally completed in the late 1890s. Meanwhile, the survey of the Atlantic and Gulf coasts had gradually been transformed by connecting it with a geodetic survey along the "eastern oblique arc" from Calais in Maine to New Orleans. (Peirce's own first year with the Survey, 1859–60, had taken him to both ends of this arc.)

Both surveys were finally completed in the late 1890s, were edited by Assistant Charles A. Schott, and were published in 1900 (871 quarto pages) and 1902 (394 quarto pages) under the titles *The Transcontinental Triangulation and the American Arc of the Parallel* and *The Eastern Oblique Arc of the United States and Osculating Spheroid*. These are two classics of the science of geodesy. Peirce's own connection with the Survey had ceased at the end of 1891, but he drafted a review of them, with emphasis on the latter. If that review had been carefully revised and published, it would itself rank as a milepost in the history of geodesy. (It should be added that, at the time Peirce drafted this review, work was beginning on the geodetic survey of the 98th meridian, which runs from eastern North Dakota through South Dakota, Nebraska, Kansas, Oklahoma, to a point not far from the southernmost tip of Texas. Obviously this was chosen as the longest nearly central meridian.)

Peirce was not merely a philosopher or a logician who had read up on science. He was a full-fledged professional scientist, who car-

ried into all his work the concerns of the philosopher and logician. So when, for example, he wrote his “Illustrations of the Logic of Science,” which moved rapidly to questions of statistics and probability, he had already made professional contributions to precisely those fields. At almost the same time, it was as a professional statistician that he reviewed his Italian friend Ferrero’s book on the method of least squares in the first issue of the *American Journal of Mathematics*.

### 3. *The Metaphysical Club and the Birth of Pragmatism*

In the first part of the introduction to volume 2 of this edition we presented evidence for concluding that Peirce was a nominalist at first, and that his first steps toward realism were taken in his *Journal of Speculative Philosophy* articles of 1868–69 and in his Berkeley review of 1871. The essential element in these steps was giving ‘real’ and ‘reality’ a forward rather than a backward reference. The natural and logical next step, we said, was the pragmatism that, according to James and Peirce in recollections of a quarter of a century later, was born in the Metaphysical Club in the early 1870s.

Of all the papers in the present volume, the one so far most often referred to has been that of January 1878, “How to Make Our Ideas Clear,” and its oftenest quoted paragraph is: “It appears, then, that the rule for attaining the third grade of clearness of apprehension is as follows: Consider what effects, which might conceivably have practical bearings, we conceive the object of our conception to have. Then, our conception of these effects is the whole of our conception of the object.”

It was over twenty years later, in September 1898, in William James’s “Philosophical Conceptions and Practical Results,” that “pragmatism” first appeared *in print* as the name for this rule; but James said there that Peirce had *called* it “the principle of . . . pragmatism . . . when I first heard him enunciate it at Cambridge in the early ’70’s.” James says nothing of the occasion for its enunciation, but we shall find reason below to conclude, at least tentatively, that it was a meeting of the Metaphysical Club not later than November 1872.

As late as 1909 Peirce was revising the “Illustrations of the Logic of Science” to reappear at last in book form, with revisions of the first two papers presented as two parts of a single paper to be entitled

“My Pragmatism.” Drafts of the preface to the projected volume, which never reached publication, contain the fullest surviving comparison between the Metaphysical Club paper of 1872 and the first two “Illustrations” of 1877–78.

We cannot identify the Club paper with any known surviving manuscript, but, on the hypothesis that there was such a paper, we may turn to what we have called “Toward a Logic Book, 1872–73” and ask ourselves how much of it anticipates the first two “Illustrations.” Item 9 in our table of contents, written between 11 and 14 May 1872, will then give us some idea how close the correspondence between the Club paper and the first two “Illustrations” *might* have been. It may next strike us that the applications of the maxim in “How to Make Our Ideas Clear” to the ideas of hardness, weight, and force are there for the sake of its application to the idea of reality; and we may then reread the different versions of a chapter on “Reality” in “Toward a Logic Book” with heightened interest. We should then be ready to interpret and evaluate the remark at the end of the first part of the introduction to volume 2, that pragmatism was the natural and logical next step from the forward reference of the idea of reality in the *Journal of Speculative Philosophy* articles and the Berkeley review.

Our earliest evidence of the Metaphysical Club’s existence is in two letters of Henry James, William’s younger brother, in January and February 1872, to friends then living in Europe. From these letters alone we might guess that the Club had been founded after Peirce’s return from Washington in January. Peirce himself often assigns it an earlier beginning, soon after his return from Europe in the spring of 1871. Perhaps the founding had been preceded by informal gatherings of some of the same people. Henry James mentions Chauncey Wright, Peirce, Oliver Wendell Holmes, Jr., William James, and “various other long-headed youths” who “wrangle grimly & stick to the question.”<sup>1</sup>

To the four members named by Henry James, Peirce in later recollections adds Nicholas St. John Green, Francis Ellingwood Abbot, John Fiske, Henry Ware Putnam, Francis Greenwood Peabody, William Pepperell Montague, and Joseph Bangs Warner. (Within the year 1872 Green and Wright reached the age of 42, Abbot 36, Peirce 33, Holmes 31, James and Fiske 30, Putnam and

<sup>1</sup>*Henry James Letters*, edited by Leon Edel (Cambridge: Harvard University Press, 1974), I:273. Cf. p. 269.

Peabody 25, Montague and Warner 24.) Peabody and Warner had attended Peirce's lectures on British Logicians in 1869–70 and had studied Kant with him privately. They had also attended Fiske's lectures, which had immediately preceded Peirce's. Abbot had been a Harvard classmate of Peirce's. With the possible exception of Putnam, all ten were important figures in Peirce's life. From his undergraduate years he had known Wright, a bit less than nine years older than he. His acquaintance with James had begun when they were in the Lawrence Scientific School together. James's family had moved to Cambridge in the fall of 1866. Their home was on Quincy Street, across from the Harvard Yard, about where the Faculty Club now stands. Peirce's review of *The Secret of Swedenborg*, by James's father, was included in our second volume. James and Holmes had attended some of Peirce's Lowell Lectures (included in our first volume) together in the fall of 1866. Holmes's father had been a Harvard classmate of Peirce's father, and they were fellow members of the Saturday Club. When Peirce's father died in 1880, Holmes's father wrote the poem in his honor that appeared in the *Atlantic Monthly*.

The most striking fact about the eleven members named by Peirce is that more than half of them were lawyers. (Only three were scientists—Wright, Peirce, and James, who was then teaching anatomy and physiology; two were theologians—Abbot and Peabody; the remaining six were lawyers, and of these all but Fiske were lifelong lawyers.) And the most striking remark that Peirce later makes about the birth of pragmatism in the Club is that, while acknowledging the paternity that James had already ascribed to him, he calls lawyer Green its grandfather, because Green had so often urged the importance of applying Alexander Bain's definition of belief as "that upon which a man is prepared to act," from which "pragmatism is scarce more than a corollary."

Since it is in letters *from* Henry James to friends in Europe that we first hear of the Metaphysical Club, it is a matter of interest that it was in a letter *to* Henry after Henry's own return to Europe that William James wrote on 24 November 1872: "Chas. Peirce . . . read us an admirable introductory chapter to his book on logic the other day."<sup>2</sup> Thomas Sergeant Perry wanted it for the *North American Review*, in which Peirce's Berkeley review had appeared in the

<sup>2</sup>Ralph Barton Perry, *The Thought and Character of William James* (Boston: Little, Brown, and Co., 1935), I:332.

previous year; but Peirce thought it not suitable for the *Review*, perhaps because it was too technical or assumed too much that had been argued out in the Club. This was probably the occasion recalled by James in 1898 as that on which Peirce enunciated the principle of pragmatism and called it by that name.

Fiske died in 1901. Perry was working on a short biography of him in 1905. James wrote Perry on 24 August: "If you want an extra anecdote, you might tell how, when Chauncey Wright, Chas. Peirce, St. John Green, Warner and I appointed an evening to discuss the 'Cosmic Philosophy,' just out, J. F. went to sleep under our noses."<sup>3</sup> That would have been in November 1874. Wright died 12 September 1875. Peirce was in Europe then. James wrote the obituary for *The Nation*. On 10 February 1876, James wrote to his brother Robertson James: ". . . we have reorganized a metaphysical club here."<sup>4</sup> The other members of the original Metaphysical Club it included were Green, Holmes, Fiske, Warner, and Abbot. Peirce was still in Europe, and he never resumed residence in Cambridge. Green died 8 September 1876, less than a year after Wright. Without Wright and Green, and without Peirce, the reorganized metaphysical club may not have borne much resemblance to the one in which pragmatism was born.

But, whether for the original or for the reorganized club, why the name "Metaphysical" rather than "Philosophical?" Negatively, because "philosophical" still meant scientific, as in the old American Philosophical Society in Philadelphia or in the new Philosophical Society of Washington. Positively, because the most famous club in the world that was philosophical in our sense was the Metaphysical Society in London, which had been founded in 1869. Many papers presented to that Society had already appeared in the *Contemporary Review* in 1870 and 1871. Peirce had spent several weeks in London in July 1870 and in February 1871, and his father had been there in October 1870 and in January 1871. They can scarcely have failed to hear of the Society.<sup>5</sup>

Back now to Holmes and the law-dominated Metaphysical Club in Cambridge. In the spring of 1872 Holmes gave a course of twelve

<sup>3</sup>Henry James, *The Letters of William James* (London: Longmans, Green, and Co., 1920), II:233.

<sup>4</sup>Perry, *Thought and Character*, I:713.

<sup>5</sup>See Alan Willard Brown, *The Metaphysical Society: Victorian Minds in Crisis, 1869-1880* (New York: Columbia University Press, 1947).

University Lectures on Jurisprudence, with Austin's *Lectures on Jurisprudence* as text. Though we have so far no evidence of such a meeting, it seems likely that at least one meeting of the Metaphysical Club that spring was devoted to discussion of the main argument of Holmes's lectures. Holmes became the sole editor of the *American Law Review* beginning with the July 1872 issue. In that issue, in a notice of an article by Frederick Pollock criticizing Austin in the April number of *Law Magazine and Review*, Holmes included a summary of his own lectures. Taking a different tack from Pollock's, he pushed to its logical conclusion Austin's view that custom only becomes law by the tacit consent of the sovereign manifested by its adoption by the courts, and that before its adoption it is only a motive for decision. What more, Holmes asked, is the decision itself in relation to any future decision?

What more indeed is a statute; and in what other sense law, than that we believe that the motive which we think that it offers to the judges will prevail, and will induce them to decide a certain case in a certain way, and so shape our conduct on that anticipation? A precedent may not be followed; a statute may be emptied of its contents by construction, or may be repealed without a saving clause after we have acted on it; but we expect the reverse, and if our expectations come true, we say that we have been subject to law in the matter in hand. It must be remembered . . . that in a civilized state it is not the will of the sovereign that makes lawyers' law, even when that is its source, but what a body of subjects, namely, the judges, by whom it is enforced, *say* is his will. The judges have other motives for decision, outside their own arbitrary will, beside the commands of their sovereign. And whether those other motives are, or are not, equally compulsory, is immaterial, if they are sufficiently likely to prevail to afford a ground for prediction. The only question for the lawyer is, how will the judges act? Any motive for their action, be it constitution, statute, custom, or precedent, which can be relied upon as likely in the generality of cases to prevail, is worthy of consideration as one of the sources of law, in a treatise on jurisprudence. Singular motives . . . are not a ground of prediction, and are therefore not considered.<sup>6</sup>

This predictive theory remained the most prominent feature of Holmes's philosophy of law. His fullest and best exposition of it was in "The Path of the Law" in 1897. It has since come to be called "legal pragmatism." Accepting that name for it, we remark that legal

<sup>6</sup>*American Law Review* 6 (1872): 724. (Reprinted in Frederic Rogers Kellogg, *The Formative Essays of Justice Holmes: The Making of an American Legal Philosophy* [Westport, CT: Greenwood Press, 1984], p. 92.)

pragmatism was in print five and a half years before logical pragmatism. And even if Peirce had permitted Perry to publish his Metaphysical Club paper in the *North American Review*, logical pragmatism would have been, at the very least, six months behind legal pragmatism in reaching print.

It is often asserted or assumed that Peirce had little or no interest in law, in the philosophy of law, or even in political or social philosophy; but we know that, at least by the end of 1871, he was intensely interested in mathematical economics; we have his wife Zina's reports of his advocacy of proportional representation; she was president of the first Woman's Parliament in 1869; his mother's father had been a lawyer, founder of one of the earliest law schools in the country and U.S. Senator from Massachusetts; his father's mother would have married lawyer Joseph Story, later Justice of the Supreme Court, if her parents had not dissuaded her; his own father was a leading member of the American Social Science Association (which antedated the more specialized social science associations) and was chairman of its Department of Education from 1869 to 1872; Charles and his father had been expert witnesses in the famous Howland will case in 1867; his older brother "Jem" (J. M.) had spent a year in the Harvard Law School; his younger brother Herbert went into diplomacy and became our Minister to Norway; and his own vividest recollections of the Metaphysical Club are of its oldest member, lawyer Green.

To Baldwin's *Dictionary of Philosophy and Psychology*, in 1902, Peirce contributed the article "Proximate," the principal section of which is on "proximate cause and effect" and derives from Green's "Proximate and Remote Cause," the leading article in the January 1870 *American Law Review*, of which Holmes was already co-editor. Thirty-one years later, this article was the first in the collection of Green's papers edited by his lawyer son under the title *Essays and Notes on the Law of Tort and Crime* (1933). Twenty-one years still later, it appeared a third time as an appendix to Jerome Frank's "A Conflict with Oblivion" as evidence that Holmes's philosophy of law derived from Green's, and hence that "Green was the grandfather not only of Pragmatism in general but of legal Pragmatism as well."<sup>7</sup>

In 1958 the *Journal of Public Law* published a symposium of

<sup>7</sup>Jerome Frank, "A Conflict with Oblivion: Some Observations on the Founders of Legal Pragmatism," *Rutgers Law Review* 9 (1954): 425-63.

three papers on Peirce, followed by a reprinting of his 1892 article "Dmesis," introduced as "one of the very few writings in which this philosopher deals directly with law."<sup>8</sup> It was also the article in which he had come closest to the words he put into Green's mouth seventeen years later, in those vividest recollections of the Metaphysical Club mentioned above.

Our purpose in looking so far beyond the present volume's years is only to encourage readers interested in the philosophy of law and in social philosophy more generally to be on the lookout for them and to expect to find them in this and in preceding and later volumes. (For brief examples in our two preceding volumes, see 1:339 and 399, and 2:464 and 465.)

One last bit of evidence: When Peirce was elected a member of the American Academy of Arts and Sciences in 1867, he was assigned to Class III, Moral and Political Sciences, Section I, Philosophy and Jurisprudence. When Green was elected at the end of November 1872, shortly after Peirce's Metaphysical Club paper was presented, he was assigned to the same class and section; and so was Holmes when he was elected in 1877. Wright had long been a member of Class I, Mathematical and Physical Sciences, Section I, Mathematics. When James was elected in 1875, he was assigned to still another class and section.

One of the striking differences between the 1872–73 chapter drafts toward a logic book and the 1877–78 "Illustrations of the Logic of Science" is the prominence of the theory of signs in the former and its absence from the latter. An obvious though not a conclusive explanation is (1) that none of the chapter drafts on representations or signs seems to have been intended as the first, or even as a very early, chapter in the logic book, and (2) that the "Illustrations" were never completed.

What are the evidences of the incompleteness? (a) The forward references to topics that the six papers do not reach. (b) The last words of the third paper: "at this early stage of our studies of the logic of science." Half way through a series of six papers is not an "early stage." (c) The readers of the *Popular Science Monthly* were given no hint that the sixth paper was to be, or had been, the last. (d) The publishers of the *Monthly* were also the publishers of the International Scientific Series, and among the volumes they advertised as in

<sup>8</sup>*Journal of Public Law* 7 (1958): 30–36. Cf. CP 2.164 (1902).

preparation was *Illustrations of the Logic of Science* by Charles S. Peirce; but the six papers would not have made much more than half a volume. (e) Early in 1881 Peirce wrote to his mother: "I am thinking of undertaking some more papers for the Popular Science Monthly though I can hardly screw myself up to that point yet."

That further "Illustrations" would still have been welcomed and published was assured by the fact that their importance had been recognized by G. Stanley Hall in his article in *Mind* for January 1879 on "Philosophy in the United States." He gave greatest space to them, assumed there were more to come, and said they promised to be "one of the most important of American contributions to philosophy."

The incompleteness of the "Illustrations" is the obvious answer to the question: "If pragmatism is the lesson in logic taught by Darwin's *Origin of Species*, why does Peirce never get back to Darwin and the *Origin*?" We may ask ourselves, "If he had got back, what would he have said?" And we may remind ourselves that in his published opening lecture at The Johns Hopkins University in September 1882 he said, among other things: "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."

And in 1909, five years from the end of his life, in revising the third and fourth "Illustrations," he wrote that when the *Origin* reached Cambridge early in the winter of 1859, he was with a Survey party on the east coast of Louisiana. A letter from his mother told him what a sensation the book had made; and thereupon "I wrote to my friend Mr. Chauncey Wright that I felt confident that Darwin had received a hint of his idea from Malthus *On Population*."

A better answer would be the paper on "Design and Chance" that he presented to the Metaphysical Club at The Johns Hopkins University in 1884, and thus got back to the *Origin* at greatest length, by way of honoring its twenty-fifth anniversary.

But, even without their intended continuation, the six "Illustrations" that were published in 1877–78 have gradually come to be recognized as the nineteenth century *Discourse on the Method of*

*Rightly Conducting the Reason and Searching for the Truth in the Sciences;* and so far no twentieth century *Discourse* has superseded it.<sup>9</sup>

MAX H. FISCH

<sup>9</sup>For more detailed discussions and further evidence regarding several of the points made in the third part of this introduction, see the following essays: "Justice Holmes, the Prediction Theory of Law, and Pragmatism," *Journal of Philosophy* 39 (1942): 85–97; "Alexander Bain and the Genealogy of Pragmatism," *Journal of the History of Ideas* 15 (1954): 413–44; "Philosophical Clubs in Cambridge and Boston," *Coranto* 2 (1964): 12–23; "Was There a Metaphysical Club in Cambridge?" in *Studies in the Philosophy of Charles Sanders Peirce, Second Series*, edited by Edward C. Moore and Richard S. Robin (Amherst: University of Massachusetts Press, 1964), pp. 3–32, and "Was There a Metaphysical Club in Cambridge?—A Postscript," *Transactions of the Charles S. Peirce Society* 17 (1981): 128–30; and "American Pragmatism Before and After 1898," in *American Philosophy from Edwards to Quine*, edited by Robert W. Shahan and Kenneth R. Merrill (Norman: University of Oklahoma Press, 1977), pp. 78–110. See also Philip P. Wiener, *Evolution and the Founders of Pragmatism* (Cambridge: Harvard University Press, 1949); and James D. Miller, "Holmes, Peirce and Legal Pragmatism," *Yale Law Journal* 84 (1975): 1123–40. (Donald R. Koehn, our contributing editor for the "Illustrations" and "Toward a Logic Book, 1872–73," has contributed also to this introduction and to other parts of the present volume.)

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## *Educational Text-Books, II*

*P 66: Nation 14 (11 April 1872): 244–46*

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We do not know when a respectable publication has been prefaced with more boastful words than Mr. Proctor's *Star Atlas* (London: Longmans). In a previous publication, Mr. Proctor had announced that all such works hitherto had been constructed on radically wrong principles, and had put forth a demonstration that there was only one proper way of making a star-atlas. This he repeats in the "Letterpress Introduction" to the present book, only it is a different manner of construction which he demonstrates to be the right one. A regular dodecagon is inscribed in the sphere, and then each face is produced so as to cut off a part of the sphere, and that part is represented on one map. There are, therefore, twelve equal circular maps which overlap each other slightly, except in five points on the circumference of each. The North Pole is made the centre of one of the maps. But after all this theorizing about the method of projection, Mr. Proctor fills in with stars in a very simple manner. He has apparently merely entered them from the British Association Catalogue. The result, at any rate, is that the magnitudes are so extremely inaccurate that there are many parts of the heavens which are perfectly unrecognizable; and on every map the errors are a source of great inconvenience. Let any one who possesses this atlas compare, for example, the Little Bear in the map with the heavens, and he will find that a bare majority of the stars are rightly inserted or omitted. When the author says, "I believe no atlas was ever constructed in which more pains were taken than in the present to avoid errors," he clearly forgets that stars exist in the sky as well as in the B.A. Catalogue, and that some makers of atlases have taken the trouble to examine them. Argelander's *Uranometria* is justly regarded as one of the most perfect works of observation, perhaps in fulfilling its purpose *the* most perfect ever executed. Its atlas is re-

nowned in all lands for its resemblance to the heavens and for its convenience in use. Its accuracy is such that its scale of magnitudes has been everywhere adopted as the standard. But Mr. Proctor has apparently never heard of it. England is eminent in astronomical observation—the Greenwich Observatory alone would suffice to make it so. But Englishmen are generally so naïvely ignorant of what takes place in the great world of science (which does not centre in London, as they seem to imagine) that it is possible for a respectable man to publish a book there the existence of which depends on such ignorance as would disgrace him in Sicily or in Spain. As for the method of dividing the sphere upon which Mr. Proctor prides himself so much, it is exceedingly inconvenient in practice. It cuts Gemini, Orion, the Great Bear, Hercules, all in two. In short, if anybody interested in the stars has not Argelander's incomparable work, then let him take Elijah Burritt's or any other, but not this new one. We speak from experience.

Heat is still the most interesting part of physics, for the time; and we have devoured Mr. Clerk Maxwell's *Theory of Heat* (London: Longmans). It is not intended, however, primarily to amuse, as Tyndall's was; and it also differs from that work in giving a correct idea of the mechanical theory of heat. It is intended for a class-book, and is the very best text-book of physics which has been published for some years. Its study will demand some thought from the student, which will be a fatal objection to its extensive use in this country. It is not made with reference to satisfying examining committees, and to getting boys over the ground with the least possible trouble to them. It discusses a good many subjects not strictly a part of the theory of heat, and we could have wished that some things which do belong here had been enlarged upon more, and that more special facts and tables had been given. Yet it must be allowed that within these 300 pages a more beautiful and perfect account of the theory could not have been given.

The old sensationalists, Hartley, Brown, and the Mills, never wrung many admissions from the advocates of *a-priority*. But Dr. Wilson's *Lectures on the Psychology of Thought and Action, Comparative and Human* (Ithaca: Andrus, McChain & Lyons) is evidence that the new physiological materialists are making more impression. The author gives up the whole of sensation as involving no mind or consciousness, and hopes by that admission to strengthen spiritualism in reference to the other parts of the intellect. But though the

new position has strength, yet the retreat will encourage the anti-supernaturalists and will make for them new converts. Respectable writers cannot long defend a theory which involves such suppositions as that animals and men acquire a knowledge of external things by an immediate action of the spinal cord without the agency of any external organs, as Dr. Wilson does on pp. 249 and 250.

We said last week that the best book for instruction in logic in colleges was Fowler's *Deductive Logic*. We added that a young man who has been through it under a teacher of power will have had his mind enlightened and strengthened, and will be the better prepared for life. In point of fact, we did not intend to apply these expressions to Fowler's *Deductive Logic*, but to his Deductive and Inductive Logics taken as one work. The mistake enables us to express, in a more emphatic way, our opinion of the almost utter worthlessness of deductive logic in education, except as an introduction to the logic of science. In former ages, logic was a pretty good representation of the methods of thought of the greatest minds. The systematic exposition of the art of thinking naturally lagged behind the practice, and men always reasoned better than if they had strictly followed the rules of their logic. Still, the discrepancy was not very great. The logic of Petrus Hispanus (which was written about 1270) exhibits well the character of thought of his time, as that of Ockham does that of his school, and those of Paulus Venetus and Buridanus do that of the latest scholasticism. At the time of the Renaissance, the treatises of Ramus and of Rudolf Agricola show pretty adequately the peculiarities of the humanist mind. But when the scientific age came, so great an intellectual step was made that logic could not well keep up with science. Then some writers, such as Bacon in his *Novum Organum*, and Locke in the *Conduct of the Understanding*, inconsiderately put aside the old syllogistic and topics as though they contained something false, instead of being only incomplete; while others either weakly endeavored to apply the old theory to the new practice or else abandoned the attempt to represent scientific methods in their logic altogether. These last writers invented the word "extralogical," and apply it to scientific reasoning, thus concealing the fact that they shirk their main duty in not investigating this reasoning. Pedants love to teach the least possible, and to teach it in as formal a way and with as complicated a system of big words as possible. Most of the school-books have, accordingly, been limited chiefly to the logic of deduction. At the same time, they have taught, not the only syllogis-

tic system which was ever actually used—the mediaeval logic—but one which could be of no practical avail whatever. The result has been to confirm the natural tendency of the young to reason from words, and to produce a captiousness which is very different from wise caution, and is simply mischievous. Indeed, the only thing to be said in favor of the study of logic as it is ordinarily taught is that it does tend to make the pupil reflect about his reasoning, and to be a little more precise in his thought and language. The greater number of logics which have come to us in the last few years have been of this vicious kind. A boy or girl could not be put to a more useless task than studying either of Day's logics. The work of Professor Bowen, a convenient though not very intelligent compend of the logic of Hamilton, Thomson, etc., is nearly without value in educating the mind. We hoped for something better from Mr. Jevons, because his previous books, while showing very little acquaintance with the history and literature of the subject, have contained some good original thought, and because he belongs to a school which thinks. But we have been sadly disappointed with his *Elementary Lessons* (New York: Macmillan), and cannot think it of any use. It is because Mr. Fowler has made his *Deductive Logic* very short and simple, and has laid the stress chiefly on the inductive logic, and because he does represent in some degree the methods of thought which modern science and learning actually use, that his books seem to us so recommendable, provided *both* are to be studied. To confine the student to the deductive part, a thing which, we fear, will be done by many teachers, owing to this part making a complete book by itself, would be just as bad as to use any of the old text-books.

We promised last week to discuss some of the errors, as they seem to us to be, of Mill's theory of logic which Mr. Fowler adopts. But we have only space here to refer to Mill's doctrine of scientific hypotheses. This was doubtless suggested by a doctrine of Auguste Comte, who divides the sciences into five classes having different degrees of certainty; and by a hypothesis means a proposition which is not proved with the degree of certainty which belongs to the order of science to which it relates. His maxim of hypothesis is, that such a proposition may be allowed a provisional and secondary place in science, provided it is capable of being proved (or disproved) with the degree of evidence appropriate to its order of science. But Comte's conception of a hypothesis is a peculiar one. A scientific hypothesis is usually defined (and is defined by Mr. Mill) as the suppo-

sition of a circumstance which, by the action of known laws (or a generalization of known laws), would result in facts such as have been observed. It is also common to use the term scientific hypothesis to denote a very doubtful conclusion of science. These two meanings are apt to be confounded, and Mill has plainly confounded them when he says that the one condition of the admissibility of a hypothesis is "that it be not destined always to remain a hypothesis, but be of such a nature as to be either proved or disproved by comparison with observed facts." Here, being proved has not the definite meaning that it has in Comte's maxim. There is no absolute distinction to be drawn anywhere between the probability of that which has a bare possibility of truth and that which has a bare possibility of falsehood. A supposition which by the known action of the laws of nature will explain a single known fact, thereby gains some slight probability. This is susceptible of exact demonstration. As the number of facts which the hypothesis explains increases, and as their variety (depending on the laws their explanation involves, and the elements of the hypothesis upon which they depend) increases, the probability of the hypothesis increases indefinitely, until it becomes as certain as any fact we know. But, as a general rule, that which was a hypothesis at first, remains a hypothesis to the last. All that we receive upon testimony is hypothesis; it explains the fact that the witnesses agree. The existence of the relation of space among things, and all that we remember, are hypotheses in the same sense in which it is a hypothesis to say that Marshal Bazaine surrendered Metz treacherously. Between these extremes, hypotheses of every degree of probability may exist, and no absolute line is to be drawn among them. A hypothesis, therefore, does not differ from any other inferential proposition; and the only thing to be considered in reference to its admissibility is the actual evidence upon the matter. Mr. Mill's view is that a hypothesis is not something inferred, but something taken as the basis of enquiry; so that the question is not what the existing evidence is, but what evidence is forthcoming. Here two questions must be distinguished: the first, in reference to what a man may logically do; the second, as to how he may best economize his scientific energies. Now a man may investigate the truth of any proposition whatever, and if he makes no false inference there is nothing illogical in his procedure. But he will be very unwise to spend a large portion of his life in putting anything to the test which can hardly be true or which can hardly be false. When the questions put to nature will only be

answered by yes or no, he will advance with the greatest rapidity (as in the game of twenty questions) by asking questions an affirmative answer to which is equally probable with a negative one. He must, however, consider what degree of certainty the answer will have, and the rule will be, among questions of equal importance, to make that investigation which will have the greatest effect in altering existing probabilities. Mr. Mill seems to suppose an absolute distinction between the adoption and the rejection of a hypothesis; but every scientific man has passed that rude state of mind, and takes into account, in every case, as well as he can, the degree of evidence. Making distinctions absolute which are really only relative is the source of most of the errors in Mill's system of philosophy.

There are various other modern schools of logic besides those to which we have referred. In the first place, Boole, De Morgan, and others have made a more exact investigation into purely formal logic, and have greatly advanced the subject. Their researches are still in a very immature state, but they have already succeeded in throwing much light upon the subject. The metaphysical part of logic has been chiefly prosecuted in Germany. Such questions as these: What is the connection between the following of a conclusion from its premises and the following of an effect from its cause? and what is the connection between the relation of a subject to its predicate and the relation of a substance to its attributes? have a high philosophical importance. Hegel considers the real relations of existing things and the formal relations of thought to be strictly identical; but he is led to modify profoundly the usual views regarding the maxims of reasoning in making out his point. His philosophy is now exploded; that is to say, hardly any of the rising men adopt it. But its historical importance has been considerable. For a short time it had immense influence in Germany. Mr. Carroll Everett's *Science of Thought* (Boston: William V. Spencer) is regarded by Hegelians as a good exposition of the fundamental positions of their philosophy. Vague conceptions and complicated reasoning are continually causing Mr. Everett to fall into fallacies; and this is the universal fault of Hegelians. The consequence is that their conclusions are entirely uncertain; and the interesting and profound suggestions with which their philosophy abounds only serve to make the bad influences of their loose reasoning upon half-educated minds all the greater. Ueberweg's treatise (*System of Logic and History of Logical Doctrines*, London: Longmans) is an excellent specimen of a modern German logic. The view

defended is that the construction of the mind corresponds with the order of nature, so that metaphysical conceptions have a double character, first, as true of things as they really exist; and, second, as merely formal principles of thought. It is a carefully written and scholarly book. The style is clear and precise, more precise than American readers enjoy, but real students do not wish a writer to beat about the bush to avoid an expression merely because it is a little too formal for the taste of literary people. The translator, we regret to say, betrays an ignorance of two things rather essential to his task, logic and the German language. On page 402, we read this extraordinary sentence: "An infinite straight line can proceed but from a figure bounded on all sides in the same plane on two sides only by means of intersecting the boundaries." This will bear a second reading. What Ueberweg says is: "Eine unbegrenzte gerade Linie kann aus einer allseitig begrenzten Figur in derselben Ebene auf beiden Seiten nur mittelst Durchschneidung der Grenzen heraustreten." This is perfectly clear. A straight line lying within an enclosed figure in the same plane cannot be extended indefinitely in either direction without cutting the boundary of that figure. The translator says, "Dr. Ueberweg has himself revised the sheets; and, as he knows English well, this translation may be held to give his opinions as he wishes them expressed in our language." There must be a misrepresentation here.

Mr. Martin Larkin, who compiles *The Rival Collection* (J. W. Schermerhorn & Co.) says that it "contains the best pieces, serious and amusing, in prose and poetry, that can be comprised in a work of this kind." Some of them it contains. And it also contains some of the worst, especially among the "amusing" pieces, a good many of which are frightful examples of vulgarity. All the same, it is a good collection, in the sense of containing a great many poems and other pieces fit to be declaimed or recited by the boys and girls on declamation day. With the old favorites are many new selections, which, if not so likely to become and remain favorites, will be a very welcome addition to any young orator's repertory of borrowed eloquence.

## [Lecture on Practical Logic]

*MS 191: Summer-Fall 1872*

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I suppose that the fundamental proposition from which all metaphysics takes its rise is that opinions tend to an ultimate settlement & that a predestinate one. Upon most subjects at least sufficient experience, discussion, and reasoning will bring men to an agreement; and another set of men by an independent investigation with sufficient experience, discussion, and reasoning will be brought to the same agreement as the first set.

Hence we infer that there is something which determines opinions and which does not depend upon them. To this we give the name of the *real*. Now this *real* may be regarded from two opposite points of view.

In the first place, to say that thought tends to come to a determinate conclusion, is to say that it tends to an end or is influenced by a *final cause*. This final cause, the ultimate opinion, is independent of how you, I, or any number of men think. Let whole generations think as perversely as they will; they can only put off the ultimate opinion but cannot change its character. So the ultimate conclusion is that which determines opinions and does not depend upon them and so is the real object of cognition. This is idealism since it supposes the real to be of the nature of thought.

But, in the second place, a cause precedes its effect. And moreover the ultimate conclusion though independent of this or that mind is not independent of mind in general. The real, therefore, which determines thought but does not depend upon it, is not the last conclusion but the first premise or what produces the first premise,—a something out of the mind and incommensurable with thought.

Since experience proceeds from the less general to the more general, the last conclusion is general, and so the first view is realistic,

while the second from a like reason is individualistic. In the first view, the real is in one sense never realized since though opinion may in fact have reached a settlement in reference to any question, there always remains a possibility that more experience, discussion, and reasoning would change any given opinion. In the second view also the real is a species of fiction for that which is logically singular,—or is determined with reference to every quality,—can from the continual change which is constantly taking place not remain for any time however short, (Daniel Webster, for example, is a class embracing Daniel Webster under 50 years of age & Daniel Webster over 50 years of age) and consequently does not exist as absolutely determinate at all.

Upon either view therefore the real is something ideal and never actually exists. But it is true on the one hand that thought tends to a determinate conclusion and on the other that if anything is true, true determinations without number are true of it. We ought therefore to discard the conception of the real as something actual and to say simply that only thought actually exists and it has a law which no more determines it than it by the mode in which it acts makes the law. Only this law is such that in a sufficient time it will determine thought to any extent.

## Third Lecture

*MS 192: Summer-Fall 1872*

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I begin with the soul of man. For we first learn that brutes have souls from the facts of the human soul. What brutes and other men do & suffer would be quite unintelligible to us, if we had not a standard within ourselves with which to measure others.

At the first dawn of cognition we began to compare and consider the objects about us. Our thought first assigned to things their right places and reduced the wild chaos of sensuous impressions to a luminous order. But after thought had classified everything a residuum was left over, which had no place in the classification. This was thought itself. What is this which is left over? After thought has considered everything, it is obliged next to think of itself. Here it is at once means and end. The question is, *what* is thought,—and the question can only be answered *by means of* thought.

This is a noticeable circumstance. How can thought think of itself, it is asked; that would be an insoluble contradiction. It is as though a tone should be heard of itself, or a beam of light be seen by itself. But this objection reminds one of the efforts of the man who tried to look at his own eye. After great difficulty he got so far as to see the end of his nose, forgetting that it would be much simpler to hold up a looking-glass to his face. Common sense, which usually hits the nail on the head, has long ago held that looking-glass up to thought. If I wish to represent to myself what my thought is, (says common sense) I have only to act as though my thought were an external object which I can consider as I should consider something not a part of myself. Thought thus objectively considered common sense terms the soul. So if we are to investigate in a scientific manner the nature of thought, we //need/can// do nothing else than consider the soul as if it were an object of experience.

Everyone grants that thought is a sort of experience; otherwise,

we could not know that we think. Everyone further sees that we have in thought a very varied experience, for it changes both with the object thought of and with mental development which we have attained. Thus, we bring together all the experiences which thought has in itself & subject them to the consideration of our thoughts. There are also other experiences, not properly thoughts, such as sensations and feelings which we term phenomena of the soul, because we recognize them as immediate products of an activity within us, which according to our observation cannot be separated from the activity of thought.

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*{TOWARD A LOGIC BOOK, 1872-73}*

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## /Logic, Truth, and the Settlement of Opinion/

*MS 179: Winter-Spring 1872*

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Logic is the doctrine of truth, its nature and the manner in which it is to be discovered.

The first condition of learning is to know that we are ignorant. A man begins to inquire and to reason with himself as soon as he really questions anything and when he is convinced he reasons no more. Elementary geometry produces formal proofs of propositions which nobody doubts, but that cannot properly be called reasoning which does not carry us from the known to the unknown, and the only value in the first demonstrations of geometry is that they exhibit the dependence of certain theorems on certain axioms, a thing which is not clear without the demonstrations. When two men discuss a question, each first endeavors to raise a doubt in the mind of the other, and that is often half the battle. When the doubt ceases there is no use in further discussion. Thus real inquiry begins when genuine doubt begins and ends when this doubt ends. And the premises of the reasoning are facts not doubted. It is therefore idle to tell a man to begin by doubting familiar beliefs, unless you say something which shall cause him really to doubt them. Again, it is false to say that reasoning must rest either on first principles or on ultimate facts. For we cannot go behind what we are unable to doubt, but it would be unphilosophical to suppose that any particular fact will never be brought into doubt.

It is easy to see what truth would be for a mind which could not doubt. That mind could not regard anything as possible except what it believed in. By all existing things it would mean only what it thought existed, and everything else would be what it would mean by *non-existent*. It would, therefore, be omniscient in its universe. To say that an omniscient being is necessarily destitute of the faculty of

reason, sounds paradoxical; yet if the act of reasoning must be directed to an end, when that end is attained the act naturally becomes impossible.

The only justification for reasoning is that it settles doubts, and when doubt finally ceases, no matter how, the end of reasoning is attained. Let a man resolve never to change his existing opinions, let him obstinately shut his eyes to all evidence against them, and if his will is strong enough so that he actually does not waver in his faith, he has no motive for reasoning at all, and it would be absurd for him to do it. That is method number one for attaining the end of reasoning, and it is a method which has been much practised and highly approved, especially by people whose experience has been that reasoning only leads from doubt to doubt. There is no valid objection to this procedure if it only succeeds. It is true it is utterly irrational; that is to say it is foolish from the point of view of those who do reason. But to assume that point of view is to beg the question. In fact, however, it does not succeed; and the first cause of failure is that different people have different opinions and the man who sees this begins to feel uncertain. It is therefore desirable to produce unanimity of opinion and this gives rise to method number two, which is to force people by fire and sword to adopt one belief, to massacre all who dissent from it and burn their books. This way of bringing about a catholic consent has proved highly successful for centuries in some cases, but it is not practicable in our days. A modification of this is method number three, to cultivate a public opinion by oratory and preaching and by fostering certain sentiments and passions in the minds of the young. This method is the most generally successful in our day. The fourth and last method is that of reasoning. It will never be adopted when any of the others will succeed and it has itself been successful only in certain spheres of thought. Nevertheless those who reason think that it must be successful in the end, & so it would if all men could reason. There is this to be said in favor of it. He who reasons will regard the opinions of the majority of mankind with contemptuous indifference; they will not in the least disturb his opinions. He will also neglect the beliefs of those who are not informed, and among the small residue he may fairly expect some unanimity on many questions.

I hope it will now be plain to the reader, that the only rational ground for preferring the method of reasoning to the other methods is that it fixes belief more surely. A man who proposes to adopt the first method may consistently do so simply because he chooses to do

so. But if we are to decide in favor of reasoning, we ought to do so on rational grounds. Now if belief is fixed, no matter how, doubt has as a matter of fact ceased, & there is no motive, rational or other, for reasoning any more. Any settlement of opinion, therefore, if it is full and perfect, is entirely satisfactory and nothing could be better. It is the peculiarity of the method of reasoning, that if a man thinks that it will not burn him to put his hand in the fire, reasoning will not confirm that belief but will change it. This is a vast advantage to the mind of a rationalist. But the advocate of any one of the first three methods, will be able to say (if either of those methods will yield a fixed belief) either that he *knows* by his method that fire will burn, so that reasoning is inferior to his method in that it may permit a man for a moment to doubt this, or else that he *knows* that fire will not burn, so that reasoning leads all astray. In either case therefore he will conceive that that which to the rationalist seems the great advantage of reasoning, to be a great fault. Thus the only ground of a fair decision between the methods must be that one actually succeeds while the others break up and dissolve. Bryant expresses the philosophy of the matter perfectly

Truth struck to earth shall rise again  
 The eternal years of God are hers  
 While error . . . writhes in pain  
 And dies amidst her worshippers.

## /Investigation and the Settlement of Opinion/

*MS 180: Winter-Spring 1872*

There is an important difference between the settlement of opinion which results from investigation and every other such settlement. It is that investigation will not fix one answer to a question as

well as another, but on the contrary it tends to unsettle opinions at first, to change them and to confirm a certain opinion which depends only on the nature of investigation itself. The method of producing fixity of belief by adhering obstinately to one's belief, tends only to fix such opinions as each man already holds. The method of persecution tends only to spread the opinions which happen to be approved by rulers; and except so far as rulers are likely to adopt views of a certain cast does not determine at all what opinions shall become settled. The method of public opinion tends to develop a particular body of doctrine in every community. Some more widely spread and deeply rooted conviction will gradually drive out the opposing opinions, becoming itself in the strife somewhat modified by these. But different communities, removed from mutual influence, will develop very different bodies of doctrine, and in the same community there will be a constant tendency to sporting which may at any time carry the whole public. What we know of growth, in general, shows that this will take place; and history confirms us. The early history of sciences before they begin to be really investigated, especially of psychology, metaphysics, etc., illustrates as well as anything the pure effect of this method of fixing opinions. The numerous well-defined species of doctrines which have existed on such subjects and their progressive historical succession gives the science of the history of philosophy considerable resemblance to that of paleontology.

Thus no one of these methods can as a matter of fact attain its end of settling opinions. Men's opinions will act upon one another & the method of obstinacy will infallibly be succeeded by the method of persecution & this will yield in time to the method of public opinion and this produces no stable result.

Investigation differs entirely from these methods in that the nature of the final conclusion to which it leads is in every case destined from the beginning, without reference to the initial state of belief. Let any two minds investigate any question independently and if they carry the process far enough they will come to an agreement which no further investigation will disturb.

But this will not be true for any process which any body may choose to call investigation, but only for investigation which is made in accordance with appropriate rules. Here, therefore, we find there is a distinction between good and bad investigation. This distinction is the subject of study in logic. Some persons will doubt whether any sort of investigation will settle all questions. I refrain, however, from arguing the matter, because I should thus be led to anticipate what

comes later, and because after any demonstration I might give I should still rest on *some* assumption and it is as easy to see that investigation assumes its own success as that it assumes anything else.

If it be objected that it does not necessarily suppose that it can solve *all* questions, I shall merely reply for the present, that it will at least never positively conclude any question to be absolutely insoluble so that it may as well assume them to be soluble after an indefinite time. Some philosophers it is true consider some questions to be demonstrably insoluble but we shall see hereafter that this is an error.

I hasten by these side questions in order to come to my central point.

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## Chapter 1

*MS 181: Winter-Spring 1872*

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Living doubt is the life of investigation. When doubt is set at rest inquiry must stop.

Four methods of effecting a settlement of opinion. 1st and simplest, obstinate adhering to whatever happens to be one's existing opinions. Doubtless this is as a matter of fact done with a vague dislike of unsettling opinions. Sometimes perhaps consciously adopted. [Reading *Advertiser*.] What can be said to a man who adopts this method of settling questions? [Ostrich.] He will not avoid pain and obtain pleasure as a rational person will. However, the person who pursues this method need not admit this. Or admitting it he need not behave or believe consistently. This method does not work in practice long (though it may for the term of a man's life) because men are influenced by one another, even if not by reason.

This suggests second method of settling opinion. By persecution. Method of the church. How it has arisen. How it has succeeded in

history. When it has failed it seems to have failed on account of *natural* influences at work causing men to believe something else. Artificial influences will generally prove less strong than nature. It may be remarked that this same reason helps to make the method of obstinacy fail.

The cause of the failure of persecution suggests a third means of settling opinions. This is by the natural development of opinion. In other words not to try to cure the disease of error, but pursue an expectant treatment. There is a natural course in the growth of opinions. The history of philosophy the great example. Bring morality into question & you will see a determination not to question or discuss it which shows the force of this method. Traditional belief remains undisturbed until one community comes in contact with another. Then it is seen that the result is quite accidental & dependent on surrounding circumstances and initial conditions and belief gets all unsettled.

In this way once more the conviction is forced on man that another's opinion if derived by the same process as his own is as good as his own, & that other's opinion is taken by him for his own. Then he says *we* in the sense of the learned world. Individuation, isolation, consists in individual imperfection.

From this conception springs the desire to get a settlement of opinion in some conclusion which shall be independent of all individual limitations, independent of caprice, of tyranny, of accidents of situation, of initial conditions, which does not confirm *any* belief but unsettles & then settles,—a conclusion to which every man would come who should pursue the same method and push it far enough. The effort to produce such a settlement of opinion is called *investigation*. Logic is the science which teaches whether such efforts are rightly directed or not.

Investigation the *natural* procedure of the mind.

## Chapter 2

That mental action called investigation leads ultimately to a conclusion not dependent on the initial condition of belief. The process consists of two parts: the determination of judgments by previous judgments, & the origination of new judgments.

Conclusion therefore ultimately dependent on these fresh judgments. Yet these are entirely accidental & various. The fact is then

they are destined to be such that a certain conclusion will ultimately result.

Two views of reality.

### Chapter 3

Categories.

### Chapter 4

Nature of signs.

### Chapter 5

Nature of inference in general.

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## Chapter 1 (Enlarged abstract)

*MS 182: Winter-Spring 1872*

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The very first of distinctions which logic supposes is between doubt and belief, a question and a proposition. Doubt and belief are two states of mind which feel different, so that we can distinguish them by immediate sensation. We almost always know without any experiment when we are in doubt and when we are convinced. This is such a difference as there is between red and blue, or pleasure & pain. Were this the whole distinction, it would be almost without significance. But in point of fact the mere sensible distinguishability is attended with an important practical difference. When we believe there is a proposition which according to some rule determines our actions, so that our belief being known, the way in which we shall behave may be surely deduced, but in the case of doubt we have such a proposition more or less distinctly in our minds but do not act from

it. There is something further removed from belief than doubt, that is to say not to conceive the proposition at all. Nor is doubt wholly without effect upon our conduct. It makes us waver. Conviction determines us to act in a particular way while pure unconscious ignorance alone which is the true contrary of belief has no effect at all.

Belief and doubt may be conceived to be distinguished only in degree.

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## Chapter 1 (Enlarged abstract)

*MS 183: Winter-Spring 1872*

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Doubt and belief are two states of mind which feel different. We can tell by our immediate sensation almost always when we doubt & when we are convinced. This is such a difference as there is between red & blue, pleasure & pain. Were this the whole distinction it would be almost without significance. But in point of fact the sensible distinguishability is attended with an important practical difference. When we believe there is a proposition in our minds which determines our conduct according to rule, so that our belief being known the way in which we shall behave may be surely deduced. In the case of doubt we have such a proposition in our minds but do not act from it, or at most it exerts only a limited force upon our action. If we do not so much as conceive the proposition to be believed, there are three cases; either our actions are entirely unconcerned with the matter, or we act as if we had some belief, or it is mere chance how we act. The last two are practically belief & doubt respectively.

But this is not all. Belief is satisfactory; doubt is unsatisfactory. It is the wavering of doubt which is unsatisfactory.

# Chapter 1

## Of the Difference between Doubt & Belief

*MS 187: Between 11 and 14 May 1872*

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We generally know when we wish to ask a question and when we wish to pronounce a judgment, for there is a dissimilarity between the sensation of doubting and that of believing.

But this is not all that distinguishes doubt and belief. There is a practical difference. Our beliefs guide our desires and shape our actions. The assassins or followers of the Old Man of the Mountain used to rush into death at his least command, because they believed that obedience to him would insure everlasting felicity. Had they doubted this, they would not have acted as they did. So it is with every belief according to its degree. The feeling of believing is a more or less sure indication of there being established in our nature something which will determine our actions. Doubt never has such an effect.

Nor must we overlook a third point of difference. Doubt is an uneasy and dissatisfied state from which we struggle to free ourselves and pass into the state of belief; while this latter is a calm and satisfactory state which we do not wish to avoid or to change to a belief in anything else. On the contrary we cling tenaciously not merely to believing but to believing just what we do believe.

Both doubt and belief have positive effects upon us, though very diverse ones. Belief does not make us act at once but puts us into such a condition that we shall behave in a certain way, when the occasion arises. Doubt has not the least effect of this sort, but stimulates us to try to destroy it. This reminds us of the irritation of a nerve and the reflex action produced thereby; while for the analogue of belief, in

the nervous system, we must look to what are called nervous associations, for example to that habit of the nerves in consequence of which the smell of a peach will make the mouth water.

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## Chapter 2. Of Inquiry

MS 188: May-June 1872

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The irritation of doubt causes a struggle to attain a state of belief. This struggle I shall term *inquiry*, though it must be admitted that this is sometimes not a very apt designation.

The irritation of doubt is the only immediate motive for the struggle to attain belief. It is certainly best for us that our beliefs should be such as may truly guide our actions so as to satisfy our desires; and this reflection will make us reject any belief which does not seem to have been so formed as to insure this result. But it will only do so by creating a doubt in place of that belief. With the doubt therefore the struggle begins and with the cessation of doubt it ends. Hence, the sole object of inquiry is the settlement [ . . . ]

1. Some philosophers have imagined that to start an inquiry, it was only necessary to utter a question or set it down upon paper, and have even recommended us to begin our studies with questioning everything! But the mere putting of a proposition into the interrogative form does not stimulate the mind to any struggle after belief. There must be a real and living doubt, and without this all discussion is idle.

2. It is a very common idea that a demonstration must rest on some ultimate and absolutely indubitable propositions. These, according to one school, are first principles of a general nature; according to another are first sensations. But in point of fact an inquiry to have that completely satisfactory result called demonstration, has only to start with propositions perfectly free from all actual doubt.

If the premises are not in fact doubted at all, they cannot be more satisfactory than they are.

3. Some people seem to love to argue a point after all the world is fully convinced of it. But no further advance can be made. When doubt ceases, mental action on the subject comes to an end; and if it did go on it would be without a purpose.

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## Chapter 3. Four Methods of Settling Opinion

*MS 189: May-June 1872*

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If the settlement of opinion is the sole object of inquiry, and if belief is of the nature of a habit, why should we not attain the desired end, by taking any answer to a question which we may fancy, and constantly reiterating it to ourselves, by dwelling on all which may conduce to that belief and learning to turn with contempt and hatred from anything which might disturb it? This simple and direct method is really pursued by many men. I remember once being entreated not to read a certain newspaper lest it might change my opinion upon free-trade. "Lest I might be entrapped by its fallacies and misstatements," was the form of expression. "You are not," my friend said, "a special [ . . . ] 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 that he will certainly go straight to heaven when he dies however he may have behaved 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, "O, 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 irrational, for that only amounts to saying that his method of settling belief is not ours. He does not propose to himself to be rational and indeed will often talk with scorn of man's weak and illusive reason. "In our free country he has a right to think as he pleases."

But this method of fixing belief, which may be called the method of obstinacy, will be unable to hold its ground in practice. The social impulse is against it. The man who adopts it will find that other men think differently from him, and it will be apt to occur to him, in some saner moment, that their opinions are quite as good as his own, and this will shake his confidence in his belief. This conception that another man's thought or sentiment may be equivalent to one's own, is a distinctly new step and a highly important one. It arises from an impulse too strong in man to be suppressed without danger of destroying the human species. Unless we make ourselves hermits we shall necessarily influence each other's opinions so that the problem becomes how to fix belief, not in the individual merely, but in the community.

Let the will of the state act then, instead of that of the individual. Let an institution be created which shall have for its object to keep correct doctrines before the attention of the people, to reiterate them perpetually and to teach them to the young, having at the same time power to prevent contrary doctrines from being taught, advocated, or expressed. Let all possible causes of a change of mind be removed from men's apprehensions. Let them be kept ignorant, lest they should learn of some reason to think otherwise than they do. Let their passions be enlisted so that they may regard private & unusual opinions with hatred and horror. Then, let all men who reject the established belief be terrified into silence. Let the people turn out

and tar and feather such men, or let inquisitions be made into the manner of thinking of suspected persons, and when they are found guilty of forbidden beliefs, let them be subjected to some signal punishment. When complete agreement could not otherwise be reached, a general massacre of all who have not thought in a certain way, has proved to be a very effective means of settling opinion, in a country. If the power to do this be wanting, let a list of opinions be drawn up to which no man of the least independence of thought can assent, and let the faithful be required to accept all these propositions, in order to separate them as radically as possible from the influence of the rest of the world.

This method has from the earliest times been one of the chief means of upholding correct theological and political doctrines, and of preserving their universal or catholic character. In Rome, especially, it has been practised from the days of Numa Pompilius to those of Pius Nonus. This is the most perfect example in history, but wherever there is a priesthood—and no religion has been without one—this method has been more or less made use of. Wherever there is an aristocracy or a guild, or any association of a class of men whose interests depend, or are supposed to depend, on certain propositions, there will be inevitably found some traces of this natural product of social feeling. Cruelties always accompany this system, and when it is consistently carried out they become atrocities of the most horrible kind, in the eyes of any rational man. Nor should this occasion surprise, for the officer of a society does not feel justified in surrendering the interests of that society for the sake of mercy, as he might his own private interests. It is natural therefore that sympathy and fellowship should thus produce a most ruthless power.

In judging this method of fixing belief, which may be called the method of despotism, we must in the first place allow its immeasurable mental and moral superiority to the method of obstinacy. Its success is proportionately greater, and, in fact, it has over and over again worked the most majestic results. The mere structures of stone which it has caused to be put together, in Siam, for example, in Egypt, and in Europe, have many of them a sublimity hardly more than rivalled by the greatest works of nature. And except the geological epochs, there are no periods of time so vast as those which are measured by some of these organized faiths.

If we scrutinize the matter closely we shall find that there has not been one of their creeds which has remained always the same; yet the change is so slow as to be imperceptible during one person's life,

so that individual belief remains sensibly fixed. For the mass of mankind, then, there is perhaps no better method than this. If it is their highest impulse to be intellectual slaves, then slaves they ought to remain.

But no institution can undertake to regulate opinions upon every subject. Only the most important ones can be attended to, and on the rest men's minds must be left to the action of natural causes. This imperfection [ . . . ] may affect every man. And though these affections are necessarily as various as are individual conditions yet the method must be such that the ultimate conclusion of every man shall be the same. This is called the scientific method. Its fundamental hypothesis stated in more familiar language is this. There are real things, whose characters are entirely independent of our opinions about them; those realities affect our senses, according to regular laws, and though our sensations are as different as our relations to the objects, yet by taking advantage of the laws which subsist we can ascertain by reasoning how the things really are, and any man if he have sufficient experience and reason enough about it, will be led to the one true conclusion. The new conception here involved is that of reality. It may be asked how I know there are any realities. If this hypothesis is the sole support of my method of inquiry, my method of inquiry must not be used to support my hypothesis. The reply is this. 1st If investigation cannot be regarded as proving that there are real things, it at least does not lead to a contrary conclusion; but the method and the conception on which it is based remain ever in harmony. No doubts of the method, therefore, necessarily arise from its practice, as is the case with all the others. 2nd, the feeling which gives rise to any method of fixing belief, is a dissatisfaction at two repugnant propositions. But here already is a vague concession that there is some one thing to which a proposition should conform. Nobody, therefore, can really doubt that there are realities, or if he did, doubt would not be a source of dissatisfaction. The hypothesis therefore is one which every mind admits. So that the social impulse does not cause me to doubt it. 3rd, Everybody uses the scientific method about a great many things and only ceases to use it when he does not know how to apply it. 4th Experience of the method has not led me to doubt it but on the contrary scientific investigation has had the most wonderful triumphs in the way of settling opinion. These afford the explanation of my not doubting either the method or the hypothesis which it supposes, and not having any doubt nor believing that anybody else whom I could influence has, it would be the merest

babble for me to say more about it. If there be anybody with a living doubt upon the subject, let him consider it.

To describe the method of scientific investigation is the object of this book. In this chapter, I shall only notice some points of contrast between it and other methods of inquiry.

This is the only one of the four methods which presents any distinction of a right and a wrong way. If I adopt the method of obstinacy and shut myself out from all influences, no matter what I think necessary to doing this, is necessary according to that method. So with the method of despotism, the state may try to put down heresy by means which from a scientific point of view seem very ill-calculated to accomplish its purpose, but the only test *on that method* is what the state thinks, so that it cannot pursue the method wrongly. So with the *a priori* method. If I endeavor to lay my susceptibilities of belief perfectly open to the influences which work upon them, I cannot on those principles go wrong. But with the scientific method, the case is different. I may start with known and observed facts to proceed to the unknown; and yet the rules which I follow in doing so may not be such as investigation would approve. The test of whether I am truly following the method is not an immediate appeal to my feelings and purposes, but on the contrary itself involves the application of the method. Hence it is that bad reasoning as well as good reasoning is possible; and this fact is the foundation of the practical side of logic.

When a man has once chosen the scientific method he positively violates it when he allows any weight [ . . . ]

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## [On Reality]

MS 194: Fall 1872

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The question is, "Whether corresponding to our thoughts and sensations, and represented in some sense by them, there are realities, which are not only independent of the thought of you, and me,

and any number of men, but which are absolutely independent of thought altogether." The objective final opinion is independent of the thoughts of any particular men, but is not independent of thought *in general*. That is to say, if there were no thought, there would be no opinion, and therefore, no final opinion. All that we directly experience is our thought—what passes through our minds; and that only, at the moment at which it is passing through. We here see, thoughts determining and causing other thoughts, and a chain of reasoning or of association is produced. But the beginning and the end of this chain, are not distinctly perceived. A current is another image under which thought is often spoken of, and perhaps more suitably. We have particularly drawn attention to the point to which thought flows, and that it finally reaches; a certain level, as it were—a certain basin, where reality becomes unchanging. It has reached its destination, and that permanency, that fixed reality, which every thought strives to represent and image, we have placed in this objective point, towards which the current of thought flows. But the matter has often been regarded from an opposite point of view; attention being particularly drawn to the spring, and origin of thought. It is said that all other thoughts are ultimately derived from sensations; that all conclusions of reasoning are valid only so far as they are true to the sensations; that the real cause of sensation therefore, is the reality which thought presents. Now such a reality, which causes all thought, would seem to be wholly external to the mind—at least to the thinking part of the mind, as distinguished from the feeling part; for it might be conceived to be, in some way, dependent upon sensation. Here then are two opposite modes of conceiving reality. The one which has before been developed at some length, and which naturally results from the principles which have been set forth in the previous chapters of this book is an idea which was obscurely in the minds of the medieval realists; while the other was the motive principle of nominalism. I do not think that the two views are absolutely irreconcilable, although they are taken from very widely separated standpoints. The realistic view emphasizes particularly the permanence and fixity of reality; the nominalistic view emphasizes its externality. But the realists need not, and should not deny, that the reality exists externally to the mind; nor have they historically done so, as a general thing. That is external to the mind, which is what it is, whatever our thoughts may be on any subject; just as that is real which is what it is, what ever our thoughts may be concerning that particular thing. Thus an emotion of the mind is real, in the sense that

it exists in the mind whether we are distinctly conscious of it or not. But it is not external because although it does not depend upon what we think about it, it does depend upon the state of our thoughts about something. Now the object of the final opinion which we have seen to be independent of what any particular person thinks, may very well be external to the mind. And there is no objection to saying that this external reality causes the sensation, and through the sensation has caused all that line of thought which has finally led to the belief. At first sight it seems no doubt a paradoxical statement that, "The object of final belief which exists only in consequence of the belief, should itself produce the belief"; but there have been a great many instances in which we have adopted a conception of existence similar to this. The object of the belief exists it is true, only because the belief exists; but this is not the same as to say that it begins to exist first when the belief begins to exist. We say that a diamond is hard. And in what does its hardness consist? It consists merely in the fact that nothing will scratch it; therefore its hardness is entirely constituted by the fact of something as rubbing against it with force without scratching it. And were it impossible that anything should rub against it in this way, it would be quite without meaning, to say that it was hard, just as it is entirely without meaning to say that virtue or any other abstraction is hard. But though the hardness is entirely constituted by the fact of another stone rubbing against the diamond yet we do not conceive of it as beginning to be hard when the other stone is rubbed against it; on the contrary, we say that it is really hard the whole time, and has been hard since it began to be a diamond. And yet there was no fact, no event, nothing whatever, which made it different from any other thing which is not so hard, until the other stone was rubbed against it. So we say that the inkstand upon the table is heavy. And what do we mean by that? We only mean, that if its support be removed it will fall to the ground. This may perhaps never happen to it at all—and yet we say that it is really heavy all the time; though there is no respect whatever, in which it is different from what it would be if it were not heavy, until that support is taken away from it. The same is true in regard to the existence of any other force. It exists only by virtue of a condition, that something will happen under certain circumstances; but we do not conceive it as first beginning to exist when these circumstances arise; on the contrary, it will exist though the circumstances should never happen to arise. And now, what is matter itself? The physicist is perfectly accus-

tomed to conceive of it as merely the centre of the forces. It exists, therefore, only so far as these forces exist. Since, therefore, these forces exist only by virtue of the fact, that something will happen under certain circumstances, it follows that matter itself only exists in this way. Nor is this conception one which is peculiar to the physicists and to our views of the external world. A man is said to know a foreign language. And what does that mean? Only that if the occasion arises, the words of that language will come into his mind; it does not mean that they are actually in his mind all the time. And yet we do not say that he only knows the language at the moment that the particular words occur to him that he is to say; for in that way he never could be certain of knowing the whole language if he only knew the particular word necessary at the time. So that his knowledge of the thing which exists all the time, exists only by virtue of the fact that when a certain occasion arises a certain idea will come into his mind. A man is said to possess certain mental powers and susceptibilities, and we conceive of him as constantly endowed with these faculties; but they only consist in the fact that he will have certain ideas in his mind under certain circumstances; and not in the fact of his having certain ideas in his mind all the time. It is perfectly conceivable that the man should have faculties which are never called forth: in which case the existence of the faculties depends upon a condition which never occurs. But what is the mind itself but the focus of all the faculties? and what does the existence of the mind consist in but in these faculties? Does the mind cease to exist when it sleeps? and is it a new man who wakes every morning? It appears then that the existence of mind equally with that of matter according to these arguments which have led to this view which is held by all psychologists, as well as physicists depends only upon certain hypothetical conditions which may first occur in the future, or which may not occur at all. There is nothing extraordinary therefore in saying that the existence of external realities depends upon the fact, that opinion will finally settle in the belief in them. And yet that these realities existed before the belief took rise, and were even the cause of that belief, just as the force of gravity is the cause of the falling of the inkstand—although the force of gravity consists merely in the fact that the inkstand and other objects will fall. But if it be asked us, whether some realities do not exist, which are entirely independent of thought; I would in turn ask, what is meant by such an expression and what can be meant by it. What idea can be attached to that of

which there is no idea? For if there be an idea of such a reality, it is the object of that idea of which we are speaking, and which is not independent of thought. It is clear that it is quite beyond the power of the mind to have an idea of something entirely independent of thought—it would have to extract itself from itself for that purpose; and since there is no such idea there is no meaning in the expression. The experience of ignorance, or of error, which we have, and which we gain by means of correcting our errors, or enlarging our knowledge, does enable us to experience and conceive something which is independent of our own limited views; but as there can be no correction of the sum total of opinions, and no enlargement of the sum total of knowledge, we have no such means, and can have no such means of acquiring a conception of something independent of all opinion and thought.

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## Chapt. 4 (2nd draft)

*MS 195: Fall 1872*

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The memory often deceives us. We not unfrequently feel as though we had had a certain sensation or had been in a certain situation before, when it is, in fact, something entirely new. In fact, to remember is to have a certain feeling now, which makes me think that I formerly had another feeling. I do not directly know that former feeling; for it is past. I only know the feeling of remembering it; and whether I really remember it, or only seem to do so, is a matter of judgment. The only thing then of which I am immediately aware, is the feeling of the passing moment.

If I can not know even what has passed in my mind, or what will pass in it, at some other time than at present, much less can I know immediately what is external to my mind. Uninstructed persons are apt to think, that when they see a chair, for example, the whole form

of that chair is impressed upon their minds at once, by an immediate perception, without any reasoning process by which the sensation is worked up, and brought into intelligible shape. But, in the first place, supposing the chair to be looked at with one eye. It is clear that the most that can be impressed upon the retina, is a flat picture of it. The vision of the chair in three dimensions, is an interpretation of this picture; but is not itself the picture. If it is looked at with both eyes, two differing pictures are made on the two retinas; and the vision of it as one will result from a still more complicated mental process. In point of fact, however, not even two dimensions are given in an immediate visual sensation; because the retina is not spread out like a sheet of paper; but consists of innumerable needle-points, which are directed towards the light, and the top of each of which is sensitive. No one of these, gives any sensation of extension, but only a flash of light without any reference to extension; therefore, all of them together give no sensation of extension, except so far as the mind is able to interpret the signs of extension which they present. It is well understood from the labors of those who have devoted themselves to the study of physiological optics, that these are but indirectly even signs of extension being primarily signs of the muscular motion which is necessary to pass from one point to another. But even if the image of the chair in its three dimensions were directly given, as it is not; still it would not be given as external to the mind. In that sensation there would be contained no decision, whether it were external or whether it were a dream; though signs might be given upon which such a decision could be based. The very fact, that dreams deceive us, shows that in the sensation itself, there is contained no judgment of the externality of the object—at least none that is of any value. A dream is distinguishable from a reality by certain signs: it is dim, and vague, and does not cohere with the rest of experience, and it is capable of explanation by the principle of association from what we have really experienced. And the opposite characters are the signs by which we know real experiences to be real. It needs no argument to show that all that we are immediately aware of is the feeling of the passing moment. And that everything outside of that, is known by this which is the sign of it. There are some feelings which are caused by previous feelings, according to certain laws which are called the laws of the association of ideas. And these are the great body of what is present to the mind, and include all that we pay particular attention to, or value; because it is naturally the

results of mental action that are of importance; while the feelings from which they spring, are examined into only when the mental process comes to be subjected to currents of criticism. The other class of feelings or, perhaps, we should rather say, elements of feeling, is not so explicable by the laws of association from what has gone before in the mind, but involves something altogether new. These are termed the impressions of sense; and it is very difficult, if not impossible, for us to separate these entirely from the results of that elaboration of thought to which they are immediately subjected. It may be said, therefore, that thought, as we know it, is a stream, which, as it goes on, is enlarged by new additions. And yet, all that we can distinctly trace, is the flow; and we can not put our finger on the points at which the new matter emerges. Thus, thought, if it takes place according to that fourth method of inquiry which is termed investigation, as it does upon most subjects, and as it doubtless will come to do on all subjects, reaches at last, as we have seen, a certain definite conclusion. And according to what has been said, the whole struggle which is the motive for investigation, is towards this settled belief and nothing else. So that that to which every image and thought in our minds endeavors to conform, and which it strives to represent, is nothing else than what will be believed in the final upshot of inquiry. On the other hand, if we mount the stream of thought instead of descending it, we see each thought caused by previous thought, until at last we reach the original sensations, which it is supposed themselves are caused by something external. In using the word "supposed," I do not wish to imply that there is any room for doubt in the matter; but only that the external realities are not themselves the immediate object of thought but are only what it is necessary to suppose exist, to account for the phenomena of sensation. We find in this stream of thought, in this succession of images, a certain coherency, harmony or consistency, which can not be due entirely to the laws of association themselves; but which extends into the additions which are made to the body of our thought from without. And it is this coherency of experience which demonstrates the existence of a reality; or something permanent and fixed, to which our thought and experience, more or less perfectly, corresponds. Now we may suppose that this reality is to be found at the one or the other extremity of the stream of thought. It either lies in some external permanency, which causes the sensation; or in the fixed opinion in which the process of thought is destined to result. Let us examine

these two opposite conceptions of reality. The first one is very simple and familiar and will require no explanation.

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## Chap. 4 (— draft)

*MS 196: Fall 1872*

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It is the business of the logician to study the nature of the fourth method of inquiry and to discover the rules for conducting it with success. The whole subject will in the exposition of it here offered to the reader be divided into three parts. The first shall treat of the essence of investigation in general, by whatever mind it is conducted and to whatever subject it is applied. The second shall treat of those maxims of investigation which become necessary owing to the peculiar constitution of man in his senses, and his mental nature. The third shall give some slight outline of the special methods of research which are applicable in the different branches of science, and which arise from the peculiarities of the matter investigated. In this first part then we have, broadly speaking, nothing to do with the nature of the human mind. Only as there are some faculties which must belong to any mind which can investigate at all, these must come under our consideration. All inquiry, for example, presupposes a passage from a state of doubt to a state of belief; and therefore there must be a succession of time in the thoughts of any mind which is able to inquire. In the fourth method of inquiry a certain predetermined though not pre-known belief is sure to result from the process; no matter what may have been the opinion of the inquirer at the outset. It follows that during the investigation elements of thought must have sprung up in the mind which were not caused by any thought which was present at the time the investigation was commenced. Such new ideas springing up in the mind and not produced by anything in the mind, are called sensations. Every mind capable of

investigation must therefore have a capacity for sensations. But were all thoughts of this kind investigation would be almost an involuntary process. We might will to investigate but we could not change the course which investigation should take. There would therefore be no distinction between a right and a wrong method of investigation. Now we have seen in the last chapter, that such a distinction is essential to the 4th method of inquiry and is, in fact, the only thing which distinguishes it from the third. There must be thoughts therefore which are determined by previous thoughts. And such a faculty of producing thoughts from others must belong to every mind which can investigate. Without a succession of ideas in time it is clear that no reasoning is possible. I shall proceed to show that without it and without the determination of one idea by another no *thought* in any proper sense of the word is possible. We may grant (what we shall hereafter see is only true in a limited sense) that without any succession of ideas we may have a feeling and this feeling may be peculiar and distinguishable from other feelings. Furthermore such a feeling may have a power of producing new feelings on appropriate occasions, in such a way as to give it an intellectual signification. For example we may have a feeling which may so affect subsequent feelings that when we see a cloven-hoofed animal we imagine him chewing the cud. And then there is no objection to our saying that the first feeling which had the effect of making us when we saw the animal which was cloven-hoofed think of his chewing the cud,—to our calling that feeling a thought. But in itself it is not a thought. For this principle I take to be axiomatical—That a feeling is nothing but what it is felt to be at the time that it is present to the mind. Any effect which it may have upon subsequent thought, is a fact relating to our mental constitution but is not a character of the feeling in itself as it exists when it is felt. If a feeling could feel itself to have such a relation to other feelings the case would be different. But in point of fact apart from the succession of time, a feeling has no relation to any other. What is it for example to say that one feeling is like another? A feeling is nothing but what it is felt to be and unless the one feeling is felt in feeling the other its likeness to that other is not felt in feeling that other. And therefore in themselves they are not alike. Nor

them should they be felt to be alike. They must be brought together in some third feeling and compared. But a feeling cannot exist except in the passing moment in which it does exist. In the

feeling neither of the others is present but only something or other which stands for them. Apart from this succession

of feelings therefore and as they exist in themselves at the moments they are felt feelings have no likeness nor unlikeness. It is the same if one feeling is said to be more intense than another, or to have any sort of relation to another. So if I say that my state of feeling at any moment, consists partly of sensations of sound and partly of sensations of color this presupposes the classifications of feelings as sound feelings and color feelings which classifications already suppose likenesses between them. And these likenesses cannot themselves exist apart from the succession of time. though feelings cannot be analyzed into other feelings without introducing conceptions belonging to the production of one feeling from another. In themselves then feelings have no parts. Nor can my state of feeling at any instant be said in itself to consist of two different feelings. But every feeling in itself is unanalyzed and absolutely simple.

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## On Reality

MS 197: Fall 1872

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I term that *nothing*, concerning which no predication can have any meaning. You can therefore predicate what you please of it in form, and the sentence will not be false because it will be meaningless. Now there are two cases of nothing.

1st If contradictions enter into the definition of a term, it is necessarily nothing. This case is called *absurdity*.

2nd A term may be nothing independently of any logical maxim. This is a case of simple *unreality*.

We have then some terms determined to be nothing by their *essence* or the ideal starting-point of information, others determined to be nothing in the progress of information. And finally we assign an ideal goal of information and we may suppose others to be determined to be nothing only then, and not before.

## On Reality

*MS 198: Fall 1872*

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The difficulties and doubts of logicians begin with questions about reality. These questions cannot be evaded without ceasing to reflect. To pronounce them unanswerable is an unwarranted metaphysical hypothesis; but no answer is yet agreed upon, and what men cannot come to agreement upon no one of them can be said to know.

The first of these questions is, what is meant by reality; and to answer this it is necessary to investigate what meaning in general is.

A word has a meaning only so far as it is translated into a thought; that is, it must in some way enter into a mind before it *actually* has a meaning. A thought is something that we feel we have; at least, this is usually the case and the exceptions can conveniently be considered separately. I neither exclude these exceptions or admit them; but if they are possible and actually occur, then this process of thinking which takes place unconsciously, at least, leads to some results which are consciously thought or all will admit they are nothing. They may then be regarded like the operations of a calculating engine which are processes of thought only in a derivative sense; that is to say they are thought only in the sense of being accepted as agreeing with thought. Strictly then every actual thought is felt. Now let any feeling have a meaning in the mind of the feeler, and that fact will constitute a thought; so that a thought may be defined, in the first place, as a feeling with a meaning.

To answer the question what meaning, in general, is, it will be sufficient to ascertain how a feeling can have meaning. In order to determine this, it is first necessary to observe that every feeling is in itself quite incomplex. True, a feeling may be highly complicated; but its recognition as such is an act of reflection, a thought about the feeling. The feeling so far as it was immediately present to itself and independent of its analysis, was not complex. So the question is,

how can an incomplex feeling have meaning? The question can be brought to this point in another way also. Every feeling is complex or incomplex. The only way of having meaning which is peculiar to complex feelings, is by a complication of the meaning of its parts. The first question, therefore, is as before how an incomplex feeling can have a meaning.

For an incomplex feeling to have a meaning, it must in the first place be considered to have a meaning. This involves a great deal. It can have no meaning which it is not considered to have. It must be considered to refer to some definite object; this object must itself be some immediate object of consciousness, some other feeling upon the occasion of which the feeling in question arises. It will not be necessary, however, that this occasion should be any actual feeling; it will be sufficient if it is something virtually present in the consciousness just before. Nor will it be necessary that it should be clearly apprehended; it will be sufficient if it is in any degree in the consciousness. Thus, a certain complication of feelings may give rise to a feeling which is a sign of that particular complication. Now this complication was not actually felt except by this very feeling, nor perhaps even then very clearly, yet it is sufficient that there is held to be some element in the preëxisting state of feeling which the feeling indicates to make this feeling mean that. The feeling must not only refer to some apprehended objects, but there must be some apprehended regularity in its application to objects. The feelings which are its objects must be seen (however dimly) to have something in common.

Finally let us lay the accent upon *held*. For a feeling to mean anything it must be *held* to mean something. That is there must be another feeling which means that it means something, and indeed there must be an infinite series of these feelings. In other words the present means nothing except so far as it appeals to the future. What we call the present is necessarily past. Time will not stop for us to think, or rather to state the matter more philosophically, a feeling is not a feeling until there is an infinite series of feelings between that feeling and the present. In other words, thought cannot be comprehended in terms of the feelings which are its ultimate elements, —it is a continuum of feelings, & is related to a feeling as a line to a point.

## Chap. 4. Of Reality

*MS 200: Fall 1872*

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Because the only purpose of inquiry is the settlement of opinion, we have seen that every one who investigates, that is, pursues an inquiry by the fourth method assumes that that process will, if carried far enough, lead him to a certain conclusion, he knows not what beforehand, but which no further investigation will change. No matter what his opinion at the outset may be, it is assumed that he will end in one predestinated belief. Hence it appears that in the process of investigation wholly new ideas and elements of belief must spring up in the mind which were not there before.

Some thoughts are produced by previous thoughts according to regular laws of association, so that if the previous thoughts be known, and the rule of association be given, the thought which is so produced may be predicted. This is the elaborative operation of thought, or thinking *par excellence*. But when an idea comes up in the mind which has no such relation to former ideas, but is something new to us, we say that it is caused by something out of the mind, and we call the process by which such thoughts spring up, sensation. And those parts of investigation which consist chiefly in supplying such materials for thought to work over, combine and analyze, are termed observations. The first thing to be noted then is that since investigation leads us from whatever state of opinion we may happen to have to an opinion which is predetermined, it must be that investigation involves observation as one part of it, and, in fact, the conclusion to which we finally come ultimately depends entirely upon the observations.

We may pause here to make a practical application of this principle. No argument can possibly be a correct one which pretends to disclose to us a fact wholly new without being based on evidence which is new. The metaphysicians are given to this kind of reasoning;

even those of them who are the most energetic in maintaining that all our knowledge comes from sense. Writers upon the nature of the human mind, especially, have built up a great body of doctrine without the aid of any observations or facts, except such as are familiar to all the world. Such things justly excite our suspicion. When Hobbes, for example, would persuade us that no man can act otherwise than for the sake of pleasure, it is clear that this belief would deeply modify our conceptions of men, and our plans of life; but when on asking what supports this momentous conclusion we learn that it is but the simple fact—if it can be dignified by that name—that every man desires to do what he does do, we are led at once to suspect that there is some sophistry in the process by which so novel a conclusion can be drawn from so familiar a premise. So, when modern necessitarians maintain that every act of the will proceeds from the strongest motive, they lay down a principle which should be expected to give rise to a psychological science as exact as mechanics, and capable of reducing human actions to precise calculation. But when we find that the advocates of this principle have made no experiments to test their law, we are strongly inclined to think that there has been some juggle of reasoning which has enabled them thus to create something out of nothing.

An observation, as we have defined it, is merely an idea arising in the mind, and not produced by previous ideas. This is not the complete description of observation as understood by scientific men, and we must be careful that the word does not lead us to conclusions which we are not yet warranted in drawing. For example a dream, a presentiment or some fancied inspiration from on high, might, as far as we have yet seen, involve entirely new elements of thought, and, therefore, be an observation in the sense of our definition, so that we are not yet warranted in saying that such things cannot be the ground of legitimate reasoning. This is a question which we shall have again to examine when we come to consider those maxims of inference which depend upon the peculiar constitution of man.

But observation alone cannot constitute investigation; for if it did the only active part which we should have to play in this method of inquiry would be simply the willing to observe, and there would be no distinction of a wrong method and a right method of investigation. But we have seen that such a distinction is essential to the idea of investigation, and that it is, in fact the only thing which separates this from the third method of inquiry. Accordingly, besides observation

it must be that there is also an elaborative process of thought by which the ideas given by observation produce others in the mind. Besides, the observations are most varied and are never exactly repeated or reproduced so that they can not constitute that settled opinion to which investigation leads. Two men, for example, agree in an opinion, and if you ask upon what their opinions rest they will perhaps allege the same fact. But trace the matter back further; ask them upon what grounds they believe that fact again and you will eventually come to premises that are different. Two minds, for example, may have formed the same judgment of a certain person's character and yet may have based their opinions on observing his behavior on different occasions. The rotation of the earth was at first inferred from the movement of the heavenly bodies; but afterwards the manner in which a long pendulum when allowed to swing would gradually turn around and change its direction of oscillation, afforded an entirely new proof; and there are certain very small movements of the stars, which, if they were capable of sufficiently exact observation, would show another ground for the same conclusion. Indeed, the fact which one man observes, is in no case precisely the same as the fact which another man observes. One astronomer observes that the moon passes over a star so as to hide it at a certain instant at his observatory, another astronomer observes that the same star is occulted at a certain instant at his observatory. These two facts are not the same, because they relate to different stations of observation. What is so plain in regard to astronomical observation, because we are accustomed to precision of thought about this, is equally true in regard to the most familiar facts. You and I both see an ink-stand on the table; but what you observe, is that there is a certain appearance from where you sit, and what I observe, is that there is a certain appearance from where I sit. The fact in which we agree that there is an ink-stand there, is what we conclude from the different appearances which we each severally observe. We may change places and still we shall fail to get each other's observations; for the difference of time then comes in. I may observe that there is such an appearance now as you describe as having existed a few moments before; but I can not observe that there was such an appearance before I took your place. It is needless to multiply these examples, because the slightest reflection will supply them in any number; but what have been adduced are sufficient to show that observations are for every man wholly private and peculiar. And not only can no man make another

man's observations, or reproduce them; but he cannot even make at one time those observations which he himself made at another time. They belong to the particular situation of the observer, and the particular instant of time.

Indeed, if we carefully distinguish that which is first given by sensation, from the conclusion which we immediately draw from it, it is not difficult to see that different observations are not in themselves even so much as alike; for what does the resemblance between the two observations consist in? What does it mean to say that two thoughts are alike? It can only mean that any mind that should compare them together, would pronounce them to be alike. But that comparison would be an act of thought not included in the two observations severally; for the two observations existing at different times, perhaps in different minds, can not be brought together to be compared directly in themselves, but only by the aid of the memory, or some other process which makes a thought out of previous thoughts, and which is, therefore, not observation. Since, therefore, the likeness of these thoughts consists entirely in the result of comparison, and comparison is not observation, it follows that observations are not alike except so far as there is a possibility of some mental process besides observation.

Without however insisting upon this point which may be found too subtle, the fact remains that the observations are not the same in the sense in which the conclusions to which they give rise are the same. All astronomers, for example, will agree that the earth is ninety-two or ninety-three millions of miles from the sun. And yet, one would base his conclusion on observations of the passage of Venus across the sun's disk; another upon observations of the planet Mars; another upon experiments upon light combined with observations of the satellites of Jupiter. And the same thing is equally true in regard to most of the ordinary affairs of life.

Now how is it that the springing up into the mind of thoughts so dissimilar should lead us inevitably though sometimes not until after a long time to one fixed conclusion? Disputes undoubtedly occur among those who pursue a proper method of investigation. But these disputes come to an end. At least that is the assumption upon which we go in entering into the discussion at all for unless investigation is to lead to settled opinion it is of no service to us whatever. We do believe then in regard to every question which we try to investigate that the observations though they may be as varied and as unlike in

themselves as possible, yet have some power of bringing about in our minds a predetermined state of belief. This reminds us of the species of necessity which is known as fate. The fairy stories are full of such examples as this: A king shuts his daughter up in a tower because he has been warned that she is destined to suffer some misfortune from falling in love before a certain age and it turns out that the very means which he has employed to prevent it is just what brings the prophecy to fulfilment. Had he pursued a different course, the idea seems to be that that would equally have brought about the destined result. Fate then is that necessity by which a certain result will surely be brought to pass according to the natural course of events however we may vary the particular circumstances which precede the event. In the same manner we seem fated to come to the final conclusion. For whatever be the circumstances under which the observations are made & by which they are modified they will inevitably carry us at last to this belief. The strangeness of this fact disappears entirely when we adopt the conception of external realities. We say that the observations are the result of the action upon the mind of outward things, and that their diversity is due to the diversity of our relations to these things; while the identity of the conclusion to which the mind is led by them is owing to the identity of the things observed, the reasoning process serving to separate among the many different observations that we make of the same thing the constant element which depends upon the thing itself from the differing and variable elements which depend on our varying relations to the thing. This hypothesis I say removes the strangeness of the fact that observations however different yield one identical result. It removes the strangeness of this fact by putting it in a form and under an aspect in which it resembles other facts with which we are familiar. We are accustomed very rightly to think that causes always precede their effects and to disbelieve in fate, which is a fancied necessity by which some future event as it were forces the conditions which precede to be such as would bring it about. That there is no such intrinsic and unconditional necessity to bring about events Western nations are fully and rightly convinced. This is why it seems strange to assert that the final conclusion of the investigation is predestined and why it is satisfactory to the mind to find a hypothesis which shall assign a cause preceding the final belief which would account for the production of it, and of the truth of this conception of external realities there can be no doubt. Even the idealists, if their doctrines are rightly under-

stood have not usually denied the existence of real external things. But though the conception involves no error and is convenient for certain purposes, it does not follow that it affords the point of view from which it is proper to look at the matter in order to understand its true philosophy. It removes the strangeness of a certain fact by assimilating it to other familiar facts; but is not that fact that investigation leads to a definite conclusion really of so different a character from the ordinary events in the world to which we apply the conception of causation that such an assimilation and classification of it really puts it in a light which, though not absolutely false, fails nevertheless to bring into due prominence the real peculiarity of its nature. That observation and reasoning produce a settled belief which we call the truth seems a principle to be placed at the head of all special truths which are only the particular beliefs to which observation and reasoning in such cases leads. And it is hardly desirable to merge it among the rest by an analogy which serves no other purpose. That the conception of external realities is a very embarrassing one for the philosophical questions to which it gives rise is very well known to metaphysicians. While it seems to bring the process of unity of mental action into an analogy with that of other facts, it at once creates the necessity of supposing an extraordinary exception to the ordinary laws of mechanics. We find that we have by this means created two worlds—a mental world of representations and images, which the laws of reflection must show can not be of the same nature with these external objects even if we adopt the belief that the mind is merely a function of the brain. And we find this world of ideas influenced by the external objects in a manner which the laws of mechanics can not possibly explain, and in its turn influencing external objects in a manner which seems absolutely contradictory to the general principles of mechanics. But if we fully acknowledge the justice of the principles which have been set forth in preceding chapters, we shall, I think, be led to the solution of these difficulties which without impugning the truth of the belief in an external world will nevertheless elucidate and translate it into terms of other conceptions it did not give rise to, of the metaphysical difficulties of this hypothesis. We have maintained and proved that the sole purpose of inquiry is to produce a settled opinion. The object at which alone we aim then in the struggle for belief, is to make our belief conform to that final belief. The only thing then which our thought strives to picture or represent is the object of final belief. Now what is the difference

between a reality and a fiction? A fiction is something whose character depends upon what we think about it; a reality is what it is whatever we may think about it. When Swift wrote *Gulliver's Travels* he was at liberty to endow the Island of Brobdignag with any qualities that seemed to suit his purpose. And these, therefore, became the characters of his fancied island. But were he to imagine any such things about the Island of Formosa, he would not make the real island of that sort at all; even if what he imagined about Formosa should happen to be true, there would be a great distinction between what he thought, and the fact; for the reason that his conception would be what it was simply because so he thought; while the real island would be unaffected by his thought. We must not forget here the distinction between a thought as an operation which takes place in the mind, or in the soul, or in the brain and the thought in the sense of an image, or some kind of representation which the thinking process brings present to us. The one is influenced in a literal sense; the other only in so far as it is made present to us by the fact of our thinking, or going through that process of the soul

Now for example if I imagine a gray dragon the process of thinking which goes on in my mind is not gray. The dragon which I imagine is gray.

It would be very preposterous to try to see any resemblance whatever between an island or any other outward object, and the process of thinking. But the thought which is the product of thinking and which is thereby made present to us differs from the real island only in these respects: First; that it may be a false or incomplete representation of the real island and second; that of what sort it is depends upon how we think it to be; while the real island has no such dependency on our aid. But now let us consider the final thought, which is that thought which is the final upshot of the investigation —that to which we always strive to make our thoughts conform. The thought thus is no longer of any particular man, or of any number of men. The thoughts of a man or of many men may conform to it; but however closely they conform, it differs from them in this respect: that their thoughts are changed if they think otherwise; but it is not changed if they think otherwise. For the prejudice, incompetency or ignorance of any number of men, or of generations of men may postpone the agreement in the final opinion but can not make that final opinion to be other than it is to be. So it is quite independent of how any number of men think, and thereby is distinguished from

other thoughts as completely as the external reality is. And indeed, in this fact that it is not even affected by any allusion to the thoughts of you or me or any number of men, it conforms entirely to the description which we have given of reality, that it should be what it is whatever we may think about it. I do not say that any thinking process is the reality; but I say that that thought to which we struggle to have our thoughts coincide, is the reality. Therefore when we say that there are external things, and that observations are only the appearances which these things produce upon sense by their relations to us, we have only in an inverted form, asserted the very same fact and no other which we assert when we say that observations inevitably carry us to a predetermined conclusion. Still it may be asked whether there may not be some other reality which is external to us in some other sense besides this. This I think a rather idle question. Because the doctrine of the hypothesis of external realities, is adopted to simplify and make clear certain facts which are as perfectly brought to a unity by this mode of conceiving the reality, as by any other. However, in order not to leave any portion of the question untouched, I will undertake to show that it can mean nothing at all to say that any other reality than this exists.

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## Of Reality

*MS 203: Fall 1872*

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According to what has been said everyone who investigates assumes that investigation will lead to some conclusion, not known beforehand but predestinated in this sense that every investigator who pushes his research far enough will certainly rest in it. Thus, the conclusion which is finally reached is independent of the state of opinion at the outset. Investigation, therefore, involves the springing up of new elements of belief in the mind. There is no term which

precisely designates this introduction of something into thought not produced by anything which was in the mind before. We shall do well to reserve the word *sensation* as the name for the element introduced into consciousness. The operation of introduction itself may be termed *mental affection* or less exactly though more expressively an *observation*. But *affection* is not the whole of investigation. It involves also the production of new beliefs out of old ones according to logical laws. This process is the *logical process*, but by an extension of the meaning of a familiar word I call it also *inference*. That inference is an essential part of investigation not merely with us but with any mind that can investigate at all is perhaps not quite clear. I suppose it however to be so; for otherwise the process of investigation would be reduced to a simple exertion of the will and there could be no wrong method and right method. Now the existence of this distinction is what separates investigation from the third method of settling opinion described in the last chapter.

Investigation then necessarily consists of observation and inference. In other words, the conclusion of a rightly conducted investigation ultimately depends entirely on the observations. Now in few cases, if in any, is it necessary that the first products of observation should be the same for all successful investigators of any one question. Mental affections, indeed, cannot in any case be said to produce like sensations independent of inference, for likeness consists in the fact that a certain conception will result from a comparison, and therefore supposes inference. But it is conceivable, perhaps, that no man could reach a certain conclusion except through one determinate series of judgments.

It may seem conceivable that any two investigators must traverse the same path, and that all their successive steps must be the same if their conclusions are to agree. This however is very far from being the fact. We may with equal certainty infer the rotation of the earth from the diurnal motions of the heavenly bodies or from the manner of swinging a pendulum. Nor do observers in different situations or at different times ever observe the same facts. Indeed, it is impossible that two investigators should agree until some steps of inference have been made; for agreement or likeness consists only in the fact that a comparison of two objects will result in a certain conception, so that independent of such a logical act the immediate results of two mental affections have no agreement but are entirely *sui generis*.

The final conclusion of an investigation then depends entirely on

observations and these observations are entirely private and peculiar for each investigator. Yet the conclusions themselves necessarily agree, if the process has been carried far enough. As *fate* in the fairy stories is the inevitability of a certain result, which will surely be brought about however the antecedent conditions may be, so we may say that there is something like this in the fact that two series of sensations without any similarity experienced by two minds will ultimately lead them with perfect certainty to one conclusion.

It is true that we may avoid this strange and paradoxical way of conceiving the fact, since we are clearly warranted by it in saying that there are certain external realities, whose characters do not depend on what we think about them and that these things cause our mental affections from which by the logical process we arrive at a knowledge of how the external things are. Thus, our sensations are as various as our relations to the external things but the cause of the agreement in our final conclusions is the identity in the real external object which through the affections of sense has been the origin of them.

This statement of the matter is entirely justified upon logical principles & perhaps no modern philosopher has questioned it. Yet I entertain no doubt that it is only another form of stating the fact that a fate leads every investigator to a predestined conclusion & not a solution of that paradox by means of any new fact. Could I have the advantage of making use of the principles of logic which are here after to be demonstrated in this book, I do not think I should have much difficulty in convincing the reader that this is so. But without such aid I confess the question is a difficult one to clear up.

The distinction between a reality and a fiction is plain enough. A fiction is something whose character depends on what some mind imagines it to be; a reality is something which is independent of how you or I or any number of persons think about it. The distinction between the external and the internal is also plain. The internal is that whose real existence depends on what I (or you or somebody) think of something. The external is that which so far as it is real is independent not only of what I think about it but also of what I think about anything. But the distinction between what is not merely external to my mind or yours but is absolutely independent of thought and what exists in thought, generally speaking, is I think far from being so clear.

Let us see then how we acquire the conception of the mind for

when we gain this we must be in possession of the distinction in question.

I believe it is a current opinion that we are aware of our own existence from the very moment of birth or earlier; though Kant seems to think we do not realize the fact till we are three or four years old.

As nobody now supposes that there are actually any innate ideas and especially as the existence of any individual mind is a contingent fact, it must be granted that the knowledge of it comes by experience. But many philosophers hold that any thought at once informs us that we exist. To know and to know that I know are one and the same thing, says Hamilton. The "I think" must accompany all my thoughts, says Kant. If Hamilton means to say that no distinction can be drawn between knowing and knowing that one knows it is very easy to refute him. We have seen that the characters of belief are three. First, there is a certain feeling with regard to a proposition. Second, there is a disposition to be satisfied with the proposition. And third, there is a clear impulse to act in certain ways, in consequence. Now there is certainly a distinction between any fact and the fact of my belief in that fact. That fire will burn the fingers and that C. S. Peirce believes that fire will burn the fingers, are distinguishable things, and as distinguishable by me as by any one else. There is nothing self-contradictory, then, in supposing that one of these excites in me a different feeling from the other, and that I should have a satisfaction in the one that I have not in the other. Nor is it inconceivable that I should act in a very decided way when the question actually came up of putting my fingers in the fire, although I was by no means sure that I should be so decided; or I might think I should be decided for example to put my hand in the fire rather than commit some disgraceful act and yet when the moment of choice came, I might find myself undecided or decided the other way. It is perfectly conceivable, therefore, that all three characters of belief should be present in regard to a proposition and yet absent in regard to the proposition "I believe in that proposition"; or *vice versa*. But it is unnecessary that the reader should admit as much as this. It suffices to say that it is conceivable I should believe something and yet not have reflected that it is a belief and not have thought of myself in reference to it, at all. The tendency to act in a certain way implies no thought of self, because even inanimate objects have tendencies to act. Neither does the absence of the irritation of doubt.

The only question is whether the having a sensation does not imply a knowledge of it as a sensation and therefore of myself as sensitive. Most philosophers say no to this, but James Mill and others think that there is no possible distinction between feeling and regarding that feeling as the affection of a sensitive *Ego*; in short they hold that the words feeling and self-consciousness are synonymous. Of course the moment you reflect upon a feeling and retain it in the imagination for examination (which is the method of study pursued by these psychologists) you do regard it as a feeling. But is that the case with the countless impressions of sense which do not attract any particular attention? Is it self-contradictory to say that it is not the case? It is well enough known that a man may have a dyspeptic sensation and yet not for hours refer it to his stomach, it merely having the effect of casting a gloom over his views of things. The moment he considers it separately, he perceives what it is. It seems strange to admit that he can do this and yet to deny that he can for one moment forget the relativity of the objects of his thought, and even to insist that there is no literal signification in the words forgetfulness of self. These very writers who say there is no conceivable distinction between feeling and regarding that feeling as something which belongs to one's self, take great credit to themselves for proclaiming and spreading the doctrine of relativity, which is simply that every object is relative to the mind as an affection of it. Now, if this cannot be overlooked for an instant their doctrine is a mere form of words, like A is A. I don't know that anybody ever attempted to offer any evidence that to feel and to know that one feels are synonymous phrases. In the absence of such evidence we ought to presume that as a distinction appears to be plainly discernible that there really is one just as there is between seeing and knowing that color exists only in the eyes.

But Kant holds that though there is a distinction between cognition with self-consciousness and cognition without self-consciousness, yet the "I think" accompanies all our judgments; or rather (if I remember rightly) that it must *be able to* accompany every judgment. For, he says, if this were not so there would be no separation between the propositions I believe and those which I do not believe. And he goes on to explain how in his opinion the unity of the *ego* accounts for the consistency of facts and the continuity of time and space. He certainly seems to have shown that these things are closely connected together. But it is only necessary for this that there should be a recognized unity in the objects of thought and that there should be

a unity of the ego, but not that I should always refer the one to the other. And this seems to be nearly Kant's own opinion. For he does not, if I understand him rightly, hold that the "I think" of which he speaks is a perception of one's own existence or that it is any knowledge of fact at all, but only that it is a form or point of view from which objects are conceived. To think consistently is one thing, to think about our selves is surely quite another.

No doubt the common opinion among people who have not considered the matter critically is that the mind has a direct experience of its own existence from the moment when it is first conscious of anything. But the only thing of which we can have a direct experience is a sensation. It may be said, however, that what constitutes the existence of the mind is the existence of its sensations, so that the experience of a sensation is the experience of the mind's existence. There is some reason in this, but if it be true, then the knowledge of a sensation as a sensation that is to say the knowledge of it as relative to the mind is not itself a sensation, for clearly if there were but one sensation, no matter what that were, it could not be relative to the mind if the mind is only the system of sensations, for that would be to say it were relative to itself. Thus, to have a sensation is one thing, but to know it as a sensation cannot on this view of the matter be a sensation. Indeed, if we are to say that consciousness or having feeling, or the capacity for feeling, in any way constitutes the existence of the mind,—which has been a very common opinion among philosophers, and to which I myself subscribe,—then to say that the existence of a feeling is relative to the mind can only mean that the whole system of feelings of one mind are connected together and that for a feeling to enter into that system it is necessary that it should produce an effect upon subsequent states of consciousness and that unless it does so it is for that mind no feeling but a perfect non-entity. Now the action of one state of mind upon another which follows it is not direct sensation but is what in another aspect is called inference. So that on this view of the nature of the mind the recognition of a sensation as such is a matter of inferences. This ought to be the view of any person who holds that all existence is relative; that sensations exist relatively to the mind, and the mind relatively to sensations. Yet the point has been overlooked by several metaphysicians of the sensational school.

If, on the other hand, the mind has an absolute existence and is something more than sensation or the capacity for sensations, then

so much the more is the knowledge of it a matter of inference and not given in any direct experience which is allowed on all hands to be mere sensation.

The data for drawing this inference may be present from a very early period of life but we are not so much concerned with the question when it is drawn as with how it is drawn.

Does the general consciousness or feeling of life, that feeling which is strong in waking and vigorous moments and is weak in old age, sickness, and slumber,—does this afford a sufficient ground for inferring one's own existence? To answer this question rightly it is necessary never to lose sight of the fact that a sensation as first presented does not appear as relative but as an absolute thing. The word objective etymologically and in its original use implies the aspect of a sensation as relative. But as the words are now generally used the subjective aspect of a sensation is its aspect as relative to the mind, and its objective aspect is its aspect as an absolute self-existence. A sensation then as first given does not appear in its subjective aspect but as an object. The ambiguity of words has produced great confusion upon this point. It has been said that because the knowledge of relatives is one, therefore, in thinking of color for example as an *object*, that is as a self-existence, external to our bodies, without referring it to sense, we must also be thinking of the *subject-ego!* But in fact color is only thought of as an object in the relative sense when we come to reflect that it is merely an affection of vision. We have proved then a sensation is not in its first presentation, any more than most of them are commonly afterwards, regarded as a sensation. The feeling of life has a certain analogy to light, and like that would at first be objectified—that is, thought of as an absolute existence and not as in us. There is no difficulty in referring the liveliness of waking times to nature, and there is a particular circumstance to aid this conception; namely that we are sleepy when our surroundings are quiet and dark and more lively the more there is going on about us. There is no sensation whatever which cannot be and which is not at first so objectified. We very early learn that hunger is to be referred entirely to ourselves, but there is no reason to doubt that a baby thinks only of milk as good. Every body knows how when other desires first manifest themselves, at the first experience of them the only conception is that of the beauty and perfection of the object, and that it takes a boy or girl a good while to discover that it is only they themselves who are in love. As all sensations are thus first presented to us as

independent existences or as dependent only upon one another, without being referred to ourselves, as the idea of an *ego* must arise first when something is thought of as relative to that *ego*, for we have seen that its existence is only inferred, so that it is known only by what is relative to it, our knowledge of our selves must appear as a *correction* of our original objective view of things. This is a most important point, and I beg the reader's attention to it. The mind is known as we have seen only through sensations, and therefore only in its relations to those sensations. There can, therefore, be no other knowledge of the mind than that which comes by our finding that sensations are relative to something, because that is the only relation which the sensations have to the mind. But as sensations first appear and are interpreted without taking into account this relativity, our knowledge of the mind comes when we find it necessary to apply a correction to our interpretation on that account. Here the term sensation must be held to include everything which is directly known to us by our feelings such as a judgment or imagination, all of which have an existence relative to the mind.

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## Chapter IV. Of Reality

*MS 204: Fall 1872*

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There is, then, a reality or something independent of what you or I or any number of men, may think about it. What is the mode of existence of this reality?

It is a truism to say that what I think depends entirely on what I think it to be. The reality therefore is not *per se* an immediate object of my thought, though my thought may happen to coincide with it. Yet reality must have some intimate connection with what is in the mind or it would be vain for us to hope by following certain rules of reasoning to arrive at the truth.

Investigation, as a method of settling opinion, goes upon the assumption, that every such process, if conducted rightly and carried far enough, will reach one destined conclusion. The process of investigation itself consists necessarily of two parts, one by which a belief is generated from others, which is called *reasoning*; and another by which new elements of belief are brought into the mind, which is called *observation*. Reasoning has been likened to a chain, because while it develops and modifies beliefs, all that it results in depends ultimately on something else, namely, on *observation*. While the final conclusion is one and the same in the minds of all who carry their researches far enough, the observations on which it hangs are for every man private and peculiar. Sometimes the difference of premises is very obvious as when Copernicus infers the rotation of the earth from the general movement of the heavenly bodies, Bradley from the aberration of light, and Fizeau from the manner in which a pendulum swings. But a close scrutiny will show that a difference always exists. No two observers can make the same observation. The observations which I made yesterday are not the same which I make today. Nor are simultaneous observations at different observatories the same, however close together the observatories are placed. Every man's senses are his observatory. Two men cannot therefore make the same observation any more than one man can repeat an observation. We may go further yet and say that two observations are not only not the same, but they are not in themselves in any degree alike. The judgment that they are alike is not contained in either observation (since they do not relate to one another) but is a belief generated by the two beliefs in which the two observations immediately result, and is therefore an inference of reasoning, as that has just been defined. Thus our reasonings begin from the most various premises (otherwise no process of investigation to settle belief would be necessary) and lead ultimately to one conclusion.

The reality must be connected with this chain of reasoning at one or other extremity. According as we place it at one end or the other, we have realism or nominalism.

The reality must be so connected with our thought that it will determine the conclusion of true investigation. But the conclusion depends on the observations. Reality must then be connected with sensation as its cause (or to use another phrase, as its possibility) and this is the nominalistic theory of reality.

But reality is independent of the individual accidental element of

thought. Now on the observation end of the chain of reasoning all is accidental and individual. But at the conclusion end is one result to which alone investigation will ultimately lead. The personal prejudices or other peculiarities of generations of men may postpone indefinitely an agreement in this opinion; but no human will or limitation can make the final result of investigation to be anything else than that which it is destined to be. The reality, then, must be identified with what is thought in the ultimate true opinion. This is the realistic view of reality.

To reconcile these two theories, it may be supposed that entirely independent of all thought there exist such things as we shall think in the final opinion; that these things affect our senses and that the nature of the mind is such that these sensations will at last lead us to the true opinion.

This I take to be the metaphysics most commonly adopted. But the idealists have shown that this is mere words without meaning. What we think when we have an opinion are thoughts. What is meant by the distinction between thoughts which exist independent of all thought and thoughts which do not so exist but only exist as thoughts? This distinction, if it exist, lies in a region wholly out of thought, to which neither our thoughts nor those of any being whatever can penetrate. It follows that there is no idea of any sort in our mind or in any possible mind corresponding to this distinction; it is therefore a distinction in words without any distinction in sense. We may put the argument another way. A conception is said to be true if there exists such a thing independent of all thought. But a thing out of all thought can have no likeness to another, for likeness is the common element that two notions have. Seeing this, some metaphysicians say that a true conception is one which *corresponds to* a thing existing independent of all thought. But nothing is gained by substituting one relation of reason for another; a thing corresponds to another only so far as the mind regards them as correlates. It would be quite beside the purpose to say that a true conception is one which is *produced by* something existing out of thought; for that would be equivalent to saying that a logically inferred notion is true and an illogical one false, thus placing the distinction in what takes place in thought. Every way considered therefore there is a complete vacuity of meaning in saying that independent of all thought there exist such things as we shall think in the final opinion.

All that we can know or conceive of the existence of real things

is involved in two premises; first, that investigation will ultimately lead to a settled opinion, and, second, that this opinion is entirely determined by the observations. The only thing that we can infer is that the observations have such a character that they are fated to lead ultimately to one conclusion. And therefore the only distinctly conceivable sense in which we can say that the objects of the final opinion exist before that opinion is formed is that that existence consists in the fact that the observations will be such as will bring about and maintain that opinion.

Suppose that we were all of us omniscient and knew the full and precise truth about everything. Then the beliefs of all of us would be identical. So much so that the barriers of individuality would be partly broken down. We should have separate minds indeed because while one of us was attending particularly to one thing another might be attending to another, and our desires might to a certain extent centre about ourselves and our surroundings as they do now. Imagine these limitations removed and there would be no respect left in which one man's thought would differ from another's. Mind would cease to be a private belonging. But I won't suppose this but only that we all should know everything. The agreement then in the objects of belief would amount to identity. And these objects would not be fictions but realities. To draw any distinction whatever in that case between the object of belief and the reality would be idle. It would be a distinction without a difference, for any discrepancy between the object believed to exist and the reality is error. This is a simple demonstration that the conception of the reality as it is for itself in contradistinction to the reality as it may be known is a self-contradictory conception. For in the case we have supposed the very reality would be an object of belief,—a thought. The race, the community is perpetually tending toward such a state. It is true we shall never know the true answer to every question, but in regard to any question concerning which there is a doubt, a struggle to rid ourselves from doubt, and an attempt at investigation, we go on the assumption that sufficient research—involving perhaps more experience and reasoning than our race will ever attain to—would produce this state of true belief. If the agreement between belief and reality were perfect the object of belief and the very reality would be completely identical. If then the agreement is partly attained a partial identity is established. That which is believed in, in true knowledge, is real. It appears then that the reality is something with which thought may

be identified and frequently is partially identified, not using the word thought to mean what takes place in the brain but as the object which is brought before us when the act of cerebration takes place.

A host of specious objections to this view will quickly suggest themselves. I cannot refute them or support it, without presuming a clear apprehension of the principles of reasoning which it is the business of logic to elucidate. I will only say that though it seems to me that the principle of the identity of the object of true knowledge with the reality is necessary for the scientific setting forth of the doctrines of logic, yet I do not imagine that those who cannot accept it will have any difficulty in admitting so much of the consequences of it as will be needed for proving the rules of right reasoning. When these have been carefully studied, we may return to this question and I hope then not only to remove the objections to this doctrine but also to fortify it by showing how it serves to bind together and explain these maxims of reasoning themselves.

Let it be granted, then, that the conception of an object which should not only be beyond a given man's thought but beyond all possible thought is an absurdity. In admitting this we do not annul the distinction between a reality and a fiction. If an object is of whatever character I or any man or men will have it to be or imagine it, it is a *fiction*; but if its characters are independent of what you or I or any number of men think about it, it is a *reality*. The object of that final settled opinion to which it is supposed that an investigation will lead, if carried far enough, satisfies this definition of reality; for though the perversity of generations of men may postpone the agreement indefinitely, yet it cannot alter the character of the belief which alone can be permanently established.

But when, to avoid the strangeness of saying that the new elements of belief that spring up in the mind, no matter how we vary them by changing the circumstances of their emergence, will inevitably be such as shall lead us at last to a destined conclusion, we preferred to say that these origins of belief are produced in us by the action of realities upon sense and must therefore be relative to these fixed realities, we have not, according to what has just been urged, stated any additional fact to explain what we found strange but have only stated the strange fact in a more familiar way. For in the one case we have said that the observations are determined by what is to be finally believed in, and in the other case we have said that they are determined by the realities. But it now appears that the object

finally believed in (if investigation is pushed so far) is absolutely identical with the realities.

It is true that the belief is future and may even not ever be attained, while the reality actually exists. But the act of believing is one thing, the object of belief another. Nor need anyone who is familiar with the conceptions of physical science shrink from admitting that the existence of a present reality is in one sense made by a contingent event. Nobody hesitates to say that a leaden weight resting upon a table is really heavy. Yet to say that it is heavy only means that if it be so placed that it is free to move it will approach the earth. The existence of any physical force is nothing but the truth of the fact that if certain conditions shall be complied with certain accelerations will take place. There is a school of natural philosophers well-worthy of that name who considering that matter is nothing apart from its properties and that its properties are nothing but forces, say that matter itself is nothing but the locus of force; so that its existence, also, depends on the fact if something happens something else happens. Thus we find the physicists, the exactest of thinkers, holding in regard to those things which they have studied most exactly, that their existence depends on their manifestations or rather on their manifestability. We have only to extend this conception to all real existence and to hold these two facts to be identical, namely that they exist and that sufficient investigation would lead to a settled belief in them, to have our Idealistic theory of metaphysics. This doctrine is that observation and reasoning are perpetually leading us towards certain opinions and that the fact of such a perpetual tendency is otherwise expressed by saying that the objects of those final opinions have a real existence.

## Chapter IV. Of Reality

*MS 205: Fall 1872*

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Investigation supposes a true and a false, truth and falsity being independent of all opinion upon the matter. The name *real* is applied to that which is independent of how you or I or any number of minds think it to be.

It is a truism to say that the character of what I think depends entirely on what I think it to be. The real is not, therefore, *per se* an immediate object of thought, even though my thought may happen to coincide with it. Yet the real must influence thought or I could not by following any rules of reasoning arrive at any truth.

Investigation consists necessarily of two parts, one by which a belief is generated from other beliefs, which is called *reasoning*; and another by which new elements of belief are brought into the mind, which is called *observation*. Thus, the conclusions depend entirely upon the observations. But while the ultimate conclusion is one and the same in the minds of all who push investigation far enough, the observations on which it hangs are for every man private and peculiar. The observations which I made yesterday are not the same that I make today; nor are simultaneous observations from different situations or with other different circumstances the same. Two men cannot therefore make the same observation. We may go further and say that no two observations are in themselves in any degree alike. The judgment that they are alike is not contained in either observation (since they do not relate to one another) but is a belief generated by the two beliefs in which the two observations immediately result, so that it is an inference of reasoning, as that has just been defined. Thus our reasonings begin with the most various premises, which have not in themselves anything in common, but which so determine our beliefs as to lead us at last to one destined conclusion.

Here is the whole statement of facts from which we must infer

whatever we can know of the mode of being of the real. But there is no additional fact which we can infer from these facts. For these embrace everything which takes place in thought, and as to anything out of thought we can know nothing.

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## Chapter —. The list of Categories

*MS 207: Winter 1872–73*

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In the doctrines which have thus far been developed, are implicitly involved certain conceptions of such universal applicability and such importance in logic, that I propose to consider them especially in this chapter under the name of *Categories*.

In the ideal final opinion which would perfectly represent the reality of things, all possible doubt would be resolved. It follows that the reality is something entirely definite. *Ens est unum*. An object may be conceived to have this character without being real, that is without being in accord with the opinion to which observations are fated to tend, and I shall call this the *being* of things. A griffin is a fabulous animal. That is, a griffin is supposed to be a definite object. You may ask as many questions as you please about a griffin and supply answers according to some rule and if all the questions which could be invented were thus answered, the animal would possess as perfect a being as if it were real, and yet be a mere creature of the imagination.

In every doubt there is one thing fixed and one thing vague; the thing which we doubt something about is fixed, what we doubt about it is vague. These two things must equally be distinguished in the belief in which the doubt is resolved. Consequently, every being has elements which are distinguished from it but which belong to it, in short it has *qualities*.

## On Representations

*MS 212: Winter-Spring 1873*

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A representation is an object which stands for another so that an experience of the former affords us a knowledge of the latter. There are three essential conditions to which every representation must conform. It must in the first place like any other object have qualities independent of its meaning. It is only through a knowledge of these that we acquire any information concerning the object it represents. Thus, the word 'man' as printed, has three letters; these letters have certain shapes, and are black. I term such characters, the material qualities of the representation. In the 2nd place a representation must have a real causal connection with its object. If a weathercock indicates the direction of the wind it is because the wind really turns it round. If the portrait of a man of a past generation tells me how he looked it is because his appearance really determined the appearance of the picture by a train of causation, acting through the mind of the painter. If a prediction is trustworthy it is because those antecedents of which the predicted event is the necessary consequence had a real effect in producing the prediction. In the third place, every representation addresses itself to a mind. It is only in so far as it does this that it is a representation. The idea of the representation itself excites in the mind another idea and in order that it may do this it is necessary that some principle of association between the two ideas should already be established in that mind. These three conditions serve to define the nature of a representation.

Every idea is a representation. It is something set before us, which stands for the real existence which produces it. The perception of redness for example stands to us for the special length of vibration among the particles of the body which excite that sensation. An idea like any other representation has its material qualities or the mere feeling of it in itself and this it is alone which enables us to discrimi-

nate one idea from another. It has also a real causal connection with its object. But that it is like other representations in that it addresses the mind is not so clear. It appears at first sight that it is complete in itself. It cannot so much as have material qualities unless the mind is conscious of it and if the mind is conscious of it that would seem to be enough. I shall endeavor to show that this view of the matter is incorrect. It will be granted without difficulty that every idea does produce another in the mind. Ideas are associated together and the mind thus establishes relations between them. An idea which is present at one moment is conceived as the same as an idea which is present at another moment. I conceive the redness which I perceive now to be the same quality as that which I perceived a moment ago. But since an idea consists only in what is thought at a particular moment it is only what it is thought to be at the moment it is thought. If two ideas are thought at different times they are not in literal strictness the same. When one is present the other is absent and since an idea can only be thought when present the thought of one is not present in that of the other. Thus, the idea of one moment is in no way the same as or similar to the idea of another moment (apart from what the idea suggests to the mind). The same idea cannot therefore be said to exist in different moments, but each idea must be strictly momentary; but a state of mind which does not exist for any space of time however short does not exist at all. For nothing is true of a point of time which is not true of a lapse of time except what is contained in saying it is the ideal limit of an interval. Accordingly an idea which should exist only for one moment, which should never before that have had any existence in the mind in any preceding time however close before and which should never have any existence in any succeeding time no matter how close after would have no existence whatever; and therefore an idea apart from what it represents and suggests to the mind, apart from its calling up to the mind another idea, does not exist in the mind at all. It is therefore an essential property of an idea that it should address itself to the mind at another time. Thus an idea is in the strictest sense a representation and the statement that it is necessary that a representation should excite an idea in the mind different from its own idea is reduced to the statement that a representation is something which produces another representation of the same object and in this second or interpreting representation the 1st representation is represented as representing a certain object. This 2nd representation must

itself have an interpreting representation and so on ad infinitum so that, the whole process of representation never reaches a completion. A representation is such only so far as it is conceived to be one. It is represented as representing a certain object. This object must therefore be indicated in the representation independently of that part of the representation which represents it to exist in a certain way. Or we may express ourselves thus:—There must be connected with any representation of an object another representation which represents that object independently & there must be a representation that the one represents whatever the other represents. When we said that every representation must give rise to & produce another representation of the same object we have said that inference or syllogism is an essential part of the process of representation & when we now say that connected with any representation of an object there is another representation—an independent representation of the same object—& the representation must be represented to represent another representation of the same object, we have said that a proposition or judgment is an essential part of the representation of the object. For as ‘A is B’ is a representation which represents that whatever is represented by the representation A is represented by the representation B, to say that ‘man is mortal’ is to say that whatever thing or word ‘man’ stands for the word ‘mortal’ also stands for. The representation not only has material qualities but it also imputes certain qualities to its object. These we may call its imputed qualities. For example the word ‘white’ printed in a book is itself black so far as its own material qualities are concerned but its imputed quality is white. The logical term or name is a maimed and imperfect representation because it says nothing. It requires to have something added to it in order to make it refer to any particular object as an assertion that that object has such a name applicable to it and is still to begin that endless process of suggestion or inference which constitutes the very essence of representation. The logical proposition ‘it is’ is wanting in this last element but as the mind will generally supply all that by its own action, it is called in grammatical works complete. The proposition is wanting in the reference to an interpreting representation but not in the explicit reference to its object. The logical term is wanting not only in its reference to the interpreting representation but also in its explicit reference to its object. It is as it were merely the representative embodiment of the imputed quality. This is most obvious in adjectives as ‘black’ &c. but

these do not differ in any way from substantive names. ‘Man’ and ‘human’ are synonymous so far as logic is concerned although usage has discriminated between occasions upon which the two points are used. We may distinguish between different kinds of signs according to the relation between their material and imputed qualities. There are some signs whose imputed qualities are derived from & similar to their material ones such as a picture. The colors of a landscape are not it is true the same as those of nature. They do not make a match but they are sufficiently like them to suggest immediately to the mind the appearance intended to be represented. In other cases the connection is a purely conventional one as in the case of most words. In the third class of cases the connection is owing to natural causation. It cannot be said to be merely conventional—still there is no resemblance. If I point my finger to an object in order to distinguish it there is no resemblance between my finger and the object but I imitate the effect of an attractive force applied to the finger which naturally carries the thoughts of the person whom I am addressing towards the object pointed out.

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## On Representations

*MS 213: Winter–Spring 1873*

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A representation is an object which stands for another, so that an experience of the former affords us a knowledge of the latter. Three things are essential to its existence. In the first place, it must like any other object have qualities independent of its meaning. The printed word ‘man’, for example, consists of three letters, which have certain shapes. It is only through an acquaintance with such distinctive characters that we are able to penetrate to the meaning of a sign. I term such qualities the material qualities of the representation, to distinguish them from those imputed qualities which can only be seen by

the mind's eye. The printed word *white* is white as to its imputed quality but is materially speaking black or red according to the color of the ink. In the second place, a true representation must have a real connection with its object. If a weathercock indicates the direction of the wind it is because the wind really turns it round. If the portrait of a man of a past generation shows me how he looked, it is because his appearance really determined the appearance of the picture by a train of causation acting through the mind of the artist. If a prediction is trustworthy, it is because those antecedents from which the predicted event follows as a consequence have had a real effect in giving rise to the prediction. In the third place, we cannot call anything a representation which does not appeal to some mind. The idea of the representing object excites in the mind an idea of the represented object, according to some principle of association already established as a habit of that mind.

But rightly to understand this third property of representations it is requisite to pause for a moment to consider the nature of an idea. An idea is in the first place an object or something set before us. It feels in a certain way which distinguishes it from every other idea. The ideas of red and of blue for example feel differently. Furthermore every idea is connected with some real event, something which takes place in the nerves or brain and in many cases also with some external object. Thus it has the first two of the three properties of representations.

I shall endeavor to show that it has the third property also.

## On the nature of signs

*MS 214: Winter-Spring 1873*

A sign is an object which stands for another to some mind. I propose to describe the characters of a sign. In the first place like any other thing it must have qualities which belong to it whether it be

regarded as a sign or not. Thus a printed word is black, has a certain number of letters and those letters have certain shapes. Such characters of a sign I call its material quality. In the next place a sign must have some real connection with the thing it signifies so that when the object is present or is so as the sign signifies it to be, the sign shall so signify it and otherwise not. What I mean will best be understood by illustration. A weathercock is a sign of the direction of the wind. It would not be so unless the wind made it turn round. There is to be such a physical connection between every sign and its object. Take a painted portrait. It is the sign of the person for whom it is intended. It is a sign of that person in virtue of its likeness to that person: but this is not enough—it cannot be said of any two things that are alike one is a sign of the other but the portrait is a sign of that person because it was painted after that person and represents him. The connection here is an indirect one. The appearance of the person made a certain impression upon the painter's mind and that acted to cause the painter to make such a picture as he did do so that the appearance of the portrait is really an effect of the appearance of the person for whom it was intended. The one caused the other through the medium of the painter's mind. Take any statement which is made concerning a matter of fact. It is caused or determined by the fact. The fact has been observed & the perception of the fact which was caused by it in its turn causes the statement to be made. Perhaps however the fact was not directly perceived. The statement may be a prediction. In that case it cannot be said that that which follows after has caused that which precedes it, the prediction, but if the event has been predicted it has been through some knowledge of its cause and this same cause which precedes the event also precedes some cognition of the mind which gave rise to the prediction so that there is a real causal connection between the sign and the thing signified although it does not consist in one's being the effect of the other but in both being the effect of the same cause. I shall term this character of signs their pure demonstrative application. In the 3rd place it is necessary for a sign to be a sign that it should be regarded as a sign for it is only a sign to that mind which so considers and if it is not a sign to any mind it is not a sign at all. It must be known to the mind first in its material qualities but also in its pure demonstrative application. That mind must conceive it to be connected with its object so that it is possible to reason from the sign to the thing. Let us now see what the appeal of a sign to the mind amounts to. It produces a certain idea in the mind which is the idea that it is

a sign of the thing it signifies and an idea is itself a sign, for an idea is an object and it represents an object. The idea itself has its material quality which is the feeling which there is in thinking. Thus the red and blue are different in the mere sensation. This difference in no way resembles the distinction which there is between those things in the outward world which are called red and those things which are called blue. Those things differ only in the rapidity with which their particles vibrate. In order that the senses discriminate between the two cases it is necessary that there should be some difference in the sensation but it is entirely indifferent so far as the difference of sensation is concerned whether it be a shorter or a longer vibration which produces that peculiar sensation which red things do. Whatever looks red might look blue or vice versa and the representation would be equally true to the facts. Thus our mere sensations are only the material quality of our ideas considered as signs. Our ideas have also a causal connection with the things that they represent without which there would be no real knowledge. It is not so clear at first sight that our ideas resemble their signs in necessarily appealing to some mind. That appeal could amount to nothing except the production of certain other ideas in which the first one should be virtually reproduced and according to the ordinary conception of the mind when the idea has once reached consciousness the correlation is complete. Nevertheless I regard this as an error of a very important character.

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## /On Time and Thought/

*MS 215*

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*March 8. 73*

Every mind which passes from doubt to belief must have ideas which follow after one another in time. Every mind which reasons must have ideas which not only follow after others but are caused by

them. Every mind which is capable of logical criticism of its inferences, must be aware of this determination of its ideas by previous ideas. But is it pre-supposed in the conception of a logical mind, that the temporal succession in its ideas is continuous, and not by discrete steps? A continuum such as we suppose time and space to be, is defined as something any part of which itself has parts of the same kind. So that the point of time or the point of space is nothing but the ideal limit towards which we approach, but which we can never reach in dividing time or space; and consequently nothing is true of a point which is not true of a space or a time. A discrete quantum, on the other hand, has ultimate parts which differ from any other part of the quantum in their absolute separation from one another. If the succession of images in the mind is by discrete steps, time for that mind will be made up of indivisible instants. Any one idea will be absolutely distinguished from every other idea by its being present only in the passing moment. And the same idea can not exist in two different moments, however similar the ideas felt in the two different moments may, for the sake of argument, be allowed to be. Now an idea exists only so far as the mind thinks it; and only when it is present to the mind. An idea therefore has no characters or qualities but what the mind thinks of it at the time when it is present to the mind. It follows from this that if the succession of time were by separate steps, no idea could resemble another; for these ideas if they are distinct, are present to the mind at different times. Therefore at no time when one is present to the mind, is the other present. Consequently the mind never compares them nor thinks them to be alike; and consequently they are not alike; since they are only what they are thought to be at the time when they are present. It may be objected that though the mind does not directly think them to be alike; yet it may think together reproductions of them, and thus think them to be alike. This would be a valid objection were it not necessary, in the first place, in order that one idea should be the representative of another, that it should resemble that idea, which it could only do by means of some representation of it again, and so on to infinity; the link which is to bind the first two together which are to be pronounced alike, never being found. In short the resemblance of ideas implies that some two ideas are to be thought together which are present to the mind at different times. And this never can be, if instants are separated from one another by absolute steps. This conception is therefore to be abandoned, and it must be acknowledged to be already presupposed in

the conception of a logical mind that the flow of time should be continuous. Let us consider then how we are to conceive what is present to the mind. We are accustomed to say that nothing is present but a fleeting instant, a point of time. But this is a wrong view of the matter because a point differs in no respect from a space of time, except that it is the ideal limit which, in the division of time, we never reach. It can not therefore be that it differs from an interval of time in this respect that what is present is only in a fleeting instant, and does not occupy a whole interval of time, unless what is present be an ideal something which can never be reached, and not something real. The true conception is, that ideas which succeed one another during an interval of time, become present to the mind through the successive presence of the ideas which occupy the parts of that time. So that the ideas which are present in each of these parts are more immediately present, or rather less mediately present than those of the whole time. And this division may be carried to any extent. But you never reach an idea which is quite immediately present to the mind, and is not made present by the ideas which occupy the parts of the time that it occupies. Accordingly, it takes time for ideas to be present to the mind. They are present during a time. And they are present by means of the presence of the ideas which are in the parts of that time. Nothing is therefore present to the mind in an instant, but only during a time. The events of a day are less mediately present to the mind than the events of a year; the events of a second less mediately present than the events of a day.

It remains to show that, adopting this conception, the possibility of the resemblance of two ideas becomes intelligible; and that therefore it is not inconceivable that one idea should follow after another, according to a general rule. In the first place, then, it is to be observed that under this conception, two ideas may be both present to the mind during a longer interval, while they are separately present in shorter intervals which make up the longer interval. During this longer interval they are present to the mind as different. They are thought as different. And this longer interval embraces still shorter intervals than those hitherto considered, during which there are ideas which agree in the respects which are defined by each of the two ideas, which are seen to be different. During the longer interval therefore, the ideas of these shortest intervals are thought as partly alike and partly different. There is therefore no difficulty in the conception of the resemblance of ideas. Let us now see what is

necessary in order that ideas should determine one another, and that the mind should be aware that they determine one another. In order that there should be any likeness among ideas, it is necessary that during an interval of time there should be some constant element in thought or feeling. If I imagine something red, it requires a certain time for me to do so. And if the other elements of the image vary during that time, in one part it must be invariable, it must be constantly red. And therefore it is proper to say that the idea of red is present to the mind at every instant. For we are not now saying that an idea is present to the mind in an instant in the objectionable sense which has been referred to above, according to which an instant would differ from an interval of time; but we are only saying that the idea is present at an instant, in the sense that it is present in every part of a certain interval of time; however short that part may be. The first thing that is requisite therefore to a logical mind, is that there should be elements of thought which are present at instants in this sense. The second thing that is requisite is, that what is present one instant should have an effect upon what is present during the lapse of time which follows that instant. This effect can only be a reproduction of a part of what was present at the instant; because what is present at the instant, is present during an interval of time during the whole of which the effect will be present. And therefore since all that is present during this interval is present at each instant, it follows that the effect of what is present at each instant is present at that instant. So that this effect is a part of the idea which produces it. In other words, it is merely a reproduction of a part of that idea. This effect is memory, in its most elementary form. But something more than this is required in order that the conclusion shall be produced from a premise; namely, an effect produced by the succession of one idea upon another.

## [On Time and Thought]

*MS 216: 8 March 1873*

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Any mind which has the power of investigation, and which therefore passes from doubt to belief, must have its ideas follow after one another in time. And if there is to be any distinction of a right and a wrong method of investigation, it must have some control over the process. So that there must be such a thing as the production of one idea from another which was previously in the mind. This is what takes place in reasoning, where the conclusion is brought into the mind by the premises. We may imagine a mind which should reason and never know that it reasoned; never being aware that its conclusion was a conclusion, or was derived from anything which went before. For such a mind there might be a right and a wrong method of thinking; but it could not be aware that there was such a distinction, nor criticise in any degree its own operations. To be capable of logical criticism, the mind must be aware that one idea is determined by another. Now when this happens after the first idea comes the second. There is a process which can only take place in a space of time; but an idea is not present to the mind during a space of time—at least not during a space of time in which this idea is replaced by another; for when the moment of its being present is passed, it is no longer in the mind at all. Therefore, the fact that one idea succeeds another is not a thing which in itself can be present to the mind, any more than the experiences of a whole day or of a year can be said to be present to the mind. It is something which can be lived through; but not be present in any one instant; and therefore, which can not be present to the mind at all; for nothing is present but the passing moment, and what it contains. The only way therefore in which we can be aware of a process of inference, or of any other process, is by its producing some idea in us. Not only therefore is it necessary that one idea should produce another; but it is also requi-

site that a mental process should produce an idea. These three things must be found in every logical mind: First, ideas; second, determinations of ideas by previous ideas; third, determinations of ideas by previous processes. And nothing will be found which does not come under one of these three heads. The determination of one thing by another, implies that the former not only follows after the latter, but follows after it according to a general rule, in consequence of which, every such idea would be followed by such a second one. There can therefore be no determination of one idea by another except so far as ideas can be distributed into classes, or have some resemblances. But how can one idea resemble another? An idea can contain nothing but what is present to the mind in that idea. Two ideas exist at different times; consequently what is present to the mind in one is present only at that time, and is absent at the time when the other idea is present. Literally, therefore, one idea contains nothing of another idea; and in themselves they can have no resemblance. They certainly do not resemble one another except so far as the mind can detect a resemblance; for they exist only in the mind, and are nothing but what they are thought to be. Now when each is present to the mind the other is not in the mind at all. No reference to it is in the mind, and no idea of it is in the mind. Neither idea therefore when it is in the mind, is thought to resemble the other which is not present in the mind. And an idea can not be thought, except when it is present in the mind. And, therefore, one idea can not be thought to resemble another, strictly speaking. In order to escape from this paradox, let us see how we have been led into it. Causation supposes a general rule, and therefore similarity. Now so long as we suppose that what is present to the mind at one time is absolutely distinct from what is present to the mind at another time, our ideas are absolutely individual, and without any similarity. It is necessary, therefore, that we should conceive a process as present to the mind. And this process consists of parts existing at different times and absolutely distinct. And during the time that one part is in the mind, the other is not in the mind. To unite them, we have to suppose that there is a consciousness running through the time. So that of the succession of ideas which occur in a second of time, there is but one consciousness, and of the succession of ideas which occurs in a minute of time there is another consciousness, and so on, perhaps indefinitely. So that there may be a consciousness of the events that happened in a whole day or a whole life time. According to this, two parts

of a process separated in time—though they are absolutely separate, in so far as there is a consciousness of the one, from which the other is entirely excluded—are yet so far not separate, that there is a more general consciousness of the two together. This conception of consciousness is something which takes up time. It seems forced upon us to escape the contradictions which we have just encountered. And if consciousness has a duration, then there is no such thing as an instantaneous consciousness; but all consciousness relates to a process. And no thought, however simple, is at any instant present to the mind in its entirety, but it is something which we live through or experience as we do the events of a day. And as the experiences of a day are made up of the experiences of shorter spaces of time so any thought whatever is made up of more special thoughts which in their turn are themselves made up by others and so on indefinitely. It may indeed very likely be that there is some minimum space of time within which in some sense only an indivisible thought can exist and as we know nothing of such a fact at present we may content ourselves with the simpler conception of an indefinite continuity in consciousness. It will easily be seen that when this conception is once grasped the process of the determination of one idea by another becomes explicable. What is present to the mind during the whole of an interval of time is something generally consisting of what there was in common in what was present to the mind during the parts of that interval. And this may be the same with what is present to the mind during any interval of time; or if not the same, at least similar—that is, the two may be such that they have much in common. These two thoughts which are similar may be followed by others that are similar and according to a general law by which every thought similar to either of these is followed by another similar to those by which they are followed. If a succession of thoughts have any thing in common this may belong to every part of these thoughts however minute, and therefore it may be said to be present at every instant. This element of consciousness which belongs to a whole only so far as it belongs to its parts is termed the matter of thought. There is besides this a causation running through our consciousness by which the thought of any one moment determines the thought of the next moment no matter how minute these moments may be. And this causation is necessarily of the nature of a reproduction; because if a thought of a certain kind continues for a certain length of time as it must do to come into consciousness the immediate effect produced

by this causality must also be present during the whole time, so that it is a part of that thought. Therefore when this thought ceases, that which continues after it by virtue of this action is a part of the thought itself. In addition to this there must be an effect produced by the following of one idea after a different idea otherwise there would be no process of inference except that of the reproduction of the premises.

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## Chap. 5th

*MS 217: 1873*

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March 10

We have seen that an inference is the process by which one belief determines another. But a belief is itself a habit of the mind by virtue of which one idea gives rise to another. When I say that I know the French language, I do not mean that as long as I know it I have all the words which compose it in my mind, or a single one of them. But only that when I think of an object, the French word for it will occur to me, and that when a French word is brought to my attention I shall think of the object it signifies. What is true of knowledge is equally true of belief, since the truth or falsehood of the cognition does not alter its character in this respect. I believe that prussic acid is poison, and always have believed it. This does not mean that I have always had the idea of prussic acid in my mind, but only that on the proper occasion, on thinking of drinking it, for example, the idea of poison and all the other ideas that that idea would bring up, would arise in my mind. Thus there are three elements of cognition; thoughts, the habitual connection between thoughts, and processes establishing a habitual connection between thoughts. We have seen already that an idea can not be instantaneously present, that consciousness occupies

time, and that we have no consciousness in an instant. So that at no time have we a thought. But now it further appears that in reference to a belief not only can we not have it in an instant, but it can not be present to the mind in any period of time. It does not consist in anything which is present to the mind, but in an habitual connection among the things which are successively present. That is to say, it consists in ideas succeeding one another according to a general rule; but not in the mere thinking of this general rule, nor in the mere succession of ideas one upon another, nor in both together. A thought must therefore be a sign of a belief; but is never the belief itself. The same thing is obviously true in regard to an inference; and even a simple idea is of intellectual value to us not for what it is in itself but as standing for some object to which it relates. Now a thing which stands for another thing is a representation or sign. So that it appears that every species of actual cognition is of the nature of a sign. It will be found highly advantageous to consider the subject from this point of view, because many general properties of signs can be discovered by a set of words and the like which are free from the intricacies which perplex us in the direct study of thought. Let us examine some of the characters of signs in general. A sign must in the first place have some qualities in itself which serve to distinguish it, a word must have a peculiar sound different from the sound of another word; but it makes no difference what the sound is, so long as it is something distinguishable. In the next place, a sign must have a real physical connection with the thing it signifies so as to be affected by that thing. A weather-cock, which is a sign of the direction of the wind, must really turn with the wind. This word in this connection is an indirect one; but unless there be some way or other which shall connect words with the things they signify, and shall ensure their correspondence with them, they have no value as signs of those things. Whatever has these two characters is fit to become a sign. It is at least a symptom, but it is not actually a sign unless it is used as such; that is unless it is interpreted to thought and addresses itself to some mind. As thought is itself a sign we may express this by saying that the sign must be interpreted as another sign. Let us see however, whether this is true of thought itself that it must address itself to some other thought. There are some cases in which it is not difficult to see that this must be the case. I have no belief that prussic acid is poisonous unless when the particular occasion comes up I am led to the further belief that that particular acid is poisonous; and unless I am further led to the belief that it is a thing to avoid drinking. For all these things

are necessary to my acting on my belief. A belief which will not be acted on ceases to be a belief. It may be that I shall finally come to a belief which is a motive for action directly without the intervention of a more special belief. In this case how does the belief address itself to a sign? When a person is said to act upon a certain belief the meaning is that his actions have a certain consistency; that is to say, that they possess a certain intellectual unity. But this implies that they are interpreted in the light of thought. So that even if a belief is a direct motive to action it still is a belief only because that action is interpretable again. And thus the intellectual character of beliefs at least are dependent upon the capability of the endless translation of sign into sign. An inference translates itself directly into a belief. A thought which is not capable of affecting belief in any way, obviously has no signification or intellectual value at all. If it does affect belief it is then translated from one sign to another as the belief itself is interpreted. And therefore this character of signs that they must be capable of interpretation in every sense belongs to every kind of cognition. And consequently no cognition is such or has an intellectual significance for what it is in itself, but only for what it is in its effects upon other thoughts. And the existence of a cognition is not something actual, but consists in the fact that under certain circumstances some other cognition will arise.

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## Chap. 6th

*MS 218**March 10 73.*

We have seen that a cognition is a sign, and that every sign has these three elements: First, the qualities which belong to it in itself as an object; second, the character of addressing itself to a mind; and thirdly, a causal connection with the thing it signifies. In the Fourth

Chapter we have seen what the general qualities of cognitions in themselves are. In the last chapter we have considered how they address themselves to other cognitions. We have now to consider what is the nature of the causal connection between a thought and the thing to which it relates. The whole effort in investigation is to make our beliefs represent the realities. What is a reality and how is it connected with thought? A reality is distinguished from a figment in that a figment is whatever we think it to be, while a reality is what it is whatever we may think it to be. Realities are either in the mind or out of the mind; for a thought is in itself a reality; something in the mind. An external reality is something which not only is what it is whatever we may think it to be, but also is independent of what we think about other things. An internal reality has characters dependent on our thoughts, although its characters are not changed by our thinking that it has a different set of characters from what we have hitherto supposed that it had. Thus if I really have the idea of a red object, that idea is an internal reality; its character depends upon what was in my mind at the time I had it. But if I make a false analysis, and think that what I thought consists in thinking of a certain rate of vibrations, that does not alter the fact that I was not really thinking of that rate of vibrations. A reality is that which is represented in the truth, and the truth is that which every judgment aims at. But we have seen that investigation and reasoning is but a particular case of that struggle which is occasioned by the irritation of doubt, and that the final cause of that struggle, is nothing but a settled belief; and consequently we aim at nothing in investigation but to reach the final settled belief. It follows from this that the reality is nothing but what is represented in the final belief. The question is, whether this is owing to some peculiar faculty of ours by which our final belief will always represent the reality which is independent of it, or whether it is owing not to any particular effect of this kind, but merely to the reality and the object of final belief, (two different modes of expressing the same thing,) that is to their being identical, not merely in our cases constituted as we are, but to a necessary identity between them, independent of any peculiarity in the constitution of the mind. Now, in the first place, the fact that the end of the struggle occasioned by the irritation of doubt is a settled belief, is not anything inferred from a particular law of the mind, but is only stating that two forms of expression are equivalent in meaning. It is only a means of distinctly expressing what we mean by saying that a struggle is occa-

sioned by the irritation of doubt. Nor is there any matter of fact involved in saying that the truth is the object aimed at in investigation; for investigation implies that the conception of truth is developed, and it is absurd to suppose a mind which should say this is the truth but I do not believe it. Every mind therefore will believe the truth as soon as it finds it out, and therefore that is the end of investigation with any mind. So that the object of a final settled opinion not merely coincides with the truth, but is the truth by the definition of words. The truth is independent of what we may think about it and the object of an opinion is a creation of thought which is entirely dependent on what that opinion is. It exists by virtue of that opinion. There seems to be a contradiction here. But the secret of the matter is this. The final settled opinion is not any particular cognition, in such and such a mind, at such and such a time, although an individual opinion may chance to coincide with it. If an opinion coincides with the final settled opinion, it is because the general current of investigation will not affect it. The object of that individual opinion is whatever is thought at that time. But if anything else than that one thing is thought, the object of that opinion changes and it thereby ceases to coincide with the object of the final opinion which does not change. The perversity or ignorance of mankind may make this thing or that to be held for true, for any number of generations, but it can not affect what would be the result of sufficient experience and reasoning. And this it is which is meant by the final settled opinion. This therefore is no particular opinion but is entirely independent of what you, I, or any number of men may think about it; and therefore it directly satisfies the definition of reality. But the object of the final opinion is something which is capable of being thought, and does not transcend thought altogether, and therefore the reality is something which is capable of being thought, and in no case can transcend thought altogether. It follows from our reasoning, that this is not only a fact but is involved in the meanings of words. The reality or what exists is the most general of expressions; for even a figment is a reality, as we have seen, when it is considered as something in itself, and not as representing something else. What is meant by the word existing, therefore, is that wherein all objects agree. But all the objects which could be used to form such a general observation agree in being objects of thought. And consequently, what exists must itself be an object of thought. That is to say thought must be implied as a part of the meaning of the word. We can have no conception of

anything which is not an object of thought; and a word to which no conception attaches, has no meaning at all. Consequently an attempt to find a word which shall express a thing that exists without implying that that thing is a possible object of thought, will result simply in a meaningless or contradictory expression. Let us define a thing in itself which is not a possible object of thought. The "is" is a word which means an object of thought. So that a thing in itself means an object of thought that is not an object of thought. Consider the matter in a less technical way. Let us suppose a mind placed in a universe, part of the objects in which could be objects of its thought, and part not. That mind would be entirely cut off from the latter part of this universe. It could have no idea of that whatever, direct or indirect, positive or negative. It could have no idea of being except as one of the things of the first part of that universe. The general idea of being it could not have, and the word which expresses it would be no word for that mind, or else would be used in a narrower sense. We, who are outside of that mind, may talk of objects that can not possibly be thought by it, but it could not use such an expression and attach any meaning to it. In the same way, it is clear, that we can no more transcend the limits of our mind, than that being could transcend its limits; and that that which can not possibly be thought by us is simply nonsensical expression. Let us consider the causal connection between the object of cognition and the cognition itself. The reality has an effect on our thought and therefore exists before that thought. But the object of the final opinion is contingent upon the future event. Thus the existence of something in the present depends upon the future conditional occurrence of a certain event. This may sound strange but the strangeness will disappear upon considering the numerous familiar instances of the same sort. A diamond is really hard. Its hardness is a quality which it possesses all the time. And now what does this hardness consist in? It consists in nothing else but this, that rock crystal will not scratch it. If no attempt has been made, as yet, to scratch it with rock crystal, its present hardness consists entirely in what will happen in the future. The inkstand upon my table is heavy. And in what does its weight consist? It consists in the fact that if taken off the table and let fall it will drop. Its weight then, which exists all the time, consists in what will happen if it is taken off the table and let fall. What makes its weight consists only in the fact that it will always drop on any future occasion; and that, although its having weight is the cause of its

falling. These conceptions are perfectly familiar to anybody who has considered the subject of forces. Every force resident in a body consists only in the fact that certain phenomena will occur under certain conditions. Matter is only the center of forces. Its existence consists only in the fact that attractions and repulsions center in it. And these attractions and repulsions themselves exist in what will happen under certain conditions. So that the existence of matter itself is of the same sort. We have already seen that all cognitions exist only in the effect they would have upon future cognitions. It is therefore but a small step to assert that all reality exists only by virtue of what will happen under certain conditions in the future. In this conception of the case all the laws and regularities of nature are resolved into a historic necessity in the process of investigation by which a certain conclusion is brought out. The final results of investigation are not in any degree determined by our opinions at the outset but are, as it were, predestinated. The method we pursue or the action of our will, may hasten or retard the time when this conclusion is reached; but it is fated to emerge at last. And every cognition ]? [ consists in what investigation is destined to result in.

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## Memorandum: Probable Subjects to be treated of

*MS 220: Between 11 and 14 March 1873*

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- Chap. 7. Of Logic as a Study of Signs.
- "    8. " Three Classes of Qualities.
- "    9. " Space as Essential in Logic.
- "    10. " the Copula and Simple Syllogism.
- "    11. " Logical Breadth and Depth, and Distribution and Composition.

- Chap. 12. Of the Collective Senses of Terms and of Number.  
 " 13. " the Mathematical Method of Reasoning.  
 " 14. " Relative Terms.  
 " 15. " Conjugative Terms.  
 " 16. " Probabilities.  
 " 17. " Maxims of Reasoning.
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## Chap. 7. Of Logic as a Study of Signs

*MS 221*

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March 14 73

A sign is something which stands for another thing to a mind. To its existence as such three things are requisite.

In the first place, it must have characters which shall enable us to distinguish it from other objects.

In the second place, it must be affected in some way by the object which it signifies, or at least something about it must vary as a consequence of a real causation with some variation of its object. One of the simplest examples of this is a weathercock, which is directly moved by the force of the wind. A photograph is caused by a radiant light from the object it represents. In the case of a picture executed by hand the causation is less direct, but none the less exists. The relation of a historical statement with its object is that of being caused by it. If a promise is made, this is a sign of the thing promised only so far as it will itself cause the existence of its being, unless we are to regard it as a prophecy which is caused by that state of mind which will cause the thing prophesied to be carried out. Thus the causation may either be from the object to the sign, or from the sign to the object, or from some third thing to both; but some causation there must be.

The third condition of the existence of a sign is that it shall address itself to the mind. It is not enough that it should be in relation to its object but it is necessary that it shall have such a relation to its object as will bring the mind into a certain relation with that object namely, that of knowing it. In other words, it must not only be in relation with its object, but must be regarded by the mind as having that relation. It may address the mind directly, or through a translation into other signs. In some way it must be capable of interpretation. We have seen that thoughts themselves have intellectual significance only so far as they prove themselves to other thoughts. So that thoughts are themselves signs which stand for other objects of thought. And since, on the other hand, there is no sign which the mind may not make use of in reasoning, it follows that the science of thought in its intellectual significance is one and the same thing with the science of the laws of signs. Now there are many general truths with regard to signs which hold good for all signs whatever, of necessity; being involved in the essential nature of signs. The origin of these principles is undoubtedly the nature of the mind. But they are involved in so much of what is true of the mind as is implied in our capability of reasoning at all and which may therefore be said to be implicitly taken for granted by all men, that is, to be deducible from what everybody agrees to and must agree to before we can begin any discussion whatever in a rational way, and which is thus taken out of the special domain of psychology and made the common property of science. These principles might be evolved from a study of the mind and of thought, but they can also be reached by the simple consideration of any signs we please. Now the latter mode of studying them is much the easiest, because the examination of external signs is one of the most simple researches which we can undertake, and least susceptible to error, while the study of the mind is one of the most difficult and doubtful. We shall therefore proceed in the remainder of this part of the work to compare signs, and generalize our results, being guided in doing so by a certain feeling of the necessity that this or that must be true, such as is felt in mathematics the origin of which necessity clearly is, in this case at least, that the principles are involved in the postulate, that the mind is so constituted as to investigate.

The business of Algebra in its most general signification is to exhibit the manner of tracing the consequences of supposing that certain signs are subject to certain laws. And it is therefore to be regarded as a part of Logic. Algebraic symbols have been made use

of by all logicians from the time of Aristotle, and probably earlier. Of late, certain logicians of some popular repute, but who represent less than any other school the logic of modern science, have objected that Algebra is exclusively the science of quantity, and is therefore entirely inapplicable to Logic. This argument is not so weak that I am astonished at these writers making use of it, but it is open to three objections: In the first place, Algebra is not a science of quantity exclusively, as every mathematician knows; in the second place these writers themselves hold that logic is a science of quantity; and in the third place, they, themselves, make a very copious use of algebraic symbols in Logic.

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## Chap. 9th

MS 223

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March 15, 73.

The object denoted by any sign whatever is more or less indeterminate. This indeterminacy is different from ambiguity. A sign is ambiguous if it is doubtful what it is applicable to and what it is inapplicable to, but the indeterminacy here spoken of merely consists in its being applicable to more than one possible object; or, in other words, what is meant is that a distinction can be drawn in the object of any sign, whatever, no matter how particular it may be. Take the term "Philip of Macedon." We may distinguish between Philip drunk and Philip sober, and so in every case. The origin of this universal generality of signs is to be found in the fact that an idea occupies time. An individual term is defined as one mono modo determinator, that is to say which in regard to every quality or character, implies either that its object has it or has it not. There actually are no individual terms, but nevertheless the conception of an indi-

vidual term like that of an infinitesimal quantity in mathematics is of the highest importance in Logic. Every proposition expresses a relation between the objects of two terms and may be put into such a form as to signify that whatever is denoted by one term is denoted by another. For example, "A loves B," may be put into the form whatever is denoted by the term A is denoted also, "Lover of B." The proposition "Every man loves himself," may be put into the form, whatever is denoted by the term man, is denoted by the term, "Self-lover." The proposition, "If it lightens it will thunder," can be put into the form, whatever is denoted by the term "The state of things in which it lightens," is also denoted by the term, "The state of things in which it thunders," or, "The state of things which will be followed by thunder." This is the form in which the logician considers every proposition to be stated. The first term is called the subject of the proposition the second term the predicate of the proposition. The subject is written first, and the predicate after it; and between is placed a sign which means that whatever is denoted by the subject is denoted by the predicate. The sign used for this purpose is called the copula; the word "is," is the one usually adopted. I have proposed the following sign  $\prec$  for this purpose and shall occasionally make use of it in this book.<sup>1</sup> Let us now consider, for a moment, the logical properties of the copula. The relation which it declares to exist between every object denoted by the subject and corresponding objects denoted by the predicate is that relation which every individual bears to itself, that is identity. From this definition of it it is easy to deduce that  $a$  and  $b$  being any terms whatever, to say that  $a$  is  $b$  is neither more nor less than it is to say that whatever is  $a$  is  $b$ . For reasons which will be explained further on, I shall term this principle the transitive equiparance of the copula. This principle does not altogether consist of the properties of the copula, although it has been sometimes expressly, and always, I think, tacitly, assumed by logicians to do so. That is to say, they have given no forms of reasoning which require any more to justify them than what is contained in this principle. This principle obviously divides itself into two. The first is, that if  $a$  is  $b$  then whatever is  $a$  is  $b$ . This I should call the transitive property of the copula. A form of enunciating it is commonly known as the *dictum de omni*. It is the leading principle of

1. I call the attention of Algebraists to the utility of this symbol. According to it we may write, for example,  $\neg\vee\prec\vee$ .

deductive syllogism, because, according to it, if  $a$  is  $b$ , and anything whatever, as  $X$ , is  $a$ , then  $X$  is  $b$ . The other part of the principle of the transitive equiparance of the copula is, that if whatever is  $a$  is  $b$  then  $a$  is  $b$ . Some logicians consider this as the leading principle of induction, because it justifies us in reasoning thus: The XYZ etc. are whatever is  $a$ , XYZ etc. are  $b$ , hence  $a$  is  $b$ . But other logicians have objected that this is not properly an induction. Because in induction we are not able to say that XYZ etc. are the only cases of  $a$ . And this seems a well founded objection. This principle may be put in another form; for from the proposition that whatever is  $a$  is  $b$ , together with the proposition that  $c$  is  $a$ , we may by the *dictum de omni* infer that  $c$  is  $b$ . If for  $c$  we put  $a$  we shall have this form of inference: Whatever is  $a$  is  $b$ ,  $a$  is something that is  $a$ , hence  $a$  is  $b$ . That is to say the *dictum de omni* enables us to deduce this principle; that if whatever is  $a$  is  $b$  then  $a$  is  $b$ , from the proposition that  $a$  is something that is  $a$ . So that this principle contains nothing that is not contained in the *dictum de omni*, except the principle that  $a$  is  $a$ . This principle is commonly known as the principle of identity, and the property that it expresses I shall term the equiparance of the copula. We may imagine a term such that it can be predicated by every other. Logicians name this *ens* or being. It may conveniently be represented by the figure 1. Anything you please, real or imaginary, is *ens* or *being* in this wide sense. We may also imagine a term of which any other may be predicated, and as there is nothing of which everything can be predicated, for nothing is both white and black, this term has the name nothing, and may appropriately be represented by the figure 0. According to this, we allow ourselves to say anything we please of that which does not exist, and there is clearly no falsity in making any assertion we please in regard to it, since we are not making a false statement about anything at all; or, to state the matter in another way, the logical formula,  $a$  is  $b$ , means that whatever  $a$  there may be in existence is  $b$ . Now there is clearly no falsity in saying, for example, that whatever griffin there may be in existence breathes fire. There can be no falsity in it because the statement is limited to such griffins as exist, and no griffins exist, so that there is no statement in it at all. It is only the form of a statement without the substance.

Let us consider a term which is so related to two others as to denote whatever is denoted by either of these and nothing else. If the other two terms are  $a$  and  $b$  this term may be written " $a$  or  $b$ "

or, " $a + b$ ." The properties of such a disjunctive term are three: First, that  $a$  is  $a$  or  $b$  or in symbols  $a \prec a + b$ ; second, that  $b$  is  $a$  or  $b$ , or,  $b \prec a + b$ ; and third, that if  $c$  is  $a$  and  $c$  is  $b$  then  $c$  is  $a$  or  $b$ , or in symbols,  $c \prec a$  and  $c \prec b$  then  $c \prec a + b$ . These properties include all those which belong to disjunctive terms. One or two corollaries may however be mentioned.  $a$  or  $b$  is precisely equivalent to  $b$  or  $a$ ; this is the commutative property of disjunction.  $a$ , or that which is  $b$  or  $c$ , in symbols,  $a \doteq (b + c)$ , is identically the same as that which is  $a$  or  $b$  or else  $c$  or, in symbols  $(a + b) + c$ . This is called the associative property of disjunction. What is either " $a$  or  $a$ " is  $a$ . Considering the terms being and nothing, in reference to disjunction, we observe that since nothing is  $a$ , what is either  $a$  or nothing is  $a$ ; and since  $a$  is ens, what is either  $a$  or ens is ens.

Let us now consider a term which is so as to denote everything which is denoted by both of these terms in common and nothing else. If  $a$  and  $b$  are the two other terms we may denote this term by the expression " $a$  and  $b$ ," or in symbols,  $a,b$ . The properties of this term may be summed up in three propositions: First, that what is both  $a$  and  $b$  is  $a$ ; second, that what is both  $a$  and  $b$  is  $b$ ; and third, that if  $c$  is  $a$  and  $c$  is  $b$ , then  $c$  is both  $a$  and  $b$ . It will be observed that the properties of such a conjunctive term are precisely parallel to those of the disjunctive term in such a way that from any conception of a property of a disjunctive term we may obtain a corresponding property of a conjunctive term by substituting the conjunctive in place of the disjunctive, and at the same time changing all predicates to subjects, and *vice versa*. Thus, corresponding to the property that  $a$  is either  $a$  or  $b$ , we have the property that what is both  $a$  and  $b$  is  $a$ ; corresponding to the property that  $b$  is either  $a$  or  $b$ , we have the property that whatever is both  $a$  and  $b$  is  $b$ ; and corresponding to the property that if  $a$  is  $c$  and  $b$  is  $c$  then whatever is either  $a$  or  $b$  is  $c$  we have the property that if  $c$  is  $a$  and  $c$  is  $b$  then  $c$  is both  $a$  and  $b$ . And the corollaries which may be deduced from these necessarily have a similar relation to one another just as the term "either  $a$  or  $b$ " is equivalent to the term "either  $b$  or  $a$ ," so the term, "both  $a$  and  $b$ " is equivalent to the term, "both  $b$  and  $a$ "; and as  $(a + b) + c$  is equivalent to  $a + (b + c)$ , so  $(a,b),c$  is equivalent to  $a,(b,c)$ . The properties relating to zero and nothing have still another sort of parallelism, namely that of changing subjects for predicates, and disjunctives for conjunctives may also interchange the terms being and nothing. Thus corresponding to the proposition that whatever is

either  $a$  or nothing is  $a$ , we have the proposition that  $a$  is both  $a$  and ens, and corresponding to the proposition, that whatever is either  $a$  or ens is ens, we have the proposition that nothing is both  $a$  and nothing. Finally there are certain corollaries which may be deduced from the combination of the fundamental properties of disjunction and of conjunction; these are: First, that the following term, that which has at once  $a$  and either  $b$  or  $c$  is the exact equivalent of the following term, that which has either both  $a$  and  $b$  or both  $a$  and  $c$ ; and corresponding to this we find that the following term that which is either  $a$  or both  $b$  and  $c$  is the equivalent of this term, that which is at once either  $a$  or  $b$  is either  $a$  or  $c$ .

The greater part of what is commonly known as formal logic is deducible from the principles set forth in this chapter, and it will therefore be worth our while to consider their consequences a little further. Any general term may be conceived as separated by disjunctions into an indefinite number of individual terms. We may write, for example,  $a \prec A + B + C + D +$ , etc., and we may also conceive of a general term as separated in like manner by successive conjunctions into an indefinite number of elementary terms of each of which nothing is predicable but itself. This is an ideal mode of conceiving a term as built up, because no individual term and no such elementary term can be found in fact; moreover if such elementary terms could be found, it would require an infinite number of them to form a general term. We might easily be led therefore, into fallacies in allowing ourselves to consider the subject from this point of view, if that we were not on our guard against the absurdity contained in the hypothesis. Similar absurdities are involved in the mathematical conception of infinitesimals but as in that case so in this, there is involved with the fiction a certain truth which can not be stated otherwise in a form so convenient for certain purposes. If two terms  $a$  and  $b$  differ so that we may write  $a$  is  $b$ , but not  $b$  is  $a$ , as for example, we may say that any man is an animal but not that every animal is a man, then the term of which the other may be predicated is said to have less breadth than the latter; meaning by that that there are fewer terms of which it can be predicated while that the other term is said to have less depth, meaning by that that there are fewer terms which can not be predicated of it. It is obvious, and requires no proof that of the two terms the one which has the greater breadth has the less depth and *vice versa*. These words, "breadth" and "depth," together with others which they naturally suggest, will be found extremely convenient

and are, many of them, in common use. Thus a man is said to be profoundly learned, and have wide information. The former implies that he knows much about certain subjects; the latter, that he is acquainted with many subjects. It is clear that a man's learning may be profound, but in a narrow range; wide, but superficial, or shallow. Other terms which have lately come into use are, "denote" and "connote." A term is said to denote all those objects of which it may be predicated, and to connote all those qualities which may be predicated of it. I do not know that the selection of these terms is very happy. They were used in a different way in the middle ages and their present use is the result of ignorance, but it may perhaps be considered as established. What is now called denoting was in the twelfth century called naming, and what is now called connoting was then called signifying. And these terms might be used now without any departure from usage. It is useful to consider the known breadth and depth of a term in different states of our knowledge. In any one state of our knowledge every term is known to be predicable of certain others, and have certain others predicable of it. And of two terms the one which has all those things predicable of it which are predicable of the other and more beside, is itself predicable of only a part of these terms of which the other is predicable, and *vice versa*. The effect of an addition to our knowledge is to make one term predicable of another which was not so before to our knowledge. And it thus at once increases the known depth of the subject term, and the known breadth of the predicate term, without any decrease of either of these qualities so that in the increase of knowledge the known breadth and depth of terms are constantly increasing and the sum of the breadths and depths on either product, if you please, will measure the extent to which investigation has been pushed. Let us now look for a moment at the differences in their effects upon the known breadths and depths of the terms of the different classes of reasoning.

## Chap. VIII. Of the Copula

*MS 229: Spring 1873*

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Every idea occupies time. In the time in which one is thought another may be thought. In this case the latter is thought as a special case of the former. This is called the *subsumption* of the latter under the former.

Every judgment expresses a relation of ideas, and consequently involves a comparison of them, and a thinking of them together. Every thinking of ideas together is a process of subsumption. Consequently every proposition or expression of a judgment, may be put in the form "A is a case under B." Thus, when we say "every man is an animal," we say that man is included under animal as a special case of it. I shall use the symbol  $\prec$  to declare that the object of the term written before it is included under the object of the term written after it. Thus " $\text{man} \prec \text{mortal}$ " will mean "man is mortal," and " $\neg\sqrt{\cdot} \prec \sqrt{\cdot}$ " will mean that the negative of a square root is itself a square root of the same quantity. The symbol  $\prec$  or "is," its equivalent in words, is termed the copula by logicians. The term which precedes the copula is called the subject of the proposition, & that which follows it the *predicate*. The latter is said to be predicated of the former.

The student of logic needs to make himself expert in putting propositions into the canonical form  $a \prec b$ . The following are a few typical examples:—

"The soul is not mortal" = "Soul  $\prec$  immortal."

"Fishes swim" = "Fish  $\prec$  thing that swims."

"Cats kill mice" = "Cat  $\prec$  killer of mice."

"Every man loves himself" = "Man  $\prec$  self-lover."

"If it rains it is cloudy" = "What exists only if it  
rains  $\prec$  what exists only if it is cloudy."

"Some men are black" = "Whatever exists  $\prec$  what exists  
only in states of things in which black men exist."

All the properties of the copula may be summed up in three propositions. They are these.

1st Anything is itself, or  $a \prec a$ . This is called the *principle of identity*.

2nd If  $a$  is  $b$  then whatever is  $a$  is  $b$ . This is called the *dictum de omni*. This is as much as to say that we can reason thus:—

$$\begin{aligned} a &\prec b \\ x &\prec a \\ \textit{Ergo} \quad x &\prec b. \end{aligned}$$

This form of argument is called *Barbara*.

Expertness in reducing arguments to this form is indispensable to the logician. Examples for practice are given in the appendix.

These two properties belong to various other relations besides that expressed by the copula. The only thing which distinguishes this from those is 3rd. If  $a$  and  $b$  have the same predicates (in true propositions) then there is no difference between  $a$  and  $b$ , so far as the objects they name are concerned. I shall term this the principle of the *singleness of the same*.

If the first two properties belong to any relation that is if anything to which the relation is applicable at all is in that relation with itself, and if what is in that relation to something else which is in that relation to a third is itself in the same relation to the third, I term it a relation of *containing*.

An example of such a relation is being as small as. For everything is as small as itself, and if  $a$  is as small as  $b$  and  $b$  as small as  $c$  then  $a$  is as small as  $c$ .

Another such relation is the converse of that expressed by the copula or that which  $b$  has to  $a$  if  $a \prec b$ .

Another is the following of one proposition from another. For it is universally true that "If  $A$  then  $A$ " and also that we can reason

$$\begin{aligned} \text{If } A \text{ then } B \\ \text{If } X \text{ then } A \\ \textit{Ergo} \quad \text{If } X \text{ then } B. \end{aligned}$$

Now we may, if we choose, express any such relation by a sign similar to the copula,—say by the sign  $\prec$  with accents as  $\prec'$ ,  $\prec''$ , etc. —and then if we will only make the third property of the copula hold good by neglecting all differences between objects except such as

subsist between  $a$  and  $b$  if  $a \prec' x$  is true while  $b \prec' x$  is not true, then we shall have a doctrine concerning these relations which will necessarily run precisely parallel with the logical doctrine concerning the copula.

Logic may be considered as the science of identity. If we let  $a \prec' b$  mean that  $a$  is as small as  $b$ , and neglect all differences between objects except such as consist in one being as small as something which the other is not as small as, we shall have a parallel science of equality, which is *mathematics* or the logic of quantity. If we let  $a \prec'' b$  mean that all  $b$  is  $a$ , and neglect all differences between terms except so far as there is something of which one can be predicated of which the other cannot be predicated, we shall have a science of the identity of qualities, which is only logic in another aspect. If we let  $a \prec'' b$  denote that  $b$  is a consequence of  $a$ , and neglect all differences between statements except so far as they lead to different consequences, we have the logic of conditionals.

It is plain that if there be two sets of objects which correspond in any way each to each singly, then for every relation among the objects of the first set there must be a corresponding relation among objects of the second set. And for every proposition concerning objects of the first set expressed with any quasi-copula  $\prec'$  there must be a corresponding proposition concerning objects of the second set either with the same or with some other quasi-copula  $\prec''$ .

The further consideration of this subject must be postponed until after we have considered relations in general.

This is the place to mention a certain term which would never be suggested to us except by the study of the relations of terms. It is called *Ens* and I denote it by the symbol 1. It is defined by the proposition that anything whatever is Ens, or  $x \prec 1$  whatever thing  $x$  may name.

## Chap. IX. Of relative terms

*MS 230: Spring 1873*

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There are some reasonings in order to analyze which it is necessary to consider a proposition not in the simple form  $a$  is  $b$ , but in the form  $a$  is  $b$  to a  $c$ . For example, 'Every man is indebted to a woman'. This brings us to the subject of relative terms. A relative term is one which names nothing taken by itself but only in conjunction with another term, its correlate. Such are "father of \_\_\_\_\_," "lover of \_\_\_\_\_," "identical with \_\_\_\_\_," etc. We may express these by single letters and write their correlates directly after them so that we may denote 'lover of a woman'. In studying relations, we shall do well to begin with those of individuals. It is true there are no individuals, strictly speaking, but nevertheless it is most useful in logic to consider what their properties would be if they existed. We may use the capital letters  $A$ ,  $B$ ,  $C$ , etc. for individual terms. The peculiarity of such terms is that if  $A \prec B$  then  $B \prec A$ .

Every individual will have a special relation to every other. Let us write  $(A : B)$  for the relative term which signifies the relation which  $A$  and  $A$  only has to  $B$  and  $B$  only. Then we shall have  $(A : B)B \prec A$ . But  $(A : B)C$  and  $(A : B)A$  will be absurd expressions and naming nothing.

We observe that such individual relatives will be of two kinds; those of the type  $(A : A)$  which signify the relation of some individual to itself, and those of the type  $(A : B)$  which signify the relation of some one individual to some other.

Since  $(B : C)C$  names the individual  $B$  and nothing else we may substitute this expression for  $B$  wherever the latter occurs. Then  $(A : B)B \prec A$  will become  $(A : B)(B : C)C \prec A$ . But  $(A : C)C \prec A$ . Comparing these two expressions we are naturally led to consider  $(A : B)(B : C)$  which has received no signification as yet as the equivalent of  $(A : C)$ . On the same principle,  $(A : B)(D : C)$ , the letters in the

middle not being the same, would be an absurdity and not equivalent to any relative.

Let us now pass to the consideration of general relative terms, first taking up those which are indeterminate among a finite number of individual cases. These are just as impossible as individual terms themselves. Let us suppose that  $l$  denotes either  $(A : B)$ ,  $(A : C)$ , or  $(C : D)$ . And let  $m$  denote either  $B$ ,  $C$ , or  $D$ . Then  $lm$  will be one of these nine individuals

$$\begin{array}{lll} (A : B)B & (A : C)B & (C : D)B \\ (A : B)C & (A : C)C & (C : D)C \\ (A : B)D & (A : C)D & (C : D)D \end{array}$$

Some of these expressions are absurd. The remainder are

$$\begin{array}{ccc} (A : B)B, \text{ that is, } A & \text{---} & \text{---} \\ \text{---} & (A : C)C, \text{ that is, } A & \text{---} \\ \text{---} & \text{---} & (C : D)D, \text{ that is, } C. \end{array}$$

Therefore  $lm$  denotes either  $A$  or  $C$ . And, in general, it is evident that  $xy$  will be indeterminate among all the cases which result from taking every case of  $x$  and every case of  $y$ . This holds even though the number of individual cases be innumerable. If therefore  $x_1 \prec x$  and  $y_1 \prec y$  then  $x_1 y_1 \prec xy$ ; and conversely if  $z \prec xy$  there must be some case  $x_1$  of  $x$  and some case  $y_1$  of  $y$  such that  $x_1 y_1 \prec z$ . For example, if to be a lover is to be a servant, and if to be a Negro is to be a man, then every lover of a Negro is a servant of a man. And if every jockey is a buyer of an animal then there must be some kind of a buyer, and some kind of an animal such that every such buyer of such an animal is a jockey.

Any expression of the form  $xyz$  may be considered as resulting from  $xy$  followed by  $z$ , or from  $yz$  preceded by  $x$ ; because we have seen that this is true with individuals, and therefore it is true with every special case which  $xyz$  denotes.

If there are a finite number of individual relatives among which a general relative is indeterminate, they may be set out in an orderly manner in a table thus:—

E		E : B	E : C	E : D	
D	D : A				D : E
C		C : B			
B			B : C		
A		A : B			
	A	B	C	D	E

If there is no finite number of individual cases the squares of the table must be made infinitesimally small and the table becomes a continuous surface and the blackened parts of it may show the nature of the relative. For example, let us represent in this way the relative "identical with." This is the relation which every individual bears to itself and nothing else.

## Chap. X. The Copula and Simple Syllogism

*MS 232: Spring 1873*

We have seen that all thought is in signs or at least is equivalent to what would be the signification of a sign. Now in order that an object may fulfill the function of a sign it is essential that it should be thought to be such this thought being itself a sign. There must be a sign which signifies that one thing is the sign of another. A sign which does this is called a proposition the corresponding thought a judgment. In the proposition then there is reference to two signs one of which is represented as standing for whatever the other stands for. To give a language the possibility of expressing propositions it is

necessary that there should be some symbol which shall mean that a word placed in a certain relation to it—say, for example, following it in order—denotes whatever is denoted by another word which is placed in some other relation to it—say, for example, preceding it. As this is necessary in every language so it is necessary in thought which is equivalent to a language. This symbol which is the soul of a proposition is called the copula. Let us illustrate. Take the proposition ‘man is mortal’. The word man by itself stands for some one of those creatures but if nothing is added to it it is left indeterminate what one it is that it stands for. The word mortal by itself stands for something which dies while it remains indeterminate what thing of that sort it stands for. Now if we say ‘man is mortal’ we imply that no matter what member of the genus homo the word man stands for it is an individual which the word mortal also is proper to denote—that is the pole of the indication of the proposition. Every proposition of whatever kind may be expressed in the general form ‘A is B’. The ‘A’ here is termed in logic the subject of the proposition the ‘B’ the predicate. If the verb is not the substantive verb then the common form in English ‘is loving’ in place of ‘loves’ suggests the manner in which the proposition may be thrown into this form. ‘Every woman loves her child’. Here ‘woman’ takes the place of ‘A’ and ‘the lover of her own child’ takes the place of ‘B’. If the proposition is a negative one as ‘no woman hates her child’ then ‘woman’ takes the place of ‘A’ and ‘non-hater of her child’ takes the place of ‘B’. If the proposition is limited as to time then the limitation attaches either to the subject or to the predicate according to the nature of the limitation. Thus if we say ‘every man has been born and will die’ the subject is ‘man’ the predicate is ‘that which has been born and which will die’. If we say ‘At the time of Alfred few priests in England could read the Psalter’ the subject is ‘one of a certain majority of priests in England at the time of Alfred’ the predicate is ‘a person unable to read the Psalter’. The copula thus is not to be understood as implying either past present or future but as meaning simply that which is denoted by the subject is also denoted by the predicate. Some logicians have held that hypothetical propositions such as ‘if it lightens it also thunders’ are not capable of being reduced to the standard form we have given which those logicians term the form of categorical propositions but they conceive that hypothetical propositions have a distinct species. It may certainly be admitted that hypotheticals involve a conception not generally contained in either proposition that of the

dependence of one thing upon another but there is nothing to prevent a categorical proposition from containing the same idea and it is certain that the whole meaning of hypothetical may be expressed in a categorical proposition. Thus 'if it lightens it will thunder on the same day'; this is the same as to say 'every day in which there is lightning there is thunder' and in general to say that 'if M happens then N happens' is the same as to say that 'whatever exists only if M happens exists only if N happens' or to use a form of expression which is apparently less intricate because it is less fully analised the above proposition is equivalent to saying that 'every state of things in which M happens is a state of things in which N happens'.

The copula expresses a certain relation between the two terms which form the subject and predicate of the proposition. We have defined this relation in terms of the properties of signs but for the purposes of formal logic it is more useful to define it in reference to its formal properties. Of these there are three. The 1st is that anything is in this relation to itself—thus 'man is man' &c. For any term whatever is proper to denote whatever is denoted by that. This I shall term the equiparant character of the copula. The 2nd formal property of the copula is that if any term A is in this relation to a second term B which is itself in the same relation to a third term C then the 1st term A is in this relation to the 3rd term C. If A is B and B is C then A is C. For if C denotes whatever is denoted by B and B denotes whatever is denoted by A then C denotes whatever is denoted by A. This is termed the transitive character of the copula. The 3rd formal property of the copula is that if two terms stand reciprocally in this relation to each other then there is no distinction between the things which they signify. If A is B and B is A then there is no further distinction to be drawn between A and B. Of these three characters of the copula the second or transitive character is the most interesting from the point of view of formal logic. It follows immediately from this that it is good reasoning to conclude from the premises A is B and B is C the conclusion that A is C. This sort of reasoning is termed the simple syllogism and inasmuch as the transitive character of the copula is the only one by virtue of which one proposition depends upon others it follows that all reasoning must depend upon this principle and therefore that all reasoning can be reduced to the form of a syllogism however important the differences between one kind of reasoning and another may be and whatever other principles some inferences may involve. The relation expressed by the copula

is by no means the only transitive relation. Examples of others are 'being greater than' or 'less than' and it is clear that the syllogism is equally valid for any kind of transitive relation whatever so that it is the same sort of reasoning to say A is greater than B, B is greater than C therefore A is greater than C as it is to say A is B, B is C therefore A is C. This was first pointed out by Mr. De Morgan.

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## Chap. XI. On Logical Breadth and Depth

*MS 233: Spring 1873*

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As Logic is the study of the laws of signs so far as these denote things—those laws of signs which determine what things they denote and what they do not—it is necessary in Logic to pay especial attention to those terms which denote signs. Such terms are genus species &c. No thing is a genus but as there are terms such as man and tree which denote some one thing leaving it more or less indeterminate what one so we may speak of whatever may be denoted by such a general term as a genus or class. Such terms are called 'terms of second intention'. The first intention is the mental act by which an object is conceived. The second intention is the mental act by which the first conception is made an object of conception in reference to its relation to its object. A term of second intention does not so much signify the sign itself as it signifies whatever is denoted by a sign of a certain description. As signs differ in their logical characters we may define an object by means of the logical characters of the sign which denotes it and in that case it is pointed out with a peculiar kind of generality which requires special attention. Two of the most important characters of general terms are their logical breadth and depth. The breadth of a term in general is that of which the term can

be predicated. The depth of a term is that which can be predicated of it. The breadth therefore may be considered as a collection of objects—real things—though it can also be considered as consisting of the terms which may be made subject of a true proposition of which the given term is the predicate. The depth of a term cannot be considered as a collection of things but can only be considered as a complex of terms or of attributes. The term attribute, character, mark or quality is a term of second intention. Two things are alike in a certain respect that is to say the same predicate can be applied to either of them. Then the capacity of having that predicate applied to it with truth is called an attribute that is a thing to which it can be applied. The attribute is therefore an abstract term. Terms are divisible into concrete and abstract. The concrete are such as white virtuous &c. the abstract such as whiteness virtue, etc. Abstract terms do not denote any real thing but they denote fictitious things. An object's being white is conceived as being due to its being in some relation with a certain fictitious thing whiteness. In point of fact that the object is white may in a certain sense be said to be due to its connection with the sign or predicate white that is to say it must be in such a relation to the name white that this name may be applied to it with truth or else it cannot be white. There is no falsity in this statement although it is more natural to state the matter in the inverse way and to say that its having that connection with that name is due to the fact that it is white. One statement is as true as the other. In the latter more natural mode of statement the existence of the thing is looked upon as the ultimate fact but we have seen in the chapter upon reality that the final information is the ultimate fact, that final information consisting in applying a certain sign to certain objects in the predication and therefore it is perfectly correct to say that the thing's being white is due to and consists of the applicability of a certain predicate to a certain thing. An attribute or quality is not precisely the same as a predicate inasmuch as when we use the word predicate we have in mind the fact that the predicate is something extraneous to the thing which does not belong to it as it exists but belongs to it as it is thought whereas an attribute is considered as belonging to a thing whatever is thought. But upon our view of the nature of reality this is a distinction of very slight moment because existence is thus not independent of all thought and what is affirmed in the final judgment is the same as what really exists. Thus in considering the breadth and depth of terms it is desirable to make a num-

ber of distinctions. By the ‘informed breadth’ of a term I shall mean all the real objects of which it is predicable with logical truth in the supposed state of information as our knowledge is never absolute but consists only of probabilities that all the information at hand must be taken into account and those things of which there is not on the whole reason to believe that the term is truly predicable are not to be reckoned as part of its breadth. If T be a term which is predicable only of S' S" and S'" then the S's, S"’s and S'''’s will constitute the informed breadth of T. If there be a second term T' which is predicable only of S' and S" and if it is not known that S''' is entirely included under S' and S" then T is considered to have a greater informed breadth than T'. If it is known that the S'''’s are not all among the S's and S"’s the excess of breadth is certain but if it is not known whether or not this is the case it is ‘doubtful’. If certain S'''’s are known to exist which are not known to be either S's or S"’s, T is said to have a greater actual breadth than T' but if all the S'''’s which are known to exist are also known to be S's and S"’s though there are other S'''’s which are not S' or S" then T is said to have a greater potential breadth than T'. If T and T' are conceptions in different minds or in different states of the same mind then T may have a doubtful excess of breadth in one mind and no excess at all in the other mind. In that case the conception is said to be more extensively distinct to the latter mind.

By the informed depth of a term I mean all the real characters in contradistinction to mere synonymous names which can be predicated of it with logical truth in the supposed state of information no character being counted twice over knowingly. The depth like the breadth will be certainly doubtful and there is a comprehensive distinctness corresponding to extensive distinctness. The informed breadth and depth suppose a state of information which lies somewhere between two imaginary extremes. These are first the state of knowledge in which no fact should be known but only the meanings of terms and, second, the state of information in which every fact should be known. This suggests two other sorts of breadth and depth corresponding to the two essential states of information which I shall term accordingly the essential and the substantial breadth and depth. The essential depth of a term which is sometimes called its essence consists of the really conceivable qualities predicated of it in its definition. This is one of the most important features of logic. Suppose the definition of the term T be this: ‘In T is at once P' P” and

P''''. This sums up the whole meaning of T. It may not be known that there is no such thing as P' and therefore the meaning of T does not imply its existence. On the other hand we must know that P' P'' and P''' are neither of them coextensive with the whole conception of being for we know the qualities of things only by comparison with their opposites hence we must know that there is something which is not P' and that this is not T, that there is something which is not P'' and that this is not T and that there is something which is not P''' and that this is not T. Accordingly if we define the essential breadth of a term as 'those real things of which according to its every meaning a term is predicable' then 'not T' has an essential breadth that is to say its very meaning implies that there are things of which it is predicable. Thus T is a term which has essential depth but no essential breadth—'not T' is a term which has essential breadth but not essential depth and all terms may be divided into two classes the 'essential positive' and 'essential negative' the former having essential depth but not essential breadth the latter having essential breadth but not essential depth. There are some terms which are affirmative in form but which according to this definition are essentially negative and vice versa. As examples of this we may allude particularly to the terms being and nothing both of which are terms of second intention. As every term has breadth and the breadth of one term is greater than that of another we may conceive of a term the breadth of which includes that of every other term so that it is predicable of anything whatever. This is the definition of the term 'being'. Its definition therefore gives it breadth but not depth and accordingly it is essentially negative. We may also conceive of a term whose depth includes the depth of all other terms so that anything whatever may be predicated of it without any falsity and this is the definition of the term 'nothing'. For you may say what you please of nothing and if it is clearly understood that what you speak of has no existence there is no falsity in what you assert because you have not made any assertion whatever. 'Nothing' therefore is a term which has essential depth without any breadth and is according to our definition essentially affirmative. If two terms have the same essential breadth or the same essential depth logic recognizes no distinction between them. They are synonymous. They may differ rhetorically. One of these words may be associated in our minds with certain feelings with which the other is not associated but logic has nothing to do with such distinctions. But two terms may be indistinctly con-

ceived so that it is not known whether they have the same essential breadth and depth or not and in this case the distinction must be admitted even in logic.

We now come to the ‘substantial breadth and depth’. The substantial breadth is the aggregate of real substance of which alone a term is predicable with absolute truth. Substantial depth is the real character as it exists in the object, which belongs to every thing of which a term is predicable with absolute truth.

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## Chapter IV. The Conception of Time essential in Logic

*MS 237: 1–2 July 1873*

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Investigation if pushed far enough will inevitably carry any mind to one destined belief, whatever may have been its opinions at the outset. Consequently, there must arise new elements of thought during the process; and these are termed *sensations*. Every thought not a sensation is determined by something previously in the mind. These also must enter into the process of investigation, or we should be perfectly passive in the business and there would be no distinction of a right and wrong method of research, which is presupposed in logic.

Every mind which passes from doubt to belief must have ideas which follow after one another in time; every mind which reasons must have ideas which not only follow after others but are caused by them; and every mind which is capable of logical criticism of its inferences must be aware of this influence of one idea upon another.

A succession in time among ideas is thus presupposed in the conception of a logical mind; but need this time progress by a continuous flow rather than by discrete steps?

A *continuum* (like time and space as they actually are) is defined as something any part of which however small itself has parts of the same kind. The point of time or space is nothing but the ideal limit towards which we approach in dividing time or space without ever reaching it, and consequently nothing is true of a point which is not true of a space or time, except that it is the ideal limit. A *discrete quantum*, on the other hand, unlike a continuum, has ultimate parts, which differ from all larger parts in their absolute separation from one another. That is to say, no two such parts have anything in common.

If the succession of images in any mind were to take place by one being suddenly replaced by another, time for that mind would be made up of indivisible instants. All ideas would be absolutely separated by each being present during one moment and absent at all other times. The ideas of different moments would be cut off from one another and would not be individually the same even if they differed in no other respect than that of being felt at different times. The consequence would be that ideas present at different moments never could be brought together in the mind to be compared, for when either was present the other would be absent. They could therefore never be thought as alike. But an idea has no existence except so far as it is thought, so that it is only what it is thought to be when it is present to the mind. Two ideas present at different times could therefore have no resemblance. It follows that no idea could determine another, because this implies that one follows after the other according to a general rule, by which every similar idea would be followed by a similar consequent, but where there is no similarity there can be no general rule. Such a mind could certainly not be a logical one. Indeed, if as it seems natural to admit resemblances and differences can only be known through a process of comparison, it could have no consciousness at all.

Abandoning this conception, then, let us contemplate the opposite one, that the flow of time is continuous. In this case we must not say that nothing is present but a fleeting instant, a point of time. For then there would be no present. For a point of time differs in no respect from an interval, except that it is the ideal limit. And if nothing is present for any length of time, nothing is present in an instant. The true conception is that the ideas of a minute are present in the minute and that these are present through the presence of the ideas which occupy the seconds of that minute. These latter are less

mediately present than the former; and they in their turn are made present through the ideas of minuter times. But carry the division as far as you may, you will never reach an idea which is quite immediately present. There can be no consciousness in an instant but an idea occupies time. We experience or pass through thoughts as we do the events of a day or a year, without in any moment having one present. [copy A] My idea may be that of an isosceles triangle and the angle opposite the base may while the idea is present gradually increase from  $0^\circ$  to  $180^\circ$ . In this case, during the whole time, I have had the general idea of an isosceles triangle; during the parts of this time more specific ideas. And during the whole time the more specific images are presented as alike in some respects and different in others.

Under this view of the matter two different ideas may have a third idea in common and that not by a figure of speech but literally. Ideas may therefore be similar. They are not similar in themselves but they are so in the wider idea which embraces them both.

A space of time may be too long for any idea to cover. But then the memory of an idea which lasts after it may be compared with it for a while directly and then with itself. Memory may in this way be proved trustworthy and afterwards relied upon. In this way ideas may be similar in an approved memory instead of in a wider idea.

A July 2. 1873

We are familiar with the fact that an idea of any difficulty requires time for its formation. But this is not the fact to which I have reference now. Not only does it take time for an idea to grow but after that process is completed the idea cannot exist in an instant. During the time of its existence it will not be always the same but will undergo changes. Thus, if I think an isosceles triangle the angle opposite the base may vary while the image is present to me. During the whole time every shape of isosceles triangle may have been present; during one part of the time only acute-angled triangles and during the remainder only obtuse-angled triangles. In this way the resemblance of the two kinds of isosceles triangles may be perceived because they both enter into the prolonged consciousness of the isosceles triangle in general. There seems to be no other way in which resemblances and other relations among ideas can be perceived.

It thus appears that as all ideas occupy time so all ideas are more or less general and indeterminate, the wider conceptions occupying longer intervals.

It may perhaps be thought that one consciousness extends over more than a limited interval of time. But then the memory of an idea which lasts after it may be compared directly with it so long as there can be an idea which embraces them both. And when that is no longer the case the memory at one time can in the same way be compared with the memory at another time.

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## Chapter IV. The Conception of Time essential in Logic

MS 238

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1873 July 1.

Investigation, if pushed far enough, will carry all minds to the same belief, independently of what their opinions were at the outset. The process is therefore clearly one which introduces new elements of thought; and these are termed *sensations*. Every thought not a sensation is determined by something previously in the mind. Such determination of one thought by another must also enter into the process of investigation, or we should be perfectly passive in the business and there would be no distinction of a right and a wrong method of research, which is presupposed in logic.

Every mind which passes from doubt to belief in any way must have ideas which follow after one another in time. But every mind that reasons must have ideas which not only follow after others but are caused by them; and every mind that reasons consciously,—that

criticises arguments,—must be aware of this influence of one idea upon another.

Thus, a succession of time among ideas is obviously presupposed in the conception of a logical mind, but that this time need progress by a continuous flow, as time actually does, rather than by discrete steps, is not at first so clear.

A *continuum* (such as time and space actually are) is defined as something any part of which however small itself has parts of the same kind. Every part of a surface is a surface, and every part of a line is a line. The point of time or space is nothing but the ideal limit towards which we approach indefinitely close without ever reaching it in dividing time or space. To assert that something is true of a point is only to say that it is true of times and spaces however small or else that it is more and more nearly true the smaller the time or space and as little as we please from being true of a sufficiently small interval. For example, we say that a body can occupy but one position at any instant. Now, in point of fact, bodies exist in time and are always moving. But what is true is that the shorter the time for which the body's position is considered, the more determinate it is, and by taking this time sufficiently short the extreme range of its positions can be made less than any assigned difference. And so nothing is true of a point which is not at least on the limit of what is true for spaces and times. A *discrete quantum*, on the other hand, unlike a continuum has ultimate single parts, which differ from all larger parts in their absolute separation from one another, no two of them having any similar part in common. Any collection of things is such a quantum. It has parts which are themselves collections, and different collections may have parts in common. But in the process of separating such a quantity you finally come to the single things and these are no longer susceptible of such division.

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## Chapter V. That the significance of thought lies in its reference to the future

*MS 239: Summer 1873*

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In every logical mind there must be 1st, ideas; 2nd, general rules according to which one idea determines another, or habits of mind which connect ideas; and, 3rd, processes whereby such habitual connections are established.

A belief is an habitual connection of ideas. For example, to say that I believe prussic acid is a poison is to say that when the idea of drinking it occurs to me, the idea of it as a poison with all the other ideas which follow in the train of this will arise in my mind. Among these ideas, or objects present to me, is the sense of refusing to drink it. This if I am in a normal condition will be followed by an action of the nerves when needed which will remove the cup from my lips. It seems probable that every habitual connection of ideas may produce such an effect upon the will. If this is actually so, a belief and an habitual connection of ideas are one and the same.

In a mind which is capable of logical criticism of its beliefs, there must be a sensation of believing, which shall serve to show what ideas are connected. The recognition that two objects present belong together as one is a judgment. All ideas arise in judgments. This is clearly the case if they are caused by previous ideas. If they are sensations then they at once cause other ideas and are connected with these in judgments. The intellectual value of ideas lies evidently in their relations to one another in judgments and not to their qualities in themselves. All that seems blue to me might seem red and *vice versa* and yet all that I now find true of those objects I should equally find true then, if nothing else were changed. I should still

perceive the same distinctions of things that I do now. The intellectual significance of beliefs lies wholly in the conclusions which may be drawn from them, and ultimately in their effects upon our conduct. For there does not seem to be any important distinction between two propositions which never can yield different practical results. Only the difference in the facility with which a conclusion can be reached from two propositions must be regarded as a difference in their effects upon our actions.

It appears then that the intellectual significance of all thought ultimately lies in its effect upon our actions. Now in what does the intellectual character of conduct consist? Clearly in its harmony to the eye of reason; that is in the fact that the mind in contemplating it shall find a harmony of purposes in it. In other words it must be capable of rational interpretation to a future thought. Thus thought is rational only so far as it recommends itself to a possible future thought. Or in other words the rationality of thought lies in its reference to a possible future.

## Notes on Logic Book

*MS 240: Summer 1873*

Show that passage from doubt to belief is essential to cognition or something equivalent so that metaphysics may be deduced from cognition simply.

Let us suppose it is shown that one representative state of things leads to another as its consequence (2nd) & this continuously.

Then what will happen will depend on the relations of things & therefore besides fixed relations there must be variable relations having continuous measure & there must also be continuously variable potential relations upon which the change in the actual relations depends.

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# Letter, Peirce to Abraham B. Conger

*L 248: 3 or 4 January 1873*

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A. B. Conger Esqr.,  
Waldberg near Haverstraw,  
Rockland Co. N.Y.

Dear Sir:

I am no longer in charge of the Office, but I will see that some copies of the charts you wish, are sent to you.

You are mistaken in supposing that I have ever published an attempt to apply the Calculus to psychological or moral problems; as I do not think that in the present state of our knowledge that anything useful could be done in that direction. I think, however, that a proper application of the Calculus to Political Economy which has never yet been made, would be of considerable advantage in the study of that science. Such a Calculus would have three fundamental equations. The first would express the relation between the number of articles produced, and the cost to the producer; or the number of articles bought by a dealer and their cost to him. The second would express the relation between the price asked, and the number of articles that can be sold at that price by the dealer or producer. The third equation would express the principle by which the dealer is guided in fixing his price.

Let us suppose that the dealer is so wise as to have no more articles for sale than he can sell at the price he is about to fix, and also that he fixes his price at such a figure as to give him the maximum of profits; then if  $N$  is the number of articles sold, and  $P$  the price of each, and  $C$  the total cost to him of these articles,  $\underline{NP - C}$  will be his profits, and the condition that the profits will be a maximum will be expressed by the equation,

$$N + P \frac{dn}{dp} - \frac{dc}{dp} = 0.$$

But a very slight study of this will show that the most prominent phenomena of buying and selling require us to take account of the deviations of conduct from this condition of perfect wisdom. For example, if all tradesmen took account of the fact that if a part of them lower their prices the rest will be led to do the same thing, and foreseeing fully from the beginning the whole effects of competition, and each one was sure that all the others equally foresaw them, it has long ago been proved by Babbage, that it would have no effect upon lowering the prices.

To discover and express in mathematical language the principles upon which dealers really act, would form an investigation which I should suppose was possible to be carried out, and which would be of considerable utility.

The only paper of mine which I can think of as answering the least to your description, is one upon the Logic of Relatives, of which I will send you a copy.

Yours truly,  
C S Peirce

## [On Errors of Observation]

*MS 224: Between 28 March  
and 2 April 1873*

A quantity is determined by two sets of observations made under different circumstances. We ascertain the mean error of each determination, and weighting it accordingly, we take the mean. This weighted mean is always a better result than the best of the two original determinations, no matter how bad the one with which it is combined, if the proper weights have really been assigned. But since there is an uncertainty in regard to the mean errors of the two sets, our mean,

$$x = \frac{[w_i x_i]}{[w_i]}$$

where  $x_i$  is the result of a single determination and  $w_i$  is its weight, is affected by two kinds of error, that is to say, not only errors of  $x_i$ , but also errors of  $w_i$  due to error of the mean errors.

Let  $\epsilon_i$  be the mean error of the  $i^{\text{th}}$  set.

"  $g_i \epsilon_i$  " the mean error of  $\epsilon_i$

" E " the mean error of weight *one*.

Then  $w_i = \frac{E^2}{\epsilon_i^2}$

The mean error of  $w_i$  will be  $w_i^2 g_i^2$

$$D_{x_i} x = \frac{w_i}{[w_i]} \quad D_{w_i} x = \frac{x_i [w_i] - [w_i x_i]}{[w_i]^2} = \frac{x_i - x}{[w_i]}$$

Then we have

$$\epsilon^2 = \frac{[(x_i - x)^2 w_i^2 g_i^2]}{[w_i]^2} + \frac{[w_i^2 \epsilon_i^2]}{[w_i]^2}$$

$$= \frac{[(x_i - x)^2 w_i^2 g_i^2] + E^2 [w_i]}{[w_i]^2}$$

If as supposed there are only two sets

$$x_1 - x = \frac{w_2}{w_1 + w_2} (x_1 - x_2)$$

$$(x_2 - x) = - \frac{w_1}{w_1 + w_2}$$

Put  $r = \frac{w_2}{w_1}$

$$\epsilon^2 = \frac{E^2}{[w_i]} + \frac{r^2}{(1+r)^4} (g_1^2 + g_2^2) (x_1 - x_2)^2$$

Now suppose  $\epsilon^2 < \epsilon_1^2 < \epsilon_2^2$

$$\epsilon_1^2 > \frac{E^2}{[w_i]} + \frac{r^2}{(1+r)^4} (g_1^2 + g_2^2) (x_1 - x_2)^2$$

But  $\epsilon_1^2 = \frac{E^2}{w_1}$ . Then this easily reduces to

$$\frac{\epsilon_1^2 + \epsilon_2^2}{(x_1 - x_2)^2} (1+r)^2 > g_1^2 + g_2^2$$

Let  $m_1$  and  $m_2$  be the numbers of observations in the two sets. Then

$$g_1 = \frac{1}{\sqrt{2m_1}} \quad g_2 = \frac{1}{\sqrt{2m_2}}$$

$$\frac{\epsilon_1^2 + \epsilon_2^2}{(x_1 - x_2)^2} (1+r)^2 > \frac{\frac{1}{m_1} + \frac{1}{m_2}}{2} \quad r = \frac{\epsilon_1^2}{\epsilon_2^2}$$

And unless this condition is satisfied, the best of the two observations is better than their weighted mean.

Suppose for example

$$\begin{array}{ll} m_1 = 5 & m_2 = 10 \\ \epsilon_1 = 1 & \epsilon_2 = 2 \\ x_1 = 0 & x_2 = 7 \end{array}$$

Had we best take  $x = 0$  or  $x = \frac{7}{5}$ ?

$$\epsilon_1^2 + \epsilon_2^2 = 5 \quad r = \frac{\epsilon_1^2}{\epsilon_2^2} = \frac{1}{4}$$

$$(1 + r)^2 = \frac{25}{16} \quad (x_1 - x_2)^2 = 49$$

$$\frac{1}{m_1} + \frac{1}{m_2} = \frac{3}{10}$$

$$\frac{5}{49} \frac{25}{16} = .16 \quad \frac{3}{10} = .15$$

$$.16 > .15$$

Hence  $x = \frac{7}{5}$  is preferable to  $x = 0$ .

But if  $x_2$  had been 8 instead of 7,  $x = 0$  would have been best.

## *On the Theory of Errors of Observations*

P 77: Coast Survey Report 1870, 200-224

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The object of this paper is to give a general account of the theory of errors of observations, with the design of showing what the limitations to the applicability of the method of least squares are, and what course is to be pursued when that method fails. We shall begin with an elementary account of the general principles of the subject, in order to state them with a little more accuracy than is commonly done.

The notation employed is one which has been suggested by the study of the logic of relations. Small Roman letters will denote objects partly indeterminate. Thus  $m$  may denote a man, without saying what man. Small Italics will be used for relative terms; thus  $l$  may denote a lover. The correlates of such relative terms will be written after them on the same line; thus  $t m$  or  $l m$  may denote a tooth of a man or a lover of a man, if  $m$  denotes man,  $t$  a tooth of and  $l$  a lover of something undetermined. Then,  $l w$  will denote a lover of a woman, it being indeterminate what lover and what woman.  $tlw$  will denote a tooth of a lover of a woman. If we wish to denote that which is a lover of all women, we must have a symbol to denote all women. As  $[x]$  is commonly used in the method of least squares to denote the sum of all the quantities  $x$ , so we may write  $[w]$  to denote all women, and then  $l[w]$  will denote something which is a lover of all women, or we may write the same thing thus,  $l^w$ . A relative term has a double indeterminacy, being indeterminate in reference to the relate and also in reference to the correlate. A lover may be this lover, that lover, or the other lover, and each of these may be lover of this, or that, or the other. Corresponding, therefore, to the  $[l]$ , which denotes all lovers, we may write  $\{l\}$  to denote the lover to whomever he is a lover. Thus,  $\{l\}^w$  will denote a lover of nothing but women, or we may write the same thing thus,  $'w$ . We may denote "loved by" by  $K$ !

Corresponding to any absolute term, as man, there is a relative term, "man that is," as in the expression "a man that is rich." I shall denote a relative of this sort by the symbol for the absolute term, with an inverted comma after it, as  $m.$ . Thus, if  $b$  denotes anything black,  $m, b$  will denote a man that is black.

Let  $V$  be the relative "a general name which is applicable to." Thus,  $Vm$  will denote a general term which is applicable to some man.  $V^m$  will denote a general term which is applicable to every man.  $V^m, V^w$  will be a general term which is applicable to every man and to every woman.  $KV[V^m, V^w]$  will denote that to which every general term is applicable which is applicable to every man and to every woman; in other words, this denotes either a man or a woman. I shall write this for short  $m +, w.$

Zero is defined by the general equation  $x +, 0 = x$ , whatever  $x$  may be. Then, zero generally denotes nothing.

Unity is defined by the general equation  $x, 1 = x$ , whatever  $x$  may be, and then 1 generally will denote anything.

But 1 and 0 have sometimes to be interpreted as relative terms. Now, it can be proved by the principles of the logic of relatives that so considered  $0^x = 0$ , unless  $x = 0$ , when  $0^0 = 1$ ; and that  $1x = 1$ , unless  $x = 0$ , when  $10 = 0$ . It follows that  $0^x$  is such a logical function<sup>1</sup> of  $x$  that it signifies "the case of the non-existence of," while  $1x$  is such a logical function of  $x$  that it signifies "the case of the existence of."

Since  $[m]$  denotes all men, we may naturally write  $\frac{[m]}{m}$  to denote what all men become when that factor is removed which makes  $[m]$  refer to *men* rather than to anything else; that is to say, to denote the *number* of men. We may write this for short  $[m]$  with heavy brackets. Then  $t$  being a relative term, ("a tooth of,") by  $[t1]$  will be denoted the total number of teeth in the universe. But  $[t]$  will be used as equivalent to  $\frac{[t1]}{[1]}$ , or the average number of teeth that anything has.

But "anything" is not to be taken here in an absolute sense. We always limit our considerations entirely to a certain class. As De Morgan expresses it, we always have a limited universe. When we reckon up the number of all things to find the average number of teeth *per* thing, it would be absurd to count among things the virtues,

1. A mathematical function of  $x$ , such as  $\varphi x$ , is something whose value is obtained by mathematical processes when  $x$  is given. A logical function of  $x$ , of which we have, as common examples, letters with a subscript  $x$ , as  $P_x$ , is something whose signification is logically deducible when the signification of  $x$  is known.

shades of color, days or seconds of time. Anything which belongs to the limited universe under consideration is called, in the theory of probabilities, an *event*. An expression like  $[t]$ , where  $t$  is a relative term, is termed a relative number, average number, or probable number. If the relative term to which the average number refers is one of those relatives which are formed by adding a comma to the symbol for an absolute term, as  $m$ , then the relative number is called a *probability*. For example,  $[m.]$  is the average number of men that anything is, but it is usually called the probability that anything is a man.

The importance of average numbers arises from the fact that all our knowledge really consists of nothing but average numbers; for all our knowledge is derived from induction, and its analogue, hypothesis. Now, the scientific conduct of this kind of reasoning is highly complex, because all sorts of precautions have to be attended to, and it has to be accompanied by a great deal of deduction. But the general nature of induction is everywhere the same, and is completely typified in the following example. From a bag of mixed black and white beans I take out a handful, and count the number of black and the number of white beans, and I assume that the black and white are nearly in the same ratio throughout the bag. If I am in error in this conclusion, it is an error which a repetition of the same process must tend to rectify. It is, therefore, a valid inference. But it clearly teaches me nothing in reference to the color of any particular bean. Of that I am as ignorant as before. The case is not in the least altered if I find all the beans of my handful to be black, or all to be white. I can still infer only the approximate general ratio, and it is only this I express when I say the observation makes the probability of any one bean being white or being black very great; for a probability is itself only an average number. At first thought it is hard to admit this; but the difficulty will be in great measure removed if we consider how it is that the knowledge of average numbers becomes useful in particular cases. Suppose we know the relative number of black beans in a certain bag; then, if we draw a large number of beans out of it, we know that the total number of black beans we shall draw will be equal to the number of drawings multiplied by the average number of beans in the bag. Suppose we know the relative numbers of black beans in a large number of bags, containing different proportions; then, if the beans are well mixed up in each, we may only draw a single bean from each, and yet we can predict nearly the total number of black beans which would be drawn by simply taking the sum

of the relative numbers. If the black beans had a value while the white ones were worthless, then the total number of black beans which would be drawn would be the important thing to know. But as knowledge derives its practical importance from its influence upon our conduct, let us suppose that at every drawing we have our choice between two bags to draw from; then the man who knew the relative number of black beans in every bag would act in every case as though the bean he would draw from the bag which contained the larger proportion of black ones were known to be black, and as though the bean he would draw from the other were certainly white. Strictly speaking, he would know nothing about the beans that would be drawn in the particular case, but he would have a knowledge which would be so far equivalent to that that it would influence his conduct in the particular case. This is the only knowledge we ever have, a knowledge of what assumption to make in the particular case in order to do the best in the long run. Whenever, then, we have to do with a *value*, the sum-total of which in the long run is the only thing which concerns us, the average amount of it is important to be known; but in all other cases the average numbers are of no consequence.

It is evident that in the example just given it would be a valuable increase of knowledge to know, for instance, what the difference in the relative number of black beans at the top and bottom of a bag was, and any limitation of the “universe” used which should separate a relative number into two different ones would be advantageous.

There are many problems in probabilities, which, being solved, give a relative number composed of two terms, one known and the other unknown. Such an indeterminate result shows that a wider “universe” must be adopted for one of the terms of the relative number.

The fundamental arithmetical formulæ relating to relative numbers are as follows:

We have seen that the relative number of things that are men, or the probability that a thing is a man, is equal to  $\frac{[m, 1]}{[1]}$ . By “thing” here is meant any object of our limited universe, as, for example, an animal. But we may wish to consider the relative number of animals that are men when our limited universe is a wider class than animal. In this case, *a* denoting an animal, we write this probability  $\frac{[m, a]}{[a]}$ . Let us suppose that, our universe being “day,” we wish to know the

probability that if it thunders on any day it will rain on that day. To say that if it thunders it will rain on the same day is the same as to say that every day on which it thunders is a day on which it rains. Then let  $t$  be a day on which it thunders and  $r$  a day on which it rains, and the probability in question is  $\frac{[t, r]}{[t]}$ . In general, the probability

that if one event happens another will happen is equal to the probability that both will happen, divided by the probability that the first will happen.

Let us now see how to express the probability that a certain quantity will have a certain value. It is clearly implied that the quantity is defined in some other way than by its value. It might be, for instance, the length of time a bird will be on wing. Let  $x$  be this quantity, and let  $n$  be any definite value. Then  $\frac{[x, n]}{[x]}$ , or the number

of cases in which the time a bird is flying has that value, divided by the whole number of cases of a bird's flying, is the probability that a bird will fly for that length of time. But since time is continuous, the length of time a bird may be up may have an infinite number of different values. Consequently, the probability of any one is zero. We, therefore, seek the probability that the time lies between  $n$  and  $n + \Delta n$ ; and if  $\Delta n$  is infinitesimal, (say  $dn$ ), then the probability is proportional to  $dn$ . We may, therefore, write this probability thus,  $[n_x] dn$ . Here  $n_x$  denotes the case of the value of  $x$  being about  $n$ .

The probability that if one quantity,  $x$ , has a value lying between  $m$  and  $m + dm$ , then another quantity,  $y$ , has a value lying between  $n$  and  $n + dn$ , will, according to what has been said, be equal to the probability that both  $x$  and  $y$  will have the supposed values, divided by the probability that  $x$  will have the supposed value, or will be  $\frac{[m_x, n_y] dm.dn}{[m_x] dm}$ , or, since the  $dm$  disappeared by cancellation,  $\frac{[m_x, n_y]}{[m_x]} dn$ .

Given, the probability of A, the probability of B, and the probability that if A happens, B happens. Required, the probability that if B happens, A happens.

The probability of A is [A].

The probability of B is [B].

The probability that if A then B is  $\frac{[A, B]}{[A]}$ .

The probability that if B then A is  $\frac{[A, B]}{[B]}$ .

Then we have—

$$\frac{[A, B]}{[B]} = \frac{[A, B]}{[A]} \times [A] \div [B]$$

or the probability, if B happens that A happens, is equal to the probability if A happens that B happens, multiplied by the probability of A and divided by the probability of B.

We now pass to the theory of observations. An observation gives us the value of a certain quantity which is connected with an unknown quantity in such a way as to be partly dependent on the latter value, and partly on accidental circumstances, not capable of being separately taken account of.

These accidental variations are, however, in all cases subject to a statistical law, so that (observations of a certain kind forming the limited universe,  $X$  being the unknown quantity,  $\Xi$  the quantity observed) the quantity,

$$\frac{[\xi_{\Xi}, x_x]}{[x_x]} d\xi$$

or the probability that if the unknown quantity  $X$  has a certain value  $x$ , then the observed quantity  $\Xi$  will have a value between  $\xi$  and  $\xi + d\xi$ , is a certain arithmetical function of the values  $\xi$  and  $x$ . If we write  $\epsilon$  for  $\xi - x$ , or the error of observation, then we may put  $\varphi$  for such a function that—

$$\frac{[\xi_{\Xi}, x_x]}{[x_x]} = \varphi(\epsilon, x)$$

The special form of the function  $\varphi$  is called the law of the facility of the errors. Except so far as this law is known, an observation can afford us no information whatever. The following conditions are invariably fulfilled by this function. (It must be understood that only *real* quantities are considered.)

1. It has but one value for each set of values of its variables.
2. Its value is always positive and less than unity.
3. It vanishes when  $\epsilon$  is infinite.

4. Its integral relatively to  $\epsilon$  from  $-\infty$  to  $+\infty$  is always unity.

Beyond this the form of the function is determined by the peculiarities of the kind of observations.

The probability that if the observed quantity  $\Xi$  has the value  $\xi$ , then the unknown quantity  $X$  has the value  $x$  is—

$$\frac{[x_x, \xi_\Xi]}{[\xi_\Xi]} dx$$

The value of this probability is for any particular kind of observations an arithmetical function of  $\epsilon$  and  $\xi$ , which we may write  $\psi(\epsilon, \xi) d\epsilon$ .

The probability that the unknown quantity  $X$  has the value  $x$  without reference to observation is  $[x_x]dx$ . This is in any case a function of  $x$ , which may be written  $\Psi_x . dx$ .

The probability that the quantity given by observation  $\Xi$  has the value  $\xi$ , without reference to the value of the unknown quantity, is  $[\xi_\Xi] d\xi$ . This an arithmetical function of  $\xi$ , which may be  $\Phi_\xi . d\xi$ .

If  $\Phi_\xi$ ,  $\Psi_x$ , and  $\varphi(\epsilon, x)$  are given, then we can obtain  $\psi(\epsilon, \xi)$  thus:

$$\psi(\epsilon, \xi) = \frac{\varphi(\epsilon, x)}{\Phi_\xi} \Psi_x$$

Suppose a number of independent observations to be made. Then we shall have a series of functions—

$$\begin{array}{ll} \varphi_1(\epsilon_1, x) & \Phi_1 \xi_1 \\ \varphi_2(\epsilon_2, x) & \Phi_2 \xi_2 \\ \varphi_3(\epsilon_3, x) & \Phi_3 \xi_3 \\ \text{\&c.} & \text{\&c.} \end{array}$$

then the probability that if the quantities observed have the values  $\xi_1, \xi_2, \xi_3, \text{\&c.}$ , the unknown quantity  $X$  has the value  $x$  will be—

$$\Psi_x \cdot \frac{\varphi_1(\epsilon_1, x)}{\Phi_1 \xi_1} \cdot \frac{\varphi_2(\epsilon_2, x)}{\Phi_2 \xi_2} \cdot \frac{\varphi_3(\epsilon_3, x)}{\Phi_3 \xi_3} \cdot \text{\&c.}$$

or—

$$\Psi^x \cdot \prod_1^n \frac{\varphi_i(\epsilon_i, x)}{\Phi_i \xi_i}$$

The probability  $\Psi_x \cdot dx$  antecedent to *all* observations will be simply  $dx$ , and, therefore, the factor  $\Psi_x$  may be omitted in the above expression.

It will be perceived that observation never gives us to know a *number* expressing the value of the unknown quantity, but only a *function* expressing the probability of each value. It happens, however, in a very comprehensive case, that this function assumes a form which involves but two constants, so that in this case observation may be said to give us two numbers, a value for the unknown quantity, and the probable error of that value.

Mr. Crofton's method of considering this case seems to me to make it more comprehensible than any other. Suppose that the unknown quantity  $X$  has been observed twice, the values given by observation being  $\xi_1$  and  $\xi_2$ . Put  $[\xi]$  for  $\xi_1 + \xi_2$ . What then is the probability that if  $x$  is the value of  $X$ , the sum of the values given by the two observations will be  $[\xi]$ . It is clearly—

$$\int_{-\infty}^{+\infty} \varphi_1(\epsilon_1, x) \cdot \varphi_2([\epsilon] - \epsilon_1, x) \cdot d\epsilon_1$$

Developing  $\varphi_2([\epsilon] - \epsilon_1, x)$  by powers of  $\epsilon_1$ , this integral becomes—

$$\begin{aligned} \varphi_2([\epsilon], x) &= \varphi'_2([\epsilon], x) \int_{-\infty}^{+\infty} \epsilon_1 \varphi_1(\epsilon_1, x) \cdot d\epsilon_1 \\ &\quad + \frac{1}{2} \varphi''_2([\epsilon], x) \int_{-\infty}^{+\infty} \epsilon_1^2 \varphi_1(\epsilon_1, x) \cdot d\epsilon_1 \\ &\quad + \frac{1}{6} \varphi'''_2([\epsilon], x) \int_{-\infty}^{+\infty} \epsilon_1^3 \varphi_1(\epsilon_1, x) \cdot d\epsilon_1 - \&c. \end{aligned}$$

If in place of two we have  $n$  observations, the probability that the sum of all will be  $[\xi]$  is—

$$\begin{aligned} &\prod_{i=1}^{n-1} (1 - \int_{-\infty}^{+\infty} \epsilon_i \varphi_i(\epsilon_i, x) \cdot D_i \\ &\quad + \frac{1}{2} \int_{-\infty}^{+\infty} \epsilon_i^2 \varphi_i(\epsilon_i, x) \cdot D_i^2 - \&c.) \varphi_n([\epsilon], x) \end{aligned}$$

Of these co-efficients—

$$\int_{-\infty}^{+\infty} \epsilon_i \varphi_i(\epsilon_i, x)$$

is the probable or mean value of the error of the observed quantity—

$$\frac{1}{2} \int_{-\infty}^{+\infty} \epsilon_i^2 \varphi_i(\epsilon_i, x)$$

is half the probable value of the square of this error, and so on. The probable value of the error is often less than the probable value of its square, owing to positive and negative errors balancing. But the co-efficients which involve the cube and higher powers of the error may often become insignificant. This, for example, will be the case if  $n$  is very great; for then, in comparison with the sum of all the errors, the value of any one will be very small. In fact, in this case we may neglect a quantity a certain number of times, say  $M$ , larger than what we could neglect before, and may take a unit of measurement  $M$  times larger. Then if—

$$[\epsilon], [\epsilon^2], [\epsilon^3], \text{ &c.,}$$

be the probable value of the error, the error square, the error cube, &c., on the old scale of measures, their numerical values on the new scale will be—

$$\frac{[\epsilon]}{M}, \frac{[\epsilon^2]}{M^2}, \frac{[\epsilon^3]}{M^3}, \text{ &c.}$$

Consequently if  $M$  is sufficiently large, the higher co-efficients may be neglected. It also frequently happens that the error of each observation is due to the combined effect of a great number of independent or nearly independent causes, each one of which alone would produce but an insignificant effect. In this case, by the same reasoning the higher co-efficients will disappear.

The manner in which sensation and volition are propagated through the nerves is unknown, but it must be by some very complicated process, because it is very slow compared with the rate of propagation of sound. It is, therefore, probable that there may be variations of the rate of passage, and that the velocity through each small portion of the whole length of the nerve is to some extent independent of the velocity through the other parts. If this is so, the whole time of propagation would be subject to a variation, the proba-

ble values of whose cube and higher powers would be insignificant. However this may be, it appears to be a fact that in all carefully-made observations, the error of which is due to the inevitable inaccuracy of the action of the human nerves, the probable values of the cube, &c., of the errors are very small.

Whenever these quantities disappear it can be proved by an analytical process which need not be reproduced here that—

$$\varphi(\epsilon, x) = \frac{h}{\sqrt{\sigma}} e^{-\frac{h^2(\epsilon-E)^2}{\sigma}}$$

where  $h$  and  $E$  are quantities which depend upon  $x$ . We thus reach the fundamental formula of the method of least squares.

It is not the object of this paper to explain that method itself. Only it may be remarked that in the deduction of it which is usually given, it is assumed that what we wish to obtain from observations in any case (whether the method of least squares is applicable or not) is the most probable value of the unknown quantity. This is not the case, for there is but an infinitesimal probability in favor of any one value. Suppose that the cause of error in observing the place of a star were a nearly simple oscillation of the image about its mean point. Then the most probable errors would be the extreme ones. But we should much prefer to assume as the value the probable or mean value than the most probable value, which would be removed the furthest possible from that value. The conception of the matter is this. What observation has to teach us is a *function*,  $\psi(\epsilon, \xi)$ , not a mere number. But in cases to which the method of least squares is applicable, this function is completely determined by two numbers, which are the value of the unknown quantity derived from the application of that method and the value of its probable error.<sup>2</sup>

It is assumed in treatises on least squares that  $\Phi\xi \cdot d\xi$ , or the probability (without regard to the value of the unknown quantity) that the quantity observed will have a value between  $\xi$  and  $\xi + d\xi$ , is equal to  $d\xi$ . When this is not the case it is only necessary to weight each observation by dividing by  $\Phi\xi$ .

If the probability of error does not follow that law which the method of least squares supposes, that is to say, if the probability of

2. The term probable error is here used in its usual but unanalogical sense, and not for the probable value of the error, which is always assumed to be zero in least squares, or else determined by some special research.

the error  $x$  in the mean of a large number of observations is not equal to  $\frac{h}{\sqrt{\Theta}} e^{-h^2 x^2}$ , where  $h$  is a constant independent of  $x$ , then it must

at least be of this form if  $h$  be considered to be a function of  $x$ . Now,  $h^2$  is the weight which has to be assigned to an observation in the application of the method of least squares; and therefore when this method does not apply in its unmodified form, it is only necessary to find what function of  $x$ ,  $h$  must be in order to give the required law of facility of errors, and then proceed according to the method of least squares, after having weighted each observation by  $h^2$ . Let the equation which represents the facility of error be plotted, the error being taken as abscissæ, and the probability of that error as ordinate;

then plot on the same diagram the curve  $y = \frac{1}{\sqrt{\Theta}} e^{-x^2}$ . Let us

suppose, then, that a certain value of  $x, y$ , is  $A$  times as great for the actual curve of errors as it is for the normal least-squares curve. Now, if  $h$  be increased in the ratio  $A$ , the ordinates will be increased in this same ratio, and the abscissæ will be diminished in the inverse ratio so that the area of the curve is preserved. But if the ordinates at any one point are to be increased in the ratio  $A$ , then the abscissæ at that point must be contracted in the inverse ratio, so as to preserve the area; so that if we had a function  $fx$  such that  $D_x fx = \frac{1}{A}$ , then the probability of this function of the error would follow the law

$y = \frac{1}{\sqrt{\Theta}} e^{-(fx)^2}$ , and consequently  $\frac{1}{(D_x fx)^2}$  is the weight which

has to be assigned to any such observation in applying the method of least squares. It will be observed that since the weight depends on the value of the error, it will be necessary first to make an approximate solution of the problem in order to get an approximate value of the error from which to determine the weight, so that when the method of least squares is not applicable in its unmodified form, an approximate method must necessarily be resorted to.

Let us now proceed to consider the method of ascertaining the law of facility of error. In any case, if the error is compounded of an infinite number of infinitely small errors, or approximates to being so, then the law of facility of errors is of that general form which the method of least squares prescribes, and nothing remains indeterminate excepting the value of  $h$ . Observation has sufficiently shown that

in transit-observations the law of error is of this sort. I copy, for example, from Chauvenet's *Astronomy* the following table, taken from Bessel's *Fundamenta Astronomiae*, showing the errors made by Bradley in observing Sirius and Procyon:

Between—	No. of errors by the theory	No. of errors by experience
" "		
0.0 and 0.1	95	94
0.1 and 0.2	89	88
0.2 and 0.3	78	78
0.3 and 0.4	64	58
0.4 and 0.5	50	51
0.5 and 0.6	36	36
0.6 and 0.7	24	26
0.7 and 0.8	15	14
0.8 and 0.9	9	10
0.9 and 1.0	5	7
Over 1.0	5	8

Fechner, in his *Elemente der Psychophysik*, has, in connection with his psychophysical law, discussed the applicability of the method to cases of sensation generally such as the estimate of the relative weights of two masses, and he finds that if  $v$  be the energy of the force which produces the excitation of any nerve, then if  $\log v$  be considered as the observed quantity, the errors of the observed quantity will follow the law of least squares. Strictly speaking, the law of least squares recognizes the possibility of any error positive or negative, however great, although the probability of indefinitely great errors will be indefinitely small. It occurred to me that in the case of the emersion of a star from an occultation, since it was impossible to strike the chronograph-key too early, while it might be struck indefinitely too late, the law of least squares could hardly apply, and I have made some experiments upon this point, which I will narrate in detail at the end of this paper, merely remarking in this place that the divergence from the theoretical law proved to be insignificant. On the other hand, there are many cases in which we have no reason

to expect that the errors will vary according to the least-squares curve. Let us consider, for example, a chemical analysis. Here the error is generally not due to the combined action of a very great number of very small causes, but, on the contrary, there are generally two or three leading causes of error, depending upon some defect in the theory of the analysis or on some error in the manipulation, which is likely to result from a single one, such as cannot be regarded as in any way compounded of a large number of independent parts. Thus, a drop may be spilled or a portion thrown out of the crucible too small to be detected, but the whole drop is spilled at once, or the whole portion goes at once. In very exact analysis, in which such causes are almost altogether eliminated, the remaining and chief cause of error will be an error of weighing, due to a want of delicacy of the balance, and will be of the same nature as an error of computation, due to the fact that the number of decimal places used has been limited. The method of considering such errors is treated by Dr. Bremiker, in the preface to his *Tabula Logarithmorum Sex Decimalium*, a work to which the attention of chemists ought to be drawn. When the law of facility of errors cannot be deduced *a priori* from the consideration of the causes to which it is due, a large number of experimental observations must be made upon a known quantity in order to find in what manner the errors vary, or the same series of observations may be used both to determine what the value of the unknown quantity is and also what the law of the variation of errors is. Thus, in the method of least squares,  $h$  is to be determined empirically, and the common way of doing it is to use the actual observations by which the unknown quantity is determined to determine also the value of  $h$ . In doing this, it should be remembered that a precise and trustworthy determination can only be obtained from a large number of observations. This procedure amounts, in fact, to adding an additional unknown quantity, a very obvious fact, and yet one which is habitually overlooked. Encke, in his *Astronomisches Jahrbuch* for 1834, has given the most complete formulæ that I have anywhere found for determining the value of  $h$ , as well as its uncertainty. These formulæ require correction on account of the circumstance just mentioned.

When it is necessary to combine, by least squares, observations of different orders of precision, they are weighted proportionately to  $h^2$ . If we have two series of observations, one of which is as accurate

as you please, and the other as inaccurate as you please, a better result than that which the most accurate series of measures gives can always be got by combining with it the least accurate series, provided the proper weights are given to the two series. This proposition seems paradoxical, and is not admitted by many very competent heads, but I cannot see how the conclusion can possibly be evaded. It does not depend at all upon any of the peculiar principles used by the method of least squares, but rests on the fundamental axioms of probabilities. Indeed, it may conveniently be based directly upon the principles of logic itself. The least accurate series of measures offers certain facts, which may be used as premises, and it cannot be that if those facts are properly used they leave us in greater ignorance than we were before. Additional facts must increase our knowledge, if the proper inferences are made from them, and, therefore, an additional series of observations, if it have any weight at all, must, if its proper weight be assigned to it, improve the value of the unknown quantity. On the other hand, when it is considered that there is an uncertainty in the value of  $h$ , it may be that if the two series of observations differ greatly in accuracy, and the value of  $h$  is not determined with much precision, it may be better at once to take the result of the best series of observations than to combine the two series with the best weights that we are able to give.

Let—

- $x_i$  be the value from any set of observations;
- $\epsilon_i$  the mean error of this set;
- $g_i \epsilon_i$  the mean error of  $\epsilon_i$ ;
- $w_i$  the true weight;
- $E$  the mean error of weight *one*; and
- $x$  the truly weighted mean.

$$x = \frac{\sum_i (w_i x_i)}{\sum_i w_i}$$

The best approximation we can get to  $x$  will be subject to two kinds of error: first, those arising from errors of  $x_i$ ; and, secondly, those arising from errors in our assumed values of  $\epsilon_i$ , from which we derive  $w_i$  by the formula,

$$w_i = \frac{E^2}{\epsilon_i^2}$$

The mean error of  $w_i$  will be  $w_i^2 g_i^2$

$$D_{x_2} x = \frac{w_2}{\sum_i w_i}$$

$$D_{w_i} x = \frac{x_i \sum_i w_i - \sum_i w_i x}{(\sum_i w_i)^2} = \frac{x_i - x}{\sum_i w_i}$$

Then we have—

$$\begin{aligned} \epsilon^2 &= \sum_i \left( \frac{x_i - x}{\sum_i w_i} \right)^2 w_i^2 g_i^2 \\ &\quad + \sum_i \left( \frac{w_i}{\sum_i w_i} \right)^2 \epsilon_i^2 \\ &= \frac{\sum_i (x_i - x)^2 w_i g_i^2 + E^2 \sum_i w_i}{(\sum_i w_i)^2} \end{aligned}$$

These are the two common rules by which  $\epsilon$  may be calculated. Call their results  $\epsilon'$  and  $\epsilon''$ , so that—

$$\begin{aligned} \epsilon' &= \frac{E}{\sqrt{\sum_i w_i}} \\ \epsilon'' &= \sqrt{\frac{\sum_i w_i (x_i - x)^2}{(m - 1) \sum_i w_i}} \end{aligned}$$

where  $m$  is the number of determinations.

The first gives—

$$\epsilon^2 = \epsilon'^2 + \frac{\sum_i (x_i - x)^2 w_i^2 g_i^2}{(\sum_i w_i)^2}$$

If  $m = 2$ ,

$$(x_1 - x) = x_i - \frac{w_1 x_1 + w_2 x_2}{w_1 + w_2} = \frac{w_2}{w_1 + w_2} x_1 - x_2$$

$$x_2 - x = - \frac{w_1}{w_1 + w_2} (x_1 - x_2)$$

$$\epsilon^2 = \epsilon'^2 + \frac{w_1^2 w_2^2}{(w_1 + w_2)^4} (g_1^2 + g_2^2) (x_1 - x_2)^2$$

$$\text{Put } \frac{w_2}{w_1} = r,$$

$$\epsilon^2 = \epsilon'^2 + \frac{r^2}{(1+r)^4} (g_1^2 + g_2^2) (x_1 - x_2)^2$$

$$\epsilon'' = \sqrt{\frac{w_1 w_2^2 + w_1^2 w_2}{(w_1 + w_2)^3} (x_1 - x_2)^2}$$

$$= \sqrt{\frac{w_1 w_2}{(w_1 + w_2)^2} (x_1 - x_2)^2}$$

$$\epsilon''^2 = \frac{w_1 w_2}{(w_1 + w_2)^2} (x_1 - x_2)^2 = \frac{r}{(1+r)^2} (x_1 - x_2)^2$$

$$\epsilon^2 = \epsilon'^2 + \frac{r}{(1+r)^2} (g_1^2 + g_2^2) \epsilon''^2$$

Now, suppose  $\epsilon^2 < \epsilon_1^2 < \epsilon_2^2$ ; then  $r < 1$ ; then—

$$\epsilon_1^2 > \epsilon'^2 + \frac{r}{(1+r)^2} (g_1^2 + g_2^2) \epsilon''^2$$

But—

$$\epsilon' = \epsilon_1 \sqrt{\frac{w_1}{w_1 + w_2}} = \frac{\epsilon_1}{\sqrt{1+r}} = \epsilon_2 \sqrt{\frac{r}{1+r}}$$

$$\epsilon'^2 = \epsilon_1^2 \frac{1}{1+r} = \epsilon_2^2 \frac{r}{1+r}$$

$$\frac{\epsilon_1^2}{\epsilon_2^2} = \frac{w_2}{w_1} = r$$

$$\epsilon_1^2 = r \epsilon_2^2$$

$$\epsilon_1^2 - \epsilon'^2 = \epsilon_1^2 \frac{r}{1+r}$$

$$\epsilon_1^2 \frac{r}{1+r} > \frac{r}{1+r^2} (g_1^2 + g_2^2) \epsilon''^2$$

$$\frac{\epsilon_1^2}{\epsilon''^2} (1+r) > (g_1^2 + g_2^2)$$

$$\epsilon_2^2 r = \epsilon_1^2$$

$$r \frac{\epsilon_2^2}{\epsilon''^2} (1+r) > (g_1^2 + g_2^2)$$

$$r \frac{\epsilon_1^2 + \epsilon_2^2}{\epsilon''^2} > g_1^2 + g_2^2$$

Unless this condition is satisfied, the combination is worse than the best determination.

It is generally admitted that  $m_i$  being the number of observations from which  $\epsilon_i$  has been determined,

$$g_i = \frac{1}{\sqrt{2m_i}}$$

Then the condition is—

$$2r \frac{\epsilon_1^2 + \epsilon_2^2}{\epsilon''^2} > \frac{1}{m_1} + \frac{1}{m_2}$$

or we may write—

$$\frac{\epsilon_1^2 + \epsilon_2^2}{(x_1 - x_2)^2} > \frac{g_1^2 + g_2^2}{(l + r)^2}$$

or—

$$\frac{\epsilon_1^2 + \epsilon_2^2}{(x_1 - x_2)^2} (l + r)^2 > \frac{\frac{1}{m_1} + \frac{1}{m_2}}{2}$$

or we may write—

$$\frac{\frac{(x_1 - x)^2}{\epsilon_1^2} + \frac{(x_2 - x)^2}{\epsilon_2^2}}{\frac{1}{\epsilon_1^2} + \frac{1}{\epsilon_2^2}} < \frac{-4 \epsilon_1^2}{\frac{1}{m_1} + \frac{1}{m_2}}$$

Some writers upon the subject have wished to assign a smaller weight to those observations which differ largely from the mean than to those which come close to it. They have reasoned as if  $h$ , or the precision of an observation, were something which belonged to a single observation; whereas, in fact, it is a statistical quantity altogether, and belongs only to an observation as a member of a certain series. We may have a large series of observations for which  $h$  has a certain value, and those observations may perhaps be separated into two series on some principle or other for which  $h$  shall have two different values, and if this can be done there is an advantage in doing it. It is, in fact, limiting our universe. In probabilities generally  $h$  is a mean or probable quantity for a series of observations, and if we can divide our universe into two parts, getting different values of  $h$ , it will be an increase of knowledge to do so. For example, suppose that some of the observations were taken under one set of circumstances and the rest under another set of circumstances. That would afford a principle upon which the observations could be distinguished, and if the value of  $h$  for the two sets turned out different, it would be an advantage to separate them and to give them different weights. Now, the degree of discordance of observations from the most probable value of the unknown quantity may be taken as a

means of estimating the relative degrees of care, &c., used in making them, and so to discriminate between them. But it would certainly be very absurd to allow no weight to the fact that we have endeavored to make them all with equal care. It must never be forgotten that  $h$  is a statistical quantity; not one which belongs to a single observation, but one which belongs to an infinite series of observations.

It is entirely in accordance with the method of least squares to reject discordant observations, and this has always been done, even by those who object to an exact criterion for determining what observations should be rejected. For example, Mr. Glaisher says that no observation should be rejected excepting obvious mistakes, thereby admitting that it is proper sometimes to reject observations, and nobody is more opposed to the rejection of observations than he. But no line of demarkation can be drawn between mistakes which are obvious and mistakes which are not obvious. In some cases it may be obvious that 53 has been written by the recorder instead of 35. In other cases it may be doubtful whether it ought to be called an obvious mistake or whether there may be some doubt hanging over it, and there is every grade of probability, from the greatest to the least; and when we examine into the facts of observation, and do not attempt to make our way through a vacuous space of pure theory, it will be found that the occasional rejection of observations is justified from every point of view; and if observations are to be rejected, exact criteria are necessary to determine upon principles of probabilities in what cases they should be rejected. The criterion of Professor Peirce is the only one which conforms rigidly to those principles, and, indeed, I am not aware that it has been attacked upon the ground of not conforming to the principles of probabilities, although it has been attacked on the ground that no such criterion should be used, and that no observation should be rejected except upon principles of guesswork, for that is what it amounts to to say that none but obvious mistakes should be rejected. Experience has shown that the errors which this criterion rejects are almost precisely those which a person of sound judgment would pronounce to be obvious mistakes, but some other criteria have been proposed, which are confessedly inexact, but which have the advantage of involving less calculation, but these are no better than the unaided judgment of an experienced person, and in some cases not so good.

*Account of the experiments*

These experiments were made in order to study the distribution of errors in the observation of a phenomenon not seen coming on, as in the case of a transit, but sudden, as in the case of the emersion of a star from behind the moon. The time was noted upon a Hipp chronoscope, which is a modification of an invention of Wheatstone's. The train of clock-work moved by weight is regulated by the vibration of a little spring or reed striking against a toothed wheel a thousand times a second. There are two hands, one of which marks tenths of a second, and the other thousandths of a second. These hands are thrown into gear when the first event occurs, and out of gear when the second event occurs, so that the amount that they have moved measures the interval. The manner in which they are thrown in and out of gear is this: The axis of one of the wheels of the main train is hung, and the axis of one of the wheels of the hand-gearing passes completely through it and comes out behind, where it rests upon a spring, which spring is influenced by an electromagnet. There are two crown-wheels, one upon the hollow axis belonging to the main train already mentioned, the other facing it at a very small distance from it, and fixed in position and upon the axis of the wheel belonging to the hand-gearing, which moves backward and forward inside, and the other axis as described. There is a little arm, which will catch in the teeth of one or other of these crown-wheels. Before the first event it is in the teeth of the fixed crown-wheel, which thus prevents the hands from turning round. When the first event occurs this arm is thrown forward into the teeth of the rapidly rotating crown-wheel, and thus the hands begin to turn round. When the second event occurs the arm is thrown into the teeth of the first crown-wheel, and so the hands are suddenly stopped.

It will be observed that although the instrument only registers to thousandths of a second, yet if an event can be repeated many times with a variation of time much smaller than that, the instrument ought, theoretically, in the mean of a large number of observations, to give a much closer result than 0.001 second; for when the first event occurs, and the arm is thrown into the moving crown-wheel, it probably will not strike exactly in any catch, but will strike on the inclined side of a tooth. If it strikes on the forward side of the tooth,

the hands will be carried forward by the fraction of a thousandth of a second more as the arm glides down this side to the bottom of the catch. But if it strikes on the back side of the tooth, the hands will be carried relatively back the fraction of the thousandth part of a second as the arm glides back to the bottom of the catch. Now, if the top of the tooth is midway between the bottom of two catches, it is equally likely to be carried forward or back. The same thing occurs when the second event happens and the arm strikes upon the fixed crown-wheel. An error in the marked interval will thus result, which error may amount to  $-0.001$  second in the extreme, or to  $+0.001$  second, and any one error between these limits is as likely as any other; consequently, these errors, being entirely incidental and independent of one another, they will balance one another in the mean of a large number of observations, and thus a much higher degree of accuracy may be reached. This, however, is a matter which has no influence on the experiments which I have made, inasmuch as the interval measured by me was a variable one, and in point of fact I have never been able to make the instrument work with such nicety as to measure much closer than  $0.001$  second. In the descriptions of this instrument which I have seen, only two instrumental corrections have been mentioned; one is owing to the rate with which the instrument goes, and the other is with reference to the time occupied by the arm in passing from one crown-wheel to the other. To determine these two constants, a little apparatus accompanies the instrument, by which the time of the fall of a ball from different heights may be registered, and by registering the time from two different heights these two corrections, one of which is proportional to the time and the other is a constant, may be determined. The ball is held in a pair of jaws; when these jaws separate, the contact is broken, the hands begin to move, and the ball begins to fall. Care should be taken that the ball is so small that the jaws cannot be separated for any appreciable time before the ball is free to fall, but if the spring with which they open is sufficiently strong the ball may fall freely from the very first. At the bottom the ball strikes upon a platform made of wood and covered with green cloth, and it throws this platform down upon two metallic springs below it, through which contact is made again, so that the hand stops, and then the platform is held down by a catch.

As the instrument came from the makers it was found that when the ball struck upon the platform it threw one of the springs down, so that the contact was made, and then immediately interrupted

before there was time for the hand to stop, so that a slight error of about 0.001 second arose in this way. This was usually corrected by putting little wooden wedges beneath these springs, so that they could not be thrown down in this way. In order further to test the instrument, I made use of a break-circuit chronometer, and measured the interval of two seconds upon the instrument for this purpose. It was necessary to employ two telegraph-repeaters. There are two ways in which this can be arranged, so as to correct a break-circuit chronometer with a Hipp chronoscope. It is sufficient to describe one of them. The arrangement is shown in the accompanying figure:

Bat. is the battery; Ch., the chronoscope; Chr., the chronometer; R., a resistance-coil; and I and II, two telegraph-repeaters. I is a common telegraph-repeater, with the non-conducting screw so far raised that when the armature once flies up the magnet cannot bring it down again. F is the magnet end of this repeater; B, the end at the second circuit. When the first circuit is broken the armature flies up and instantly breaks the second circuit. II is arranged differently from common repeaters. As long as there is a current through the first circuit and the armature is held down, there is no connection in the second circuit. When the first circuit is broken, the armature, under the influence of a very strong spring, flies up for a distance of a tenth of a millimeter, and there makes the connection in the second circuit.

This repeater can be extemporized out of a common relay. The resistance-coil should always be used in connection with the chronoscope in such a way that when the circuit is broken in the first place the current shall be so weak as just to be able to hold the hands still, while when it is made again the current shall be so strong as to make the circuit as quickly as possible.

The rate as given by the break-circuit chronometer did not agree with that found by the fall-apparatus, and indeed there was a slight discrepancy in the rate given by the latter for different heights of fall. Professor J. E. Oliver suggested to me that this discrepancy was due to a retardation of the instrument when the hands were geared in, which took place somewhat gradually, and I found that this was the case, and that the ear could detect that when the hands were geared in, during the space of three-fourths of a second, the note produced by the vibration of the reed was lowered about the sixth of a tone. The supposition that this took place uniformly sufficiently accounted

for all the discrepancy, and this gave me two more instrumental constants, viz: the amount of retardation on gearing in the hands, and the time during which that retardation was brought about. With this instrument as well as with the other chronometer I made a large number of experiments upon the time occupied in answering signals of various kinds, such as the emission of points upon paper from behind a screen, the appearance of induction-sparks from a Ruhmkorff coil, flashes of light thrown upon a screen, sudden changes from one magic-lantern figure to another, &c., the general result of which was to confirm the facts already in our possession, and which are due to the researches of Hirsch, Daumbusch, and others. But there was one series of experiments which deserves particular description. I employed a young man about eighteen years of age, who had had no previous experience whatever in observations, to answer a signal consisting of a sharp sound like a rap, the answer being made upon a telegraph-operator's key nicely adjusted. Five hundred observations were made on every week-day during a month, twenty-four days' observations in all. The results are given in the accompanying table, and are also shown upon plate No. 27. On this plate the abscissæ represent the interval of time between the signal and the answer as indicated on the Hipp chronoscope, the ordinates measure the number of observations, which were subject to a large amount of error. The curve has, however, not been plotted directly from the observations, but after they have been smoothed off by the addition of adjacent numbers in the table eight times over, so as to diminish the irregularities of the curve. The smoother curve on the figures is a mean curve for every day drawn by eye so as to eliminate the irregularities entirely. It was found that after the first two or three days the curve differed very little from that derived from the theory of least squares. It will be noticed that on the first day, when the observer was entirely inexperienced, the observations scattered to such an extent that I have been obliged to draw the curve upon a different scale from that adopted for the other days. It will also be seen that the personal equation from the mean amount by which the answer came too late rapidly decreased for the first five days, until it was about one seventh of a second, and that it then gradually increased until the twelfth day, when it amounted to about 0.22 seconds. But while the personal equation was thus first diminishing and afterward increasing, the probable error or range of errors was constantly decreasing after the twelfth day. There was some variation in the per-

sonal equation, but not much, but the range of errors continually decreased as long as the observations lasted, and so remarkably that for the twenty-fourth day the probable error does not exceed one-eightieth of a second. I think that this clearly demonstrates the value of such practice in training the nerves for observation, for it can hardly be supposed that the best observer has so small a range of error as this, and I would therefore recommend that transit-observers be kept in constant training by means of some observations of an artificial event which can be repeated with rapidity, so that several hundred can be taken daily without great labor, and I do not think that it is essential that these observations should very closely imitate the transit of a star over wires, inasmuch as it is the general condition of the nerves which it is important to keep in training more than anything peculiar to this or that kind of observation.

### *Details of the experiments*

FIRST DAY, JULY 1, 1872

Number of observations	2	1	1	3	1	1	0	0	0	0	0	0	0	0	0	0	0	0	0
Thousands of observations	633	4	5	6	7	8	9	640	1	2	3	4	5	6	7	8	9	650	1
Thousands of a second	593	4	5	6	7	8	9	600	1	2	3	4	5	6	7	8	9	610	1
Number of observations	0	1	1	2	2	0	1	1	1	0	0	0	2	0	2	0	1	1	0
Thousands of a second	553	4	5	6	7	8	9	560	1	2	3	4	5	6	7	8	9	570	1
Number of observations	1	2	2	3	2	1	0	2	1	0	0	1	0	3	1	2	1	2	3
Thousands of a second	512	3	4	5	6	7	8	9	520	1	2	3	4	5	6	7	8	9	570
Number of observations	1	4	4	2	1	2	1	3	1	5	2	1	0	4	2	2	0	1	2
Thousands of a second	471	2	3	4	5	6	7	8	9	480	1	2	3	4	5	6	7	8	9
Number of observations	0	2	0	3	2	1	5	1	2	0	2	3	1	1	1	3	3	3	5
Thousands of a second	430	1	2	3	4	5	6	7	8	9	440	1	2	3	4	5	6	7	8
Number of observations	1	3	2	3	4	2	1	4	2	0	3	1	3	3	3	3	3	3	5
Thousands of a second	389	390	1	2	3	4	5	6	7	8	9	400	1	2	3	4	5	6	7
Number of observations	0	1	0	0	0	1	2	2	0	1	0	3	3	3	3	3	3	3	3
Thousands of a second	348	9	350	1	2	3	4	5	6	7	8	9	400	1	2	3	4	5	6
Number of observations	1	1	1	1	0	0	0	1	1	0	1	0	3	3	3	3	3	3	3
Thousands of a second	320	1	2	3	4	5	6	7	8	9	360	1	2	3	4	5	6	7	8
Number of observations	1	1	1	1	0	0	0	0	0	0	2	0	1	1	1	2	0	0	0
Thousands of a second	158	240	261	277	296	312	3	4	5	6	7	8	9	320	1	2	3	4	5

8	0	9	1	410	2	1	2	3	2	6	3	2	1	4	4	0	0
9	0	0	1	370	1	1	2	3	1	2	3	0	1	5	5	0	0
330	0	0	1	1	3	3	1	4	2	2	4	1	0	6	6	1	0
1	0	0	2	1	3	3	1	4	3	6	5	2	0	6	7	0	0
2	1	3	0	4	4	5	3	6	3	7	3	8	1	7	8	0	0
3	0	4	1	5	3	6	3	7	2	8	2	9	2	9	9	1	1
4	0	5	1	6	6	7	2	8	9	2	9	2	3	580	620	660	1
5	0	6	0	7	2	8	2	9	1	500	0	1	3	1	1	1	0
6	0	7	2	8	2	9	1	3	1	2	3	2	0	2	1	1	2
7	2	8	1	9	2	1	6	2	3	3	1	4	2	3	3	2	3
8	0	9	1	420	2	1	6	2	3	3	1	4	2	4	4	4	1
9	1	380	1	1	1	2	1	2	1	3	5	1	5	5	5	6	682
340	0	1	1	2	0	3	3	4	1	4	0	5	0	6	6	1	692
1	0	2	0	3	3	4	2	5	1	5	2	6	1	7	7	0	732
2	1	3	0	4	2	5	1	6	4	7	0	8	0	8	8	2	748
3	1	4	1	5	4	6	3	7	6	8	2	9	1	9	9	0	757
4	2	5	0	6	3	7	6	8	4	9	2	590	1	630	0	900	1
5	2	6	4	7	0	8	4	9	1	550	1	1	2	1	1	1	919
6	0	7	2	8	4	9	4	510	2	1	1	592	2	632	2	978	1
347	3	388	1	429	2	470	4	511	0	552	2						

## SECOND DAY, JULY 5, 1872

97	1	176	3	203	3	230	8	257	3	283	1	309	1	335	0	361	1
106	1	7	1	4	4	1	10	8	7	4	5	310	0	6	1	2	0
119	1	8	1	5	3	2	6	9	2	5	0	1	0	7	0	3	1
124	1	9	2	6	1	3	5	260	3	6	1	2	0	8	0	4	0
127	1	180	2	7	4	4	1	1	2	7	0	3	2	9	1	5	0
129	1	1	2	8	4	5	5	2	3	8	4	4	2	340	0	6	2
134	1	2	1	9	3	6	3	3	5	9	0	5	0	1	0	7	0
136	1	3	1	210	3	7	7	4	5	290	1	6	2	2	0	8	0

**SECOND DAY, JULY 5, 1872 Continued**

## THIRD DAY, JULY 6, 1872

90	2	142	1	164	2	186	4	208	5	229	3	250	0	271	1	292	1
106	1	3	1	5	3	7	4	9	4	230	2	1	1	2	2	3	1
111	1	4	3	6	3	8	2	210	3	1	6	2	2	3	0	4	0
113	1	5	1	7	5	9	2	1	4	2	4	3	5	4	2	5	1
124	2	6	3	8	2	190	2	2	3	3	2	4	1	5	1	6	0
5	2	7	5	9	5	1	6	3	1	4	3	5	2	6	1	7	0
6	2	8	5	170	4	2	4	4	9	5	2	6	2	7	0	8	0
7	2	9	2	1	8	3	5	5	9	6	2	7	2	8	1	9	0
8	2	150	4	2	1	4	10	6	5	7	1	8	2	9	0	300	0
9	0	1	7	3	3	5	10	7	1	8	4	9	4	280	1	1	0
130	1	2	2	4	4	6	1	8	2	9	3	260	1	1	2	0	0
1	0	3	0	5	6	7	4	9	7	240	2	1	2	2	3	3	1
2	2	4	3	6	2	8	5	220	3	1	1	2	1	3	1	318	1
3	0	5	1	7	3	9	10	1	4	2	3	3	2	4	0	323	1
4	1	6	1	8	6	200	8	2	7	3	1	4	1	5	1	347	1
5	0	7	5	9	2	1	3	3	5	4	0	5	3	6	0	356	1
6	2	8	1	180	6	2	4	4	3	5	3	6	0	7	1	364	1
7	0	9	3	1	5	3	4	5	2	6	4	7	0	8	0	365	1
8	1	160	4	2	2	4	4	6	5	7	2	8	1	9	1	381	1
9	1	1	6	3	2	5	8	7	8	8	1	9	1	290	0	445	1
140	3	2	1	4	8	6	4	228	3	249	1	270	0	291	1	452	1
141	1	163	3	185	2	207	5										

## FOURTH DAY, JULY 8, 1872

65	1	148	1	169	2	190	6	211	10	231	3	251	1	271	2	291	0
97	1	9	1	170	4	1	9	2	6	2	5	2	2	2	1	2	0
111	2	150	0	1	7	2	9	3	9	3	3	3	2	3	2	3	0
126	1	1	1	2	5	3	4	4	8	4	3	4	1	4	0	4	1

**FOURTH DAY, JULY 8, 1872 Continued**

## FIFTH DAY, JULY 9, 1872

62	1	82	0	101	7	120	3	139	8	158	2	177	6	196	1	215	0
3	0	3	1	2	5	1	3	140	7	9	8	8	3	7	0	6	1
4	0	4	0	3	2	2	4	1	5	6	9	9	6	8	0	7	0
5	2	5	0	4	3	3	5	2	8	1	9	5	5	9	0	8	1
6	0	6	2	5	4	4	1	3	8	2	7	1	3	200	1	9	0
7	1	7	0	6	0	5	4	4	11	3	3	2	5	1	0	220	1
8	1	8	0	7	1	6	2	5	4	4	3	3	3	2	1	0	0
9	0	9	0	8	3	7	4	6	8	5	3	4	5	3	2	2	0
70	0	90	2	9	2	8	4	7	6	6	3	5	3	4	2	3	0
1	0	1	1	110	3	9	1	8	4	7	3	6	4	5	0	4	0
2	1	2	3	1	2	130	6	9	3	8	4	7	4	6	0	5	1
3	1	3	3	2	2	1	6	150	5	9	2	8	3	7	1	237	1
4	1	4	0	3	0	2	5	1	4	170	7	9	2	8	2	247	1
5	0	5	3	4	3	3	4	2	5	1	6	190	2	9	2	249	1
6	1	6	2	5	4	4	7	3	8	2	5	1	1	210	2	256	1
7	2	7	2	6	6	5	7	4	4	3	8	2	4	1	2	259	1
8	2	8	0	7	4	6	8	5	7	4	6	3	3	2	3	270	1
9	1	9	0	8	4	7	6	6	10	5	3	4	1	3	0	285	1
80	2	100	3	119	1	138	9	157	5	176	3	195	1	214	0	310	1
81	0																

## SIXTH DAY, JULY 10, 1872

66	1	117	0	137	2	157	5	177	4	197	3	217	1	237	1	257	0
72	1	8	1	8	0	8	6	8	3	8	3	8	3	8	1	8	0
75	1	9	1	9	5	9	7	9	7	9	1	9	2	9	2	9	1
87	2	120	1	140	5	160	7	180	3	200	5	220	3	240	0	260	1
88	1	1	1	1	3	1	7	1	4	1	1	1	1	1	0	1	0
101	2	2	3	2	6	2	3	2	11	2	8	2	1	2	1	2	0

SIXTH DAY, JULY 10, 1872 Continued

SEVENTH DAY, JULY 15, 1872

65	1	107	0	129	2	151	5	173	6	195	4	217	3	238	1	269	0
67	1	8	1	130	6	2	4	4	3	6	4	8	5	9	0	260	0
76	1	9	2	1	1	3	6	5	0	7	7	9	4	240	1	1	1
82	1	110	1	2	2	4	5	6	5	8	4	220	1	1	1	2	0

89	1	1	0	3	2	5	2	7	4	9	6	1	4	2	0	3	1
90	1	2	1	4	1	6	5	8	2	200	2	2	3	2	3	4	0
1	0	3	0	5	0	7	4	9	6	1	4	3	4	2	2	5	1
2	0	4	4	6	1	8	5	180	6	2	1	4	3	4	1	6	0
3	0	5	3	7	1	9	2	1	9	3	5	5	1	5	3	7	0
4	0	6	2	8	2	160	3	2	13	4	5	6	5	7	2	8	1
5	0	7	3	9	2	1	4	3	6	5	2	7	3	8	2	278	1
6	1	8	0	140	1	2	5	4	8	6	4	8	4	9	0	282	1
7	0	9	0	1	1	3	4	5	7	7	7	9	4	250	2	301	2
8	0	120	0	2	2	4	2	6	6	8	4	230	1	1	1	314	1
9	0	1	1	3	3	5	7	7	7	9	6	1	5	2	1	336	1
100	1	2	0	4	2	6	4	8	4	210	4	2	1	3	0	357	1
1	0	3	0	5	3	7	14	9	5	1	5	3	3	4	0	273	1
2	2	4	0	6	2	8	4	190	4	2	4	4	2	5	1	376	1
3	0	5	0	7	7	9	2	1	7	3	8	5	2	6	0	386	1
4	1	6	1	8	4	170	7	2	1	4	2	6	1	7	1	459	1
5	0	7	2	9	2	1	3	3	7	5	5	237	3	258	0	687	1
106	1	128	1	150	1	172	5	194	5	216	0						

## EIGHTH DAY, JULY 16, 1872

61	1	133	0	151	2	169	5	187	9	204	9	221	5	238	1	255	1
85	1	4	0	2	2	170	7	8	10	5	10	2	7	9	1	6	0
105	1	5	0	3	1	1	3	9	3	6	3	3	2	240	0	7	1
111	1	6	0	4	4	2	3	190	11	7	8	4	1	1	2	8	0
119	1	7	2	5	5	3	5	1	6	8	5	5	4	2	1	9	0
120	0	8	2	6	3	4	7	2	8	9	5	6	1	3	1	260	1
1	0	9	1	7	2	5	4	3	11	210	4	7	2	4	1	1	0
2	1	140	0	8	4	6	4	4	8	1	3	8	3	5	0	2	1
3	1	1	0	9	1	7	8	5	8	2	10	9	5	6	0	3	0

**EIGHTH DAY, JULY 16, 1872 Continued**

NINTH DAY, JULY 17, 1872

71	1	136	1	154	4	172	2	189	6	206	9	223	2	240	1	257	0
77	1	7	0	5	3	3	6	190	9	7	3	4	4	1	0	8	1
88	1	8	1	6	1	4	5	1	6	8	6	5	5	2	0	9	0
106	1	9	2	7	3	5	8	2	5	9	8	6	0	3	0	260	0
114	1	140	1	8	3	6	6	3	8	210	5	7	5	4	2	1	1
115	1	1	1	9	2	7	6	4	6	1	8	8	1	5	0	2	0
124	1	2	1	160	1	8	4	5	10	2	6	9	4	6	4	3	0
5	0	3	3	1	2	9	7	6	7	3	7	230	2	7	1	4	1
6	0	4	0	2	2	180	2	7	6	4	5	1	0	8	0	276	1

TENTH DAY, JULY 18, 1872

## ELEVENTH DAY, JULY 19, 1872

## TWELFTH DAY, JULY 20, 1872

95	1	175	1	193	2	211	1	229	6	247	3	265	5	282	0	299	0
109	1	6	0	4	2	2	2	230	8	8	1	6	3	3	2	300	1
126	1	7	0	5	0	3	6	1	4	9	2	7	5	4	3	1	0
141	1	8	0	6	5	4	5	2	3	250	2	8	1	5	0	2	0
157	1	9	1	7	0	5	5	3	9	1	7	9	4	6	1	3	0
162	1	180	0	8	4	6	7	4	6	2	3	270	4	7	0	4	1
3	0	1	1	9	1	7	5	5	6	3	3	1	2	8	0	5	1
4	1	2	1	200	3	8	8	6	5	4	3	2	1	9	2	314	1
5	0	3	0	1	2	9	6	7	4	5	5	3	1	290	0	316	1
6	0	4	0	2	2	220	12	8	4	6	6	4	3	1	0	318	1
7	0	5	0	3	1	1	8	9	6	7	5	5	1	2	1	322	1
8	0	6	0	4	1	2	11	240	11	8	1	6	1	3	0	329	1
9	0	7	1	5	2	3	4	1	4	9	3	7	1	4	0	354	1
170	1	8	0	6	4	4	4	2	7	260	2	8	0	5	0	365	1
1	0	9	0	7	5	5	6	3	6	1	4	9	1	6	1	374	1
2	2	190	0	8	3	6	5	4	5	2	3	280	0	7	1	390	1
3	1	1	3	9	4	7	5	5	5	3	1	281	3	298	1	391	1
174	0	192	0	210	2	228	13	246	6	264	1						

## THIRTEENTH DAY, JULY 22, 1872

133	1	166	0	185	0	204	0	223	8	242	8	261	14	280	3	299	0
140	1	7	0	6	0	5	1	4	5	3	11	2	8	1	0	300	0
148	1	8	0	7	0	6	3	5	9	4	8	3	4	2	3	1	0
9	1	9	0	8	1	7	3	6	5	5	9	4	7	3	6	2	0
150	0	170	0	9	0	8	4	7	5	6	7	5	2	4	2	3	0
1	0	1	0	190	0	9	2	8	9	7	9	6	6	5	1	4	1
2	0	2	0	1	0	210	2	9	4	8	7	7	4	6	0	5	0
3	0	3	0	2	0	1	6	230	8	9	9	8	5	7	4	6	1

**THIRTEENTH DAY, JULY 22, 1872 Continued**

FOURTEENTH DAY, JULY 23, 1872													
Thousands of observations		Number of observations		Thousands of observations		Number of observations		Thousands of observations		Number of observations		Thousands of observations	
Thousands of a second	a second	Thousands of a second	a second	Thousands of a second	a second	Thousands of a second	a second	Thousands of a second	a second	Thousands of a second	a second	Thousands of a second	a second
4	0	4	5	2	3	13	2	250	1	9	270	7	0
5	0	5	6	3	4	7	2	1	2	4	1	8	0
6	0	6	7	4	5	6	3	2	3	5	1	9	0
7	1	7	8	0	0	8	5	13	2	6	2	310	1
8	1	8	9	0	0	5	4	10	7	7	2	3	1
9	0	9	0	0	0	6	3	3	5	6	1	4	0
160	0	180	0	200	1	7	8	240	1	7	2	3	0
1	0	1	0	0	0	9	9	241	5	9	4	5	0
2	0	2	0	1	0	4	4	260	7	8	6	6	0
3	0	3	0	2	0	5	5				2	4	1
4	1	184	1	203	2	5	7				2	5	1
165	0										298	6	373

174	1	2	1	210	5	8	11	6	11	3	5	280	1	7	1	4	0
5	0	3	1	1	8	9	14	7	5	4	3	1	1	8	0	5	0
6	0	4	0	2	1	230	10	8	8	5	1	2	1	9	0	6	0
7	0	5	2	3	5	1	11	9	3	6	4	3	1	300	0	7	2
8	0	6	4	4	4	2	5	250	5	7	2	4	0	1	0	8	0
9	1	7	3	5	3	3	8	1	7	8	1	5	0	2	0	9	1
180	1	8	2	6	2	4	6	2	9	9	0	6	1	3	0	359	1
1	1	9	1	7	4	5	9	3	9	270	1	7	0	4	1	470	1
2	0	200	4	8	7	6	8	4	8	1	3	8	0	5	1	693	1
3	0	1	4	9	3	7	11	255	7	272	4	289	1	306	2	705	1
184	3	202	1	220	4	238	12										

## FIFTEENTH DAY, JULY 24, 1872

73	1	177	0	195	2	213	4	231	5	249	4	267	1	285	1	302	1
105	1	8	0	6	1	4	9	2	11	250	7	8	7	6	0	3	0
110	1	9	1	7	1	5	8	3	8	1	4	9	1	7	1	4	0
112	1	180	0	8	3	6	4	4	8	2	6	270	4	8	2	5	0
140	1	1	0	9	1	7	8	5	9	3	3	1	3	9	1	6	0
148	1	2	0	200	0	8	7	6	15	4	4	2	4	290	1	7	0
158	1	3	0	1	1	9	9	7	7	5	7	3	1	1	0	8	0
166	1	4	1	2	4	220	5	8	12	6	8	4	1	2	1	9	2
7	0	5	0	3	1	1	5	9	10	7	6	5	2	3	1	310	1
8	1	6	1	4	2	2	10	240	15	8	3	6	1	4	0	1	1
9	1	7	3	5	2	3	12	1	9	9	1	7	1	5	2	2	0
170	0	8	0	6	1	4	7	2	8	260	2	8	1	6	1	3	0
1	0	9	0	7	0	5	6	3	14	1	3	9	3	7	0	4	0
2	0	190	0	8	5	6	7	4	4	2	8	280	1	8	0	5	1
3	0	1	0	9	4	7	8	5	4	3	3	1	0	9	0	406	1
4	2	2	2	210	6	8	11	6	7	4	1	2	1	300	0	443	1
5	1	3	4	1	5	9	8	7	7	5	1	3	0	301	0	467	2
176	0	194	1	212	5	230	12	248	8	266	2	284	1				

SIXTEENTH DAY, JULY 25, 1872

## SEVENTEENTH DAY, JULY 26, 1872

76	1	195	0	216	1	237	5	258	9	279	10	300	4	321	1	342	1
104	1	6	2	7	0	8	2	9	7	280	4	1	0	2	0	3	0
128	1	7	0	8	1	9	5	260	11	1	7	2	1	3	1	4	1
147	1	8	0	9	2	240	7	1	9	2	6	3	1	4	1	5	3
162	1	9	0	220	3	1	2	2	8	3	7	4	1	5	1	6	0
166	1	200	1	1	3	2	4	3	5	4	7	5	1	6	2	7	1
180	1	1	1	2	0	3	5	4	7	5	3	6	2	7	0	8	2
1	0	2	0	3	4	4	4	5	8	6	6	7	0	8	0	9	0
2	0	3	1	4	3	5	9	6	5	7	7	8	2	9	0	350	0
3	0	4	0	5	2	6	2	7	3	8	3	9	5	330	0	1	0
4	0	5	0	6	0	7	5	8	7	9	5	310	6	1	0	2	0
5	0	6	1	7	2	8	4	9	2	290	5	1	1	2	1	3	1
6	3	7	2	8	1	9	8	270	6	1	6	2	4	3	0	366	1
7	0	8	5	9	3	250	5	1	5	2	7	3	0	4	0	372	1
8	0	9	4	230	6	1	7	2	6	3	5	4	1	5	0	380	1
9	0	210	2	1	2	2	4	3	7	4	5	5	4	6	0	390	1
190	1	1	2	2	4	3	5	4	5	5	3	6	1	7	1	392	1
1	0	2	2	3	3	4	7	5	6	6	5	7	2	8	1	394	1
2	0	3	2	4	3	5	7	6	11	7	1	8	0	9	1	448	1
3	1	4	2	5	4	6	5	7	2	8	3	9	0	340	0	467	1
194	0	215	2	236	3	257	7	278	5	299	3	320	2	341	1		

## EIGHTEENTH DAY, JULY 27, 1872

184	2	201	3	218	0	235	4	252	16	269	5	286	6	302	2	318	0
5	0	2	1	9	3	6	10	3	6	270	11	7	4	3	1	9	0
6	0	3	2	220	2	7	5	4	10	1	10	8	1	4	0	320	0
7	0	4	2	1	3	8	2	5	11	2	6	9	1	5	1	1	0
8	1	5	0	2	0	9	5	6	8	3	6	290	1	6	1	2	1

**EIGHTEENTH DAY, JULY 27, 1872 Continued**

NINETEENTH DAY, JULY 29, 1872

119	1	200	0	217	0	234	5	251	11	267	8	283	2	299	1	315	0
148	1	1	0	8	1	5	2	2	10	8	6	4	1	300	1	6	0
152	1	2	0	9	2	6	7	3	13	9	11	5	3	1	2	7	0
157	1	3	1	220	3	7	7	4	15	270	6	6	5	2	1	8	1
187	1	4	0	1	3	8	9	5	11	1	10	7	4	3	0	9	0
8	0	5	1	2	3	9	4	6	12	2	12	8	4	4	0	320	0

9	0	6	0	3	0	240	11	7	7	3	9	9	5	5	0	1	1
190	1	7	2	4	1	1	8	8	11	4	10	290	1	6	0	2	0
1	1	8	0	5	0	2	7	9	6	5	4	1	0	7	0	3	0
2	0	9	0	6	2	3	8	260	11	6	5	2	1	8	2	4	1
3	0	210	2	7	7	4	7	1	8	7	5	3	2	9	1	334	1
4	0	1	0	8	0	5	10	2	11	8	4	4	1	310	1	359	1
5	0	2	1	9	3	6	11	3	4	9	2	5	1	1	1	366	1
3	0	3	0	230	3	7	8	4	4	280	7	6	1	2	0	591	1
7	0	4	1	1	4	8	11	5	4	1	5	7	0	3	0	779	1
8	1	5	1	2	3	9	13	266	8	282	4	298	1	314	0	851	1
199	1	216	1	233	6	250	7										

## TWENTIETH DAY, JULY 30, 1872

130	1	195	0	214	2	233	10	252	12	271	1	290	1	309	1	328	0
159	1	6	0	5	3	4	4	3	9	2	8	1	1	310	0	9	1
177	0	7	0	6	1	5	8	4	4	3	7	2	3	1	0	330	1
8	0	8	1	7	5	6	6	5	7	4	5	3	1	2	1	1	0
9	0	9	2	8	2	7	9	6	9	5	3	4	0	3	0	2	0
180	0	200	0	9	2	8	5	7	8	6	8	5	1	4	0	3	0
1	0	1	0	220	9	9	6	8	8	7	6	6	2	5	0	4	0
2	0	2	1	1	6	240	13	9	6	8	2	7	3	6	0	5	0
3	1	3	2	2	2	1	12	260	8	9	4	8	0	7	0	6	0
4	0	4	2	3	2	2	9	1	9	280	5	9	1	8	0	7	1
5	0	5	0	4	4	3	4	2	8	1	2	300	0	9	0	8	0
6	1	6	1	5	3	4	6	3	14	2	7	1	2	320	1	9	0
7	1	7	0	6	4	5	7	4	7	3	2	2	0	1	0	340	0
8	0	8	2	7	4	6	8	5	11	4	0	3	1	2	0	1	0
9	0	9	0	8	3	7	7	6	6	5	1	4	0	3	0	2	1
190	0	210	2	9	2	8	9	7	8	6	1	5	0	4	1	360	1
1	0	1	2	230	4	9	3	8	7	7	3	6	1	5	0	486	1



## TWENTY-SECOND DAY, AUGUST 1, 1872

139	1	204	0	219	2	233	2	247	7	261	14	275	12	289	6	303	1
157	1	5	2	220	0	4	7	8	10	2	8	6	3	290	0	4	0
171	1	6	1	1	1	5	6	9	3	3	12	7	4	1	1	5	0
192	1	7	2	2	6	6	6	250	8	4	7	8	8	2	1	6	0
3	1	8	0	3	1	7	6	1	13	5	8	9	4	3	0	7	1
4	1	9	1	4	4	8	12	2	8	6	17	280	2	4	2	8	0
5	0	210	1	5	4	9	10	3	14	7	8	1	4	5	0	9	1
6	1	1	2	6	4	240	4	4	8	8	6	2	7	6	2	325	1
7	1	2	2	7	4	1	7	5	6	9	5	3	3	7	1	339	1
8	0	3	2	8	1	2	10	6	8	270	5	4	1	8	2	342	1
9	0	4	1	9	6	3	7	7	6	1	9	5	4	9	1	382	1
200	0	5	1	230	5	4	3	8	8	2	7	6	5	300	1	406	1
1	0	6	3	1	10	5	6	9	10	3	9	7	3	1	0	410	1
2	0	7	0	232	6	246	10	260	9	274	2	288	1	302	0	689	1
203	0	218	0														

## TWENTY-THIRD DAY, AUGUST 2, 1872

142	1	184	0	202	1	220	1	238	5	256	10	274	7	292	0	310	1
166	1	5	1	3	0	1	6	9	9	7	10	5	3	3	4	1	0
7	2	6	0	4	1	2	5	240	8	8	11	6	1	4	1	2	0
8	0	7	0	5	0	3	6	1	8	9	9	7	3	5	2	3	0
9	0	8	0	6	1	4	5	2	7	260	8	8	4	6	0	4	1
170	0	9	1	7	1	5	6	3	12	1	9	9	7	7	0	5	0
1	0	190	0	8	0	6	4	4	9	2	13	280	3	8	2	6	1
2	2	1	0	9	0	7	3	5	7	3	6	1	2	9	0	7	0
3	0	2	0	210	1	8	4	6	7	4	11	2	3	300	0	8	0
4	0	3	1	1	0	9	5	7	6	5	7	3	4	1	0	9	0
5	1	4	0	2	3	230	4	8	6	6	6	4	1	2	1	320	0

## TWENTY-THIRD DAY, AUGUST 2, 1872 Continued

		Number of observations	0 1 1 1 1 1 1
	<i>Thousandsths of a second</i>	1 2 3 340 371 406 661	
	<i>Number of observations</i>	0 0 0 1 0 0 1	
	<i>Thousandsths of a second</i>	3 4 5 6 7 8 309	
	<i>Number of observations</i>	0 3 1 1 1 2 1	
	<i>Thousandsths of a second</i>	5 6 7 8 9 9 290	
	<i>Number of observations</i>	8 8 6 14 3 7 291	
	<i>Thousandsths of a second</i>	7 8 9 270 1 2 273	
	<i>Number of observations</i>	12 9 8 10 12 9 5	
	<i>Thousandsths of a second</i>	9 250 1 2 3 4 255	
	<i>Number of observations</i>	9 8 6 6 6 4 2	
	<i>Thousandsths of a second</i>	1 2 3 4 5 6 237	
	<i>Number of observations</i>	3 2 4 1 0 5 2	
	<i>Thousandsths of a second</i>	3 4 5 6 7 8 219	
	<i>Number of observations</i>	0 3 0 0 0 0 0	
	<i>Thousandsths of a second</i>	15 6 1 8 9 9 200	
	<i>Number of observations</i>	0 0 0 1 1 0 1	
	<i>Thousandsths of a second</i>	6 7 8 9 9 1 2 183	

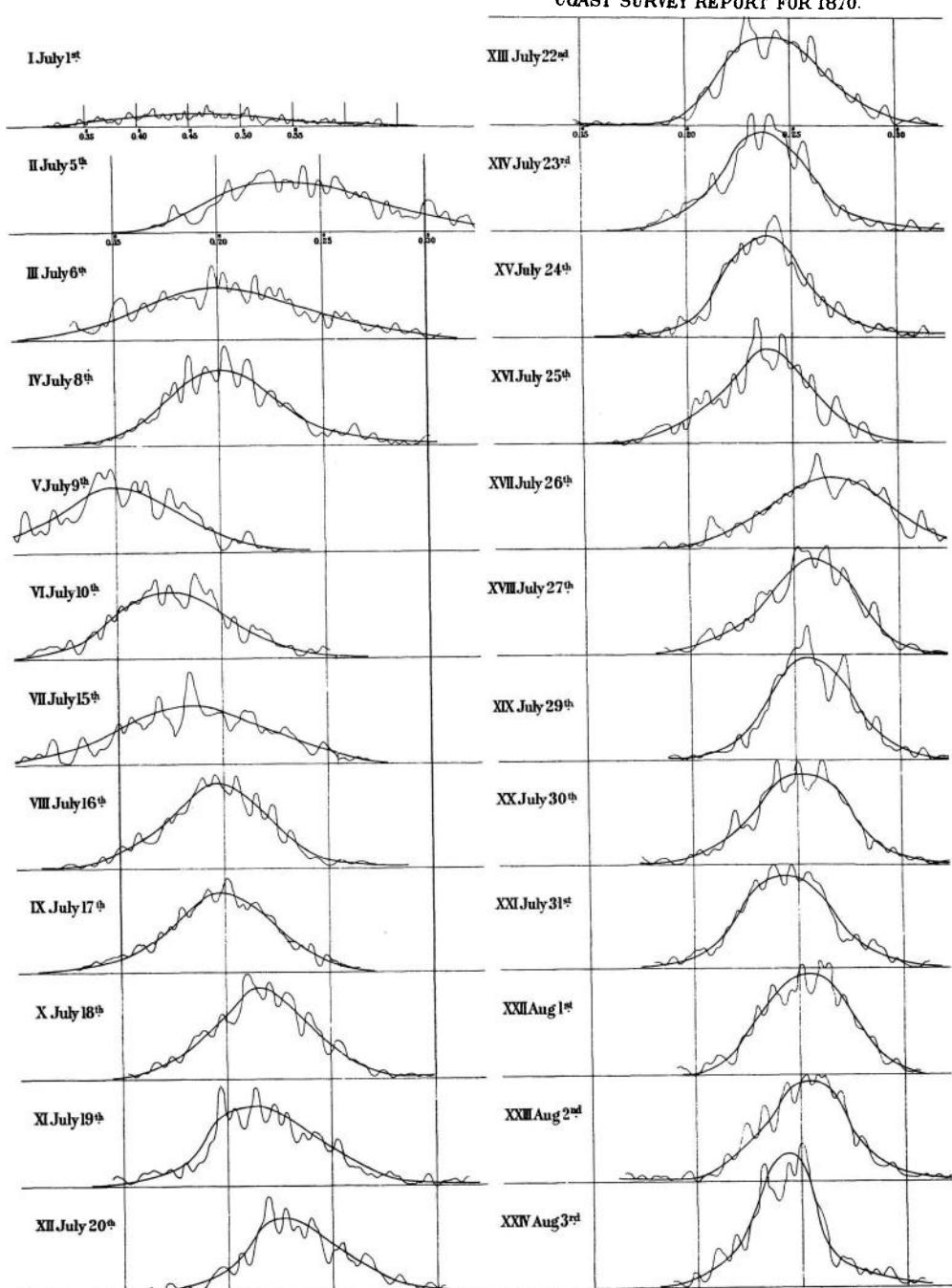
TWENTY-FOURTH DAY, AUGUST 3, 1872

119	1	198	2	213	3	228	9	243	12	257	12	271	3	285	1	299	0
134	1	9	1	4	4	9	4	4	10	8	5	2	1	6	4	300	0
156	1	200	2	5	1	230	7	5	16	9	8	3	4	7	0	1	0
172	1	1	1	6	1	1	11	6	9	260	6	4	1	8	1	2	1
175	1	2	0	7	4	2	12	7	7	1	9	5	5	9	0	3	0
188	2	3	0	8	7	3	18	8	11	2	2	6	0	290	2	4	0
9	0	4	1	9	5	4	5	9	13	3	6	7	2	1	0	5	0
190	1	5	1	220	3	5	7	250	13	4	4	8	0	2	4	6	0
1	0	6	1	1	6	6	11	1	14	5	1	9	0	3	0	7	2
2	1	7	1	2	3	7	10	2	14	6	1	280	3	4	3	8	0
3	0	8	2	3	1	8	8	3	12	7	2	1	2	5	0	9	1
4	0	9	6	4	4	9	8	4	12	8	2	2	1	6	0	334	1
5	0	210	3	5	8	240	6	5	6	9	3	3	1	7	1	340	1
6	0	1	3	6	6	1	9	256	8	270	3	284	0	298	0	374	1
197	0	212	3	227	5	242	8										

Plate 27 DIAGRAMS ILLUSTRATING APPENDIX NO. 21.

*On the Theory of Errors of Observation*

CQAST SURVEY REPORT FOR 1870.



# Linear Associative Algebra Improvement in the Classification of Vids

*MS 227: Between 3 and 7 April 1873*

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## Theorem I

Benjamin Peirce shows that of idempotent vids one belongs to the first group and the rest to the fourth.

Then §§54–55 all the idempotent vids may be made nil factors to one another.

Now I say that in reference to each such idempotent vid every other may be made either idempotent or nilpotent.

For let  $i_1, i_2, i_3$ , etc. be the idempotent vids, nilfactorial to one another.

Suppose then  $i_1 A = B$

$$\text{Then } i_1 B = B \quad i_1(A - B) = 0$$

$$\text{Then } i_2 B = i_2 i_1 B = 0 \quad i_3 B = 0 \quad i_4 B = 0 \quad \text{etc.}$$

If  $i_2 A$  does not vanish suppose it = C. It cannot be of the form  $aA + bB$  for  $i_1 i_2 A = 0$  but  $i_1(aA + bB) = aB$ .

$$\begin{array}{llllll} \text{Then} & i_2 C = C & i_1 C = 0 & i_3 C = 0 & i_4 C = 0 & \text{etc.} \\ \text{Suppose} & i_3 A = C' & i_1 C = 0 & i_2 C = 0 & i_3 C = C & i_4 C = 0 \\ & i_4 A = C'' & i_1 C = 0 & i_2 C = 0 & i_3 C = 0 & i_4 C = C \end{array}$$

Then

$$\begin{aligned} i_1(A - B - \Sigma' C) &= 0 & i_2(A - B - \Sigma' C) &= 0 \\ i_4(A - B - \Sigma' C) &= 0 & \text{etc.} \end{aligned}$$

In this way the vids are divided into two classes  
 1st idempotents, nilfactorial to each other  
 2nd vids, which are at most idemfacient to one of the first class, and  
 idemfacient to one of the first class.

*Corollary.* Denote these second sort by  $a j_b$   $a k_b$   $a l_b$

$$\begin{array}{ll} \text{So that } i_{aa} j_b = a j_b & a j_b i_b = a j_b \\ i_{aa} k_b = a k_b & a k_b i_b = a k_b \\ & \text{etc.} \end{array}$$

All of this class are nilpotent and  $(a j_b)(a k_b) = 0$ .

### Theorem II

To every algebra we may add a vid I such that  $I^2 = I$ ,  $Ii_1 = i_1 I = I$  and for every value of x except 1,  $Ii_x = i_x I = 0$  and  $I_1 j_1 = j_1$ ,  $j_1 I = 0$ .

For if there is no inconsistency in the equations  $I^2 = I$ ,  $Ii_1 = i_1 I = I$ ,  $I_1 j_1 = j_1$ ,  $j_1 I = 0$  then there can be none in  $Ii_x = i_x I = 0$  for these last have no connection with the others nor with each other. But the rest give

	I	$i_1 - I$	$j_1$
I	I	0	$j_1$
$i_1 - I$	0	$i_1 - I$	0
$j_1$	0	$j_1$	0

This is a known algebra.

Put  $i_1$  for I,  $i_2$  for  $i_1 - I$ .

Then  $j_1$  becomes  $j_2$ .

Hence by the addition of vids all vids of the form  $j_1, k_1, j_2, k_2$ , etc. may be got rid of.

### Theorem III

Suppose an algebra contains the vids  $j_2$  and  $k_2$ . Then there is nothing inconsistent in adding a vid I such that

- (1)  $I^2 = I$
- (2)  $Ii_1 = i_1 I = I$
- (3)  $Ii_x = i_x I = 0$  where  $x$  is not 1
- (4)  $I_1 j_2 = j_2 I$     (6)  $j_2 I = 0$
- (5)  $I_1 k_2 = 0$     (7)  $k_2 I = 0$

For equation (3) has nothing to do with the others and they give

	$I$	$i_1 - I$	$j_2$	$k_2$
$I$	$I$	0	$j_2$	0
$i_1 - I$	0	$i_1 - I$	0	$k_2$
$j_2$	0	0	0	0
$k_2$	0	0	0	0

This is a compound of two known double algebras.

Now putting

$$\begin{aligned} i_1 &\text{ for } I \\ i_3 &\text{ " } i_1 - I \\ j_2 &\text{ becomes } j_2 \\ k_2 &\text{ " } j_2 \end{aligned}$$

And in this way all vids of the form  $k_2$  (additional to  $j_2$ ) can be got rid of.

Now write

$$i_1 = (1:1)$$

$$i_2 = (2:2)$$

$$i_3 = (3:3)$$

etc.

$$j_2 = (1:2)$$

$$j_3 = (1:3)$$

$$j_2 = (2:3)$$

etc.

And all the vids are reduced to the forms (X:X) (X:Y) where Z and W being different

$$(X:Z) (Z:Y) = (X:Y)$$

$$(X:Z) (W:Y) = 0$$

## Lazelle's One Law in Nature<sup>1</sup>

P 73: Nation 17 (10 July 1873): 28-29

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We cannot speak of Captain Lazelle's *One Law in Nature* with much respect. Though it does not betray the dense ignorance which many pretentious theories of the universe do, we cannot say that it has any value as a contribution to natural philosophy. We may defend this judgment by two citations. On page 17 we read:

Though tractive effort between masses of matter, without an intervening medium, cannot be understood, and though the mode of this invisible sympathy is as incomprehensible as is its nature, yet its existence is undeniable.

Now, in point of fact, there is nothing to determine whether gravitation acts through a medium or directly at a distance. All that we know is this: if it is propagated through a medium from one part to another adjacent to it, this process must, according to all analogy, occupy time. But, on the other hand, if there is no medium, the action cannot take time without violating the law of the conservation of energy, a law which, if it is not known positively to hold in such a case, may reasonably be supposed to do so. Now Laplace has shown that if the action is propagated through a medium, its velocity is, at least, many million times that of light, and that there is no reason for abandoning the simpler supposition that gravitation acts instantaneously. But Captain Lazelle's notion that any simple and obvious facts disprove the existence of a medium has no foundation.

The second citation shall be from page 19:

1. *One Law in Nature: A New Corpuscular Theory, comprehending Unity of Force, Identity of Matter, and its Multiple Atom Constitution, applied to the Physical Affections or Modes of Energy.* By Capt. H. M. Lazelle, U.S. Army. New York: D. Van Nostrand.

Though this force [gravitation] may extend through space independently of matter, yet it cannot be said to do so instantaneously; as successive positions must be occupied in successive increments of time.

These two opinions, that gravitation acts without a medium and yet that it takes time to act, do not harmonize. But observe the reasoning: Gravitation cannot act instantaneously because successive positions must be occupied in successive times! But what if these positions are not successive? Cannot there be attraction at different points at once?

Physicists are perfectly ready to examine general theories of the forces of nature, notwithstanding the fact that there is not a single instance of such a theory (imagined, and not derived by induction) which has finally taken a place among established truths. For example, the undulatory theory of light is proved up to a certain point, namely that light consists of some sort of vibration transverse to its direction of propagation. This is a result of induction. But no attempts to go further and imagine of what sort this vibration is, though the greatest mathematicians have made them, have met with such success as to be admitted to a place among established truths. Yet physicists always look upon such attempts to represent the mechanism of natural forces with favor; but they demand that they shall be developed with mathematical precision and be shown to express known laws with mathematical accuracy. This Captain Lazelle has not done.

All physicists believe that everything in the outward world may be expressed in terms of mass, of space, and of time. The redness of a rose, as it exists in the mind which sees it, is what it appears to be; but as it exists in the rose itself it is only the fact that the particles vibrate in a certain time. This time may be expressed as a number. And in a similar way, no doubt, every property of any body, might, if we only knew how to do it, be expressed numerically in terms of the pound, the yard, and the second. Of these physical constants or numbers expressing properties almost all are either peculiar to some particular thing (such as the dimensions of the earth) or to some kind of substance (such as the atomic weight of hydrogen). In the whole range of physics, we can expect to find no others and know of no others, except only two: first, the amount that one gramme attracts another gramme placed at a distance of a metre, which is 0.0000000000006 metre cubes per gram-(second)<sup>2</sup>, and the velocity

of light, which is 300000000 metres per second. By choosing the appropriate relation between our units of mass, space, and time, we can give these constants any numerical values we please. For example, we might make them both unity. But if we had a third universal constant, we could not make all three unity, at least without determining the absolute value of our fundamental units. Now it may be considered reasonable to suppose that considerations relating to the general laws of nature should lead us to adopt a certain ratio between our units. We have an example of this in the measure of lengths in different directions. A length north and south, a length east and west, and a length up and down, are three quantities as incomparable with one another as a time and a weight. We may therefore take a mile north and south as our unit of length in that direction and an inch east and west as our unit of length in that direction, and since these units cannot be compared they are unequal only in the sense in which a day and a pound are unequal. But now it is a great law of nature (our familiarity with which must not be allowed to breed contempt) that bodies may be turned from one direction to another, and that when a body is so turned without being subjected to any strain, the numerical value of its length north and south bears a certain constant ratio to the numerical value of its length east and west. This ratio necessarily depends on the relative magnitude of the units of length in different directions, and this fact has naturally led us to assume these units so as to reduce this ratio to unity. If there is only one law in nature it is this law of the rotation of bodies, and if this is the only one there is, times and masses are in no way subject to law. A natural force is in fact nothing but a general relation connecting measures of different quantities. We must, therefore, suppose at least two forces to establish relations of mass and of time to space. These are the two forces whose constants are the absolute modulus of gravitation and the velocity of light. But our whole conception of the universe, and therefore our whole experience, are opposed to there being another general relation, for such a one could only exist by establishing absolute values of our units. Now it is not to be believed that general considerations in regard to the nature of things could ever lead us to assign a particular numerical value to the measure of any particular thing, such as our standard measure. We have, therefore, reason to believe that while we doubtless are ignorant of the precise form of the fundamental principles of nature, we at least are not mistaken as to their number.

## *Rainfall*

*P 8I: Atlantic Almanac, 1874, p. 65*

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All storms are due to the combined action of heat and moisture. Suppose that the ground at a certain place is more heated than elsewhere in the neighborhood. This might happen from some difference in the nature of the surface or of the rocks. The heated ground heats the air above it. The air expands, and there is not only an upward current, but the whole atmosphere is raised up in consequence of the expansion. The atmosphere, being higher over this place than about it, flows off all around at the top. The consequence is that the pressure on the air is less at that place because there is less atmosphere, part of it having flowed off. In consequence of this diminished pressure the air flows in, near at the surface of the earth, from other places all around where the pressure is greater, and thus a wind towards the heated place is produced. So, if any place is cooler than other places about it, the air above it is cooled and contracts, the top of the atmosphere becomes lower, and air flows in at the top. There is consequently a greater atmospheric pressure at that place than elsewhere in the neighborhood, and consequently an outward wind springs up. I have spoken as though the disturbance necessarily extended to the very top of the atmosphere. This is not necessarily so, but everything which we readily see at the surface is just as if it were so, and happens too in consequence of the same mechanical principles, as if the top of the air were really raised or lowered.

Suppose, next, that instead of a place being hotter or cooler than surrounding places, there is a body of water surrounded by very dry land. Evaporation will take place from the surface of that water. The air becomes mixed with aqueous vapor, of which a certain amount is added to it. There is consequently so much added to the weight of the atmosphere at that point, and there will be greater atmospheric pressure at that place than at surrounding places. If, therefore, the

aqueous vapor had the same specific gravity as air, there would be a gentle wind away from such a body of water exactly representing the amount of aqueous vapor added to the atmosphere. But aqueous vapor is in reality only two-thirds of the specific gravity of air, so that the pressure of the atmosphere is increased by only two-thirds of the amount that it would be in the other case; and as the atmosphere from the lake has a larger proportion of aqueous vapor than from the surrounding land, and therefore a less specific gravity, the surface of the atmosphere will be elevated so as to make an equal weight on the different parts of the earth's surface. The atmosphere will therefore flow off at the top from this place. But we have hitherto failed to take account of two important circumstances, viz. the fact that the air is everywhere colder as we ascend from the surface of the earth, and the fact of the condensation of aqueous vapor at lower temperatures into rain or snow. The reason why the atmosphere is colder at higher latitudes than at lower ones is this. If you put a pressure upon any gas so as to condense it, if, for example, you have air in a cylinder in which a piston works and forcibly press down the piston, heat will be produced in an amount which is the equivalent of the work which has been expended in pressing down the piston, and conversely, if you take the pressure off, the condensed gas which is allowed to expand itself performs a certain amount of work, and the equivalent of that work in heat is lost to the gas, so that it is cooled. Now the pressure under which the atmosphere is, is owing entirely to its gravitation, so that in the higher parts of the atmosphere there is less pressure than in the lower parts. Suppose, therefore, that a certain amount of air be carried up through the surface a thousand feet, an equal amount of air will go down to fill its place. The air which goes up will be under less pressure and will expand, and in doing so will do a certain work, viz. that of assisting to support the upper strata of air, which the other body of air, which has been carried down, previously did, and the air that is carried up will lose an equivalent amount of heat in consequence, while the air that is carried down will gain the same amount. Now there are so many upward and downward currents in the air, that the heat in this way gets so distributed that when there is an upward current of air, and the air in consequence of going upward is expanded and becomes cooler, it finds itself in general at the same temperature as the air which is already at that same elevation. When the aqueous vapor over our lake, therefore, rises, in consequence of its lightness, it is cooled, and if it rises high enough is so much cooled as to be condensed into water, which falls

to the ground, and there is then a deficiency of atmospheric pressure at that place, and the wind will blow in towards the lake to supply the loss. Now let us consider the combined action of heat and moisture in producing storms. A place being heated, a wind inwards and upwards is produced there, and in consequence of the greater heat, greater evaporation goes on there. The moisture which is carried up becomes condensed in rain, and keeps the air at the surface continually saturated. The storm will therefore continue, when it has once been begun, as long as moisture is supplied. This is the explanation of the essential features of a storm, but there are two very noticeable circumstances about storms which we have not yet noticed. The first is the rotatory movement of the wind about the centre of a storm, and the second is the movement of the centre itself. The rotatory or cyclonic character of storms is due to the rotation of the earth. If we were standing at the North Pole the stars would appear to move round in horizontal circles from left to right, but this would of course really be due to the rotation of the earth from right to left. In any other northern latitude the stars generally appear to move round in the same direction as at the pole, so that the ground really is moving around from right to left under our feet. At the equator there is no such rotation, and in the Southern Hemisphere its direction is reversed. In consequence of this, if we take a pendulum which is free to move in all directions and swing it, the momentum of the bob will tend to keep it moving on in one plane, and as the earth revolves from right to left under it, the plane will appear to rotate (when its direction is referred to terrestrial objects) from left to right, or, in other words, there will be a continual apparent deflection of the motion of the bob towards the right in the Northern Hemisphere. It is just the same with any other object which is moving with a high degree of momentum. It will always tend to be deflected to the right when the direction of the motion is referred to terrestrial objects, and as the particles of air in a storm have a high velocity, any current of air will be deflected to the right. Consequently, when the wind blows inwards towards the warm centre, that which blows from the south will be deflected towards the east, that which blows from the east will be deflected towards the north, that which blows from the north will be deflected towards the west, and that which blows from the west will be deflected towards the south, and so there will be a rotation of the wind about the centre of the storm from south to east, from east to north, from north to west, and from west to south. In other words, the wind will blow round from right to left. Although

this motion is originally due to the deflection of each of its particles from left to right when the wind blows outwards from the cooled centre, that which blows towards the north is deflected to the east, that which blows towards the east is deflected to the south, that which blows to the south is deflected to the west, and that which blows to the west is deflected to the north, so that there is a whirlwind from right to left, or in the opposite direction to the whirl about the warm centre, and yet these two opposite whirls are equally due to the deflection of the separate particles towards the right. Whoever will look over a series of General Myer's weather-maps will find it an invariable rule, that whenever there is a wind the barometer is lower where the wind is blowing to, than where it is blowing from; and it is an equally invariable rule, that the barometer is higher on the right-hand side of a current than on its left-hand side. It is also a general fact, that at a centre of high pressure the wind blows directly outwards, while at a centre of low pressure it blows much more round the centre. These facts are precisely what the theory requires.

The motion of the centres of storms is partly due to the motions of the great currents of air which are owing to the general distribution of heat upon the surface of the earth, and to the rotation of the earth, but it is doubtless also due to the tendency which the centre of a storm will have to move to where it is warmer and moister.

The amount of rain which falls at any place and at any season of the year depends first upon there being evaporation sufficient to produce the rain, and second upon there being condensation of the moisture after it has evaporated. On account of the necessity of moisture there will be more rain in the neighborhood of rivers than elsewhere. This is especially the case in reference to the great rivers which fall into the Gulf of Mexico. Mr. Schott of the Coast Survey, in his work upon the rainfall of the United States, published by the Smithsonian Institution, has given maps showing, by shading, the rainfall in different parts of the United States for the whole year, for the three summer months and for the three winter months. It is a curious fact that there is a remarkable and even a minute resemblance between the map of the rainfall during the three winter months and the map of the distribution of illiteracy over the United States, given in the census of 1870. Such a resemblance could not exist unless the rainfall, or the cause of it, had some influence direct or indirect upon illiteracy. It may be, for example, that where there is a copious winter rainfall, agriculture becomes more easy, and that where the earning of a bare subsistence requires so little effort there

is a greater proportion of the population who are in a degraded state.

There is nothing more characteristic of the climate of the United States east of the Mississippi and north of the cotton States, than the equality with which the rainfall is distributed through the year. Nevertheless, the new reductions and generalizations of the observations worked out by Mr. Schott show that there are, at least upon the Atlantic seaboard, three yearly maxima and three minima of rainfall. Though these comparatively rainy and dry seasons begin and end gradually, and are, moreover, some years earlier and some years later, yet any one who has lived long near the Atlantic coast will recognize them easily. If we leave out of account every other month, beginning with January, as of undecided character, the remainder are alternately dry and wet. Of course, no month is dry in the sense in which the summer is dry in California and in Southern Europe, nor is any month wet as July is wet in Florida; still the distinctions are quite clear. February has much cold, settled, clear weather. April is celebrated for easterly storms and showers. The fine days and beautiful nights of June are remembered by everybody. August is the month of heavy and often prolonged thunderstorms, the time of dog-days. October is clear and dry, the Indian summer. December is a very stormy month. For a large part of the country the year might perhaps be better divided into six seasons than into four. On the coast of Maine, where the winter is earlier and the spring later, the distinction between the stormy season of November and December and the sharp frosts of midwinter is even more marked, but there is no more rain in the dog-days than in June. Passing southward, on the other hand, to Carolina, Georgia, and Florida, the dog-days gradually assume the character of a tropical rainy season.

At Fort McHenry, Md.,	4.2	inches	rain falls in August.			
" Fortress Monroe, Va.,	5.7	"	"	"	"	"
" Fort Moultrie, S.C.,	7.6	"	"	"	"	"
" Charleston, S.C.,	7.0	"	"	"	"	"
" Savannah, Ga.,	8.3	"	"	"	"	"
" Fort Brooke, Fla.,	10.6	"	"	"	"	"

In that part of the country the spring is so early and the autumn so late, that the other seasons are crowded and confounded together in great measure. The only other features strongly marked are dry seasons, which precede and follow the summer rainy season, and a pretty wet winter. A closer examination, however, seems to reveal

traces of all the seasons which we have at the North. Passing round to the Gulf coast, we find the rainy season of July and August still extremely developed, though somewhat less so than in Georgia, but we now have an almost equally rainy season in December. This is undoubtedly identical with the so-called subtropical rainy season. But it is equally certain that it is the same as the stormy December of the North. In New Orleans and Baton Rouge, the dry February, the wetter April, and the less wet May are also plainly distinguishable. Farther up the Mississippi, at Natchez and Vicksburg, the summer rains are much diminished. December is the wettest season, and the April maximum exceeds that of August, but all the time between December and April is wet. Let us now return to Boston, and trace the effect of passing into the interior. In Boston the three dry seasons are equally dry, and of the three wet seasons April is a little wetter than August and August than December. Already at Cambridge, three miles distant, a difference begins to be perceptible. October is a little less dry than the other dry seasons, and of the three wet seasons August is decidedly the wettest, December next, and April the least so. At Worcester the wet seasons have the same order of intensity; of the dry seasons, October is the least dry, and February the most. At Amherst these differences are much exaggerated, for February is by far the driest time, and July the wettest. This dog-day period, moreover, is extended earlier, so that the June minimum disappears. At Williamstown, both the June and October minima vanish, and there is simply a wet summer and a dry winter, the other differences being barely traceable. This is the general type of the seasons throughout the West, with various modifications, however. In northern districts there is, for instance, a somewhat less rainy interval in the middle of summer. In the valley of the Mississippi the December rains appear again, and October is the driest season. Farther west the system of seasons seems to be quite different. In Arkansas, April and November are the times of maximum rainfalls, September and January minimum. In Indian Territory the maxima fall is in May and October, the minima in August and January. In California the maximum is in December, and there is no rain at all from the 1st of June to the 1st of October. In Washington Territory the maximum is in December, the minimum in July or August. At Sitka the maximum is early in October, the minimum late in June; but three and one-half inches fall even in that month.

## [On Political Economy]

*MS 267: 21 September 1874*

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Political Economy treats of the relations of these three quantities

- x the Value per unit.
- y the Demand
- z the Cost of production of the last unit.

We shall first assume the following propositions:

1. The demand is a continuous function of the Value or Price and is independent of the Cost of production or

$$(1) \quad y = Fx$$

2. The Cost of production is a continuous function of the Demand or amount produced and is independent of the price, or

$$(2) \quad z = \varphi y$$

[3]. The seller is absolutely wise and sets the price so as to make his total profits (or  $xy - \int_y z = p$ ) a maximum. Then

$$(3) \quad D_x p = y + D_x y (x - z) = 0$$

$$(4) \quad D_x^2 p < 0$$

Then we have the following variations

$$(5) \quad \frac{\partial x}{\partial y} = + \frac{D_x y D_y z - 1}{D_x^2 p}$$

$$(6) \quad \frac{\partial x}{\partial(D_x y)} = -\frac{x - z}{D_x^2 p}$$

$$(7) \quad \frac{\partial x}{\partial z} = \frac{D_x y}{D_x^2 p} = \frac{D_x y}{2D_x y + D_x^2 y(x - z)}$$

$$(8) \quad \frac{\partial x}{\partial f_y z} = 0$$

It is evident that  $x$ ,  $y$ , and  $z$ , will always be positive and when (3) will hold  $D_x y$  is negative if  $x > z$ . Consequently we have the following propositions.

From (5) An absolute increase of the demand will always increase the price unless the increased price would (by diminishing the sales again) increase the final cost of production more than by the increase of price. This supposes the sensitiveness of the market to be unchanged.

From (6) we have: An increased sensitiveness of the market will invariably lower the price.

From (7) An increase in the cost of production invariably raises the price. If  $x = z$ , that is if the demand equals the supply the additional cost would be equally divided between buyer and seller were it not that the buyers partly escape by leaving off buying which hurts the seller's profits. When  $x > z$  the increased cost falls more on the seller, except when the sensitiveness of the market increases with the price & then it only appears not to do so because it falls more on the *profits* of the seller & not so much on his *price*. But as  $D_x^2 y$  is always small the price changes about half as much as the cost.

We will next consider the effect of exceptions to our three propositions. And first of points of discontinuity.

If there is a sudden change in the value of  $\varphi y$  so that for values of  $y$  less than  $y_c$ ,  $z = \varphi_1 y$  and for values greater than  $y_c$ ,  $z = \varphi_2 y$ , and if the first value of  $z$  gives  $y$  a value greater than  $y_c$  while the second gives a value less than  $y_c$ ,  $y_c$  is the resulting value of  $y$ . In this case, ( $D_x y$  being negative)

$$y_c + D_x y_c (x - \varphi_1 y) < 0$$

$$y_c + D_x y_c (x - \varphi_2 y) > 0$$

and  $\varphi_2 y > \varphi_1 y$ .

If there is a sudden change in the value of  $F_x$  so that for values of  $x$  less than  $x_c$ ,  $y = F_1x$  and for  $x > x_{fc}$ ,  $y = F_2x$ , and if the first equation would make  $x > x_c$  and the other  $x < x_c$ , then  $x_c$  is the resulting value. In this case

$$F_1x + D_x F_1 x(x - z) > 0$$

$$F_2x + D_x F_2 x(x - z) < 0$$

This case cannot occur.

If a sudden change takes place in  $F'x$  of the same sort, for  $x = x_c$  we have

$$y + F'_1 x(x - z) > 0$$

$$y + F'_2 x(x - z) < 0$$

$F'_1 x > F'_2 x$  or the market must become suddenly more sensitive if the price is raised.

We will next suppose that  $z$  depends on  $x$  as well as on  $y$ , and  $y$  on  $z$  as well as on  $x$ .

The dependence of  $y$  on  $z$  will make

$$\frac{\partial x}{\partial z} = \frac{D_x y - D_z y - D_z D_x y(x - z)}{D_x^2 p}$$

The dependence of  $z$  on  $D_x y$  will make

$$D_{D_x y} D_x p = (x - z) - D_x y D_{D_x y} z$$

$$\frac{\partial x}{\partial D_x y} = \frac{D_x y D_{D_x y} z - (x - z)}{D_x^2 p}$$

The dependence of  $z$  on  $x$  gives

$$D_x^2 p = D_x y (2 - D_x z) + D_x^2 y (x - z)$$

altering the value of the denominator of the variations.

1874 Sep. 21

It is true as said at the beginning of this cahier that Political economy treats of the relations of Price, Demand, and Cost of Production.

Demand depends on price and not on cost; cost depends on demand and not directly on price; price is fixed according to seller's idea of demand and cost.

Thus the facts of political economy are of three categories.

- I Dependence of demand on price
- II Dependence of cost on demand
- III Dependence of price on demand and cost, or other circumstances.

The dependence of demand on price arises from this fundamental proposition. The desire of a person for anything has a quantity of one dimension, and a person having a choice will take that alternative which will give him the greatest satisfaction. In other words if a person prefers A to B and B to C he also prefers A to C. This is the first Axiom of Political Economy.

The desirability of a thing depends partly on the possession of other things which are related to it in this respect either as alternative or as coefficient.

*Alternatives* So far as things serve the same purpose more or less well the possession of one diminishes the desire for the others. So far as things are alternative the possession of the less valuable will leave a desire for the more valuable equal to the difference of desirability. This is strictly true. A person may desire superfluity but that is another desire & so far the things are not alternative.

Alternative things may be fit either of them to satisfy one of several desires but not any one thing more than one desire. In that case the first thing will be wanted for the greatest utility and consequently the second thing will be desired less. Under this head comes such a case as my desiring say strawberries. I want one very much, a second less, and so on until I reach a limit beyond which my desire ceases.

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*On the Application of  
Logical Analysis to Multiple Algebra*

*P 90: Presented (by title) 11 May 1875*

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The letters of an algebra express the relation of the product to the multiplicand. Thus  $iA$  expresses the quantity which is related to  $A$  in the manner denoted by  $i$ . This being the conception of these algebras for each of them we may imagine another “absolute” algebra, as we may call it, which shall contain letters which can only be products and multiplicands not multipliers. Let the general expression of the absolute algebra be  $aI + bJ + cK + dL + \dots$  etc. Multiply, this, by any letter  $i$  of the relative algebra and denote the product by

$$\begin{aligned} & (A_1 a + A_2 b + A_3 c + \text{etc.}) I \\ & + (B_1 a + B_2 b + B_3 c + \text{etc.}) J \\ & + \text{etc.} \end{aligned}$$

Now we may obviously enlarge the given relative algebra so that

$$\begin{aligned} i = & A_1 i_{11} + A_2 i_{12} + A_3 i_{13} + \text{etc.} \\ & + B_1 i_{21} + B_2 i_{22} + B_3 i_{23} + \text{etc.} \\ & + \text{etc.} \end{aligned}$$

where  $i_{11}, i_{12}$ , etc. are such that the product of either of them into any letter of the absolute algebra shall equal some letter of that algebra. That there is no self-contradiction involved in this supposition seems axiomatic.

In this way each letter of the given algebra is resolved into a sum of terms of the form  $aA : B$ ,  $a$  being a scalar, and  $A : B$  such that

$$\begin{aligned} (A : B) (B : C) &= A : C \\ (A : B) (C : D) &= 0 \end{aligned}$$

The actual resolution is usually performed with ease, but in some cases a good deal of ingenuity is required. I have not found the process facilitated by any general rules. I have actually resolved all the Double, Triple, and Quadruple algebras and all the Quintuple ones that appeared to present any difficulty. I give a few examples

 $bi_5$ 

	$i$	$j$	$k$	$l$	$m$
$i$	$j$	0	$l$	0	0
$j$	0	0	0	0	0
$k$	$j + al$	0	0	0	$bj + cl$
$l$	0	0	0	0	0
$m$	$a'j + b'l$	0	$c'j + d'l$	0	$l$

$$i = cd' A:B + b' B:C + b' D:E$$

$$j = b'cd' A:C$$

$$k = cd' A:B + acd' D:B + b'c^2d' D:F + cd' E:C + bb'cd' A:F$$

$$l = b'cd' D:C$$

$$m = a'cd' A:B + b'c' A:E + b'cd' D:B + b'd' D:E \\ + b'cd' D:F + F:C$$

 $bd_5$ 

	$i$	$j$	$k$	$l$	$m$
$i$	$j$	0	$l$	0	0
$j$	0	0	0	0	0
$k$	$j + rl$	0	$i + m$	$rij$	$-j - rl$
$l$	0	0	$j$	0	0
$m$	$(r^2 - 1)j$	0	$-l$	0	$-r^2j$

$$i = A:D + D:F + B:E + C:F$$

$$j = A:F$$

$$k = rA:B + rB:C + D:E - \frac{1}{r}D:F + E:F$$

$$l = A:E - \frac{1}{r}A:F + B:F \\ m = r^2A:C - A:D - B:E - C:F$$

 $bh_6$ 

	<i>i</i>	<i>j</i>	<i>k</i>	<i>l</i>	<i>m</i>	<i>n</i>
<i>i</i>	<i>i</i>	<i>j</i>	<i>k</i>	<i>l</i>	<i>m</i>	0
<i>j</i>	<i>j</i>	<i>k</i>				
<i>k</i>	<i>k</i>					
<i>l</i>	<i>l</i>	<i>ak</i>		<i>k</i>		
<i>m</i>	0					<i>k</i>
<i>n</i>	<i>n</i>					

$i = A:A + B:B + C:C + D:D$

$j = A:B + B:C$

$k = A:C$

$l = aA:B + A:D + D:C$

$m = A:E$

$n = E:C$

 $br_5$ 

	<i>i</i>	<i>j</i>	<i>k</i>	<i>l</i>	<i>m</i>
<i>i</i>	<i>j</i>				
<i>j</i>					
<i>k</i>	<i>l</i>		<i>m</i>		
<i>l</i>					
<i>m</i>					

$i = A:B + B:C$

$j = A:C$

$k = D:B + D:E + E:F$

$l = D:C$

$m = D:F$

## [Early Abstract of *Photometric Researches*]

MS 279: Summer-Fall 1875

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Mr. C. S. Peirce has since 1872 been making certain photometric researches which will be published in the Annals of the observatory. The following is an abstract of his memoir.

Chapter I shows the application of the known principles of physiological optics to the subject of star magnitudes. Mr. Peirce also gives the calculations from Maxwell's experiments upon which he based his announcement to the American Academy of Arts and Sciences in April 1872 that the wave length and the apparent color of any part of the spectrum are connected by such a law that if  $\lambda_x$ ,  $\lambda_y$ , and  $\lambda_z$  be the wave lengths of three points of the spectrum which are more refrangible than C and less so than G and which are either all more refrangible or all less refrangible than  $\lambda = 525$ ; and if  $C_x$ ,  $C_y$ ,  $C_z$  be the apparent color of these three points of the spectrum, we shall not only have

$$C_y = XC_x + ZC_z$$

as Maxwell showed, but also

$$\frac{X}{Z} = \frac{\lambda_y - \lambda_z}{\lambda_x - \lambda_y}.$$

This law was afterwards published in Poggendorff's *Annalen* by von Bezold and proved by Helmholtz's experiments. The following tables show the agreement of Maxwell's experiments with the theory.

## Observer K

*Red to Green*

Point of spectrum on Maxwell's scale	Per cent of blue		Per cent of green		Per cent of Red	
	Obs.	Calc.	Obs.	calc.	Obs.	calc.
24	00	00	00	+01	100	101
28	-01	00	24	25	77	75
32	00	-01	51	51	49	50
36	-02	-01	73	72	29	29
40	-01	-01	91	92	10	09
<i>Green to Blue</i>						
48	11	12	97	96	-08	-08
52	38	37	69	70	-07	-07
56	63	62	43	44	-06	-06
60	84	84	21	20	-05	-04
64	100	100	04	04	-04	-04

## Observer J

*Red to Green*

24	00	-01	0	+01	100	100
28	-03	-02	26	27	77	75
32	-02	-02	56	55	43	47
36	-01	-01	78	77	23	24
40	-02	-02	95	97	07	05

*Green to blue*

48	29	31	75	74	-04	-05
52	60	59	44	45	-04	-04
56	93	84	12	20	-05	-04
60	101	107	2	-3	-03	-04

Certain speculations as to the true law to which the above is an approximation are made.

Chapter II treats of the method of reducing the different observers' scales of magnitudes to one. The scale to which it is sought to reduce all the rest is such that if  $m$  be the numerical magnitude and  $N$  the number of stars in the northern heavens brighter than the star whose magnitude is  $m$ , then

$$m = -\frac{1}{3} + \frac{2}{3 \log 2.25} \log N.$$

The scales of the *Durchmusterung*, Argelander's *Uranometria*, and Heis are reduced to this scale by simply counting the numbers of each magnitude given in each of these catalogues in the northern heavens.

In order to reduce the scales of Ptolemy, Ulugh Beg, Tycho Brahe, and Hevelius, it is necessary to take account of their liability to omit stars. For this purpose each of these catalogues has been carefully compared with the *Durchmusterung* to see how many stars were omitted and for each of them an empirical equation has been obtained connecting the number of stars in the northern heavens brighter than a given star according to the ancient observer with the same number according to the *Durchmusterung*. The numbers of stars in the ancient catalogue having received this increase the formula given above becomes applicable & their scales have actually been so reduced.

Sir Wm. Herschel made many observations on the comparative brightness of Flamsteed's stars. He puts a comma between the Flamsteed numbers of two stars to signify that the first is a little brighter than the second, a dash to signify that the difference is considerable, a period to show the equality of the two stars, and various combinations of periods, commas, and dashes, to denote various differences. After a careful study of these observations Mr. Peirce has succeeded in reducing them to magnitudes by least squares, and has thus brought to light as it were an extensive and highly valuable series of observations.

The scale of magnitudes of Sir John Herschel, and the logarithms of the ratios of light of Seidel's and Zöllner's photometric observations have been reduced to the required scale by empirical linear equations.

A comparative catalogue is given showing all the stars in Heis's catalogue together with some others, with the magnitudes of Ptolemy, Ulugh Beg, Tycho Brahe, Hevelius, Sir Wm. Herschel, Arge-

lander's *Uranometria*, Heis, the *Durchmusterung*, Sir John Herschel, Seidel, Zöllner, and Peirce all reduced to a uniform scale. This catalogue contains about four thousand stars.

Another catalogue is given comparing the judgments of the color of the stars made by Secchi, Schmidt, Sestini, and Peirce.

Mr. Peirce has also in this chapter described a method for investigating the form of the galactic cluster and has applied it to the imperfect data now in our possession. He does not find it necessary to assume, either as Sir Wm. Herschel originally did that the density of the stars is the same in every part of the cluster, or as Struve practically did that the intrinsic brightness of all stars is the same, but he has assumed that the proportions of stars of the different degrees of intrinsic brightness are everywhere the same so that the different parts of space differ only in the number of stars they contain but not in the relative brightness of those stars. This being the assumption, the object of inquiry is to determine the forms of the surfaces of equal condensation of the cluster. Now there are two classes of facts to be used as premisses. First the relative numbers of stars of different apparent brightness; and second the relative numbers of stars of each degree of brightness at different distances from the pole of the milky way. The facts at present in our possession are the following.

#### *Data of the first class*

1. Seidel's very accurate photometric measures of 200 of the brightest stars visible at Munich.
2. Peirce's photometric measures of about 500 stars between 40° and 50° North Declination.
3. Rosén's measures of about 100 stars from the 5th to the 10th magnitudes in different parts of the northern heavens.
4. The Herschelian gauges which introduce a rough photometric determination.

#### *Data of the Second Class*

1. Positions of the 30 brightest stars with reference to the milky way. The list made by C.S.P.
2. Gauges of bright, 7th mag., 8th mag., and 9th mag. stars extracted by Argelander from the *Durchmusterung* and given in the introduction to the third volume of that work.
3. The Herschelian gauges.

The surfaces of equal condensation approach to being planes lying parallel to the milky way. The proposed method of research consists in comparing the data at hand with the results of supposing the surfaces in question to be accurately such planes. It is shown that on that hypothesis the density of stars having an apparent brightness greater than  $L$  at any distance  $\theta$  from the pole of the milky way gives when multiplied by  $L^{3/2}$  some function of

$$\frac{\cos \theta}{\sqrt{L}}.$$

But if the surfaces in any direction are parallel but have an inclination to the plane of the milky way (away from the sun, lateral inclination being left out of account) equal to  $v$  then the above quantity becomes a function of

$$\frac{\cos (\theta - v)}{\sqrt{L}}.$$

If all the surfaces were parallel in any direction observation would readily afford us the value of  $v$ . The surfaces not being parallel, but those at one distance having a different inclination from those at another distance, we shall get different values of  $v$  for stars of different apparent brightness. If we allowed ourselves to assume that the stars all had the same intrinsic brightness we could still under these circumstances get numerical values of  $v$  at different relative distances. But this is more than we need seek. We only desire to know at first the general shape of the surfaces of equal condensation and consequently not the numerical values of  $v$  so much as some idea of the nature of its variations with the distance. Now since the apparently bright stars are necessarily and independently of any hypothesis on the whole nearer to us than the apparently faint stars, we can by considering stars of various degrees of brightness ascertain whether  $v$  increases or decreases as the distances increase.

Following out such considerations, Mr. Peirce reaches the following conclusion from the very imperfect data now in our possession. If a system of Cassinian ovals be first immensely stretched in the direction of their major axis and then rotated around their minor axis, the surfaces of revolution which they generate will resemble the surfaces of equal condensation of the galactic cluster.

Chapter III describes Mr. Peirce's own observations which were made with a Zöllner's astrophotometer. The sources of error in using this instrument are first minutely considered, and a modified form of it is proposed. This consists chiefly in allowing the artificial star to remain fixed and altering the brightness of the real star until it equals the artificial one. The particular instrument used with its defects and instrumental corrections, and also the observatory, table, etc., are described. The plan of observations is detailed. All the stars in Arge-lander's *Uranometria* between 40° and 50° of north declination were divided into seventy groups of neighboring stars, these groups lying thus:—



Then all the stars in each group were compared with all the stars of each of the groups adjacent to it. This required 140 sets of observations. Besides that groups I and XXI, XI and XXXI, etc., were compared. Owing to repetitions there were in all 190 sets of observations.

In the first reduction of the observations, 2.25 was assumed for the ratio of light between successive magnitudes and the mean magnitude of the stars of one group was arbitrarily taken as zero. Then by a mathematical application of the method of least squares which is fully described in the memoir, the magnitudes of all the stars observed were obtained on this scale. Afterwards this scale was reduced to the "scale of equable distribution" of the comparative catalogue in the same way that other photometric observations had been reduced.

*Chapter IV* discusses chiefly the probable errors of the different observers of magnitudes. These can be obtained according to Arge-lander's method by comparing three observers, or according to a new method by comparing only two observers provided that these observers have each made a catalogue of all the stars in the northern heavens. Extensive tables comparing the different observers are given and the results are carefully deduced. It is shown that the general variability of stars is perceptible and that it is of a cyclical character for most stars.

The observations of Mr. Peirce are shown to be less accurate than those of Seidel but more so than those of any other observer.

This abstract only notices the main points of the memoir and passes over many less interesting computations and experimental determinations which it contains.

## Notes on the Fundamentals of Algebra

*MS 287: Winter-Spring 1876*

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The copula is the first symbol. Supposing there is but one copula it can be shown that its definition should be,

1.  $x \prec x$  whatever  $x$  may be.
2. If  $x \prec y$  and  $y \prec z$ , then  $x \prec z$ .
3. If  $x \prec y$  and  $y \prec x$ , then  $x$  and  $y$  are indistinguishable,

(and we write  $x = y$ ).

The letters usually have material significance. But some have a merely formal significance being evolved from algebra itself. Such are

- 0, or zero, which is thus defined,  
 $0 \prec x$  whatever  $x$  may be.  
 $\infty$ , or general infinity, which is thus defined,  
 $x \prec \infty$  whatever  $x$  may be.

If  $x \prec 0$ ,  $x = 0$ . If  $\infty \prec x$ ,  $\infty = x$ .

### *Ligation*

Ligations of letters are either single or branching.

Example of simple ligation     $x \times y \times z \times u \times v \times w$

Example of branching ligation     $x \left\{ \begin{array}{c} \times y \\ \times z \end{array} \right\} u \left\{ \begin{array}{c} \times v \\ \times w \\ \times x \end{array} \right\}$

All branching ligation may be reduced to bifurcation

$$x \left\{ \begin{smallmatrix} \times y \\ \times z \\ \times w \end{smallmatrix} \right\} = x \left\{ \begin{smallmatrix} \times y \\ \times \\ \left\{ \begin{smallmatrix} \times z \\ \times w \end{smallmatrix} \right\} \end{smallmatrix} \right\}$$

### Simple Ligation

Simple ligation is of the following kinds

Supposing  $l \prec m \quad x \prec y$

1. Such that  $lx \prec my$ . This is called *multiplication*.
  2. Such that  $l^y \prec m^x$ . This is called *progressive involution*.
  3. Such that  $m^x \prec l^y$ . This is called *regressive involution*.
- Such that  $m \cdot y \prec l \cdot x$ .
- 

## Notes on the Fundamentals of Algebra

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The fundamental symbol is  $\prec$  which is thus defined

1.  $x \prec x$  whatever  $x$  may be.
2. If  $x \prec y$  and  $y \prec z$ , then  $x \prec z$ .
3. If  $x \prec y$  and  $y \prec x$ , then  $x$  and  $y$  are not distinct.

### Ligation

Ligations of letters are either in a single series or branching.  
Thus,  $x \times y \times z \times w \times u \times v$  is an example of simple ligation.

$x \left\{ \begin{smallmatrix} \times y \\ \times z \end{smallmatrix} \right\} w \left\{ \begin{smallmatrix} \times u \\ \times v \end{smallmatrix} \right\}$  is an example of branching ligation.

*All branching ligation may be reduced to bifurcation.*

$$x \left\{ \begin{smallmatrix} \times y \\ \times z \\ \times w \end{smallmatrix} \right\} = x \left\{ \begin{smallmatrix} \times y \\ \times 1 \\ \times w \end{smallmatrix} \right\} \times z$$

### *Simple Ligation*

Simple ligation is of three kinds viz:—

1. Such that if  $l \prec m$  and  $x \prec y$ ,  $lx \prec my$ . This is called *multiplication*
2. Such that if  $l \prec m$  and  $x \prec y$ ,  $ly \prec mx$ . This is called *progressive involution*
3. Such that if  $l \prec m$  and  $x \prec y$ ,  $mx \prec ly$ . This is called *regressive involution*

(A fourth kind such that  $m \cdot y \prec l \cdot x$  seems useless because it is perfectly represented by  $X^{my} \prec X^{lx}$ . The necessity of distinguishing the two involutions arises from a circumstance not yet considered.)

These are the distinguishing characters of the three ligations and are the characters from which all the others are developed. But they do not constitute complete definitions of the operations. These definitions have now to be evolved.

## The Axioms of G/eome/try

*MS 293: Spring 1876*

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*Axiom 1.* All the points of space have the same properties. Any proposition which can be enunciated without the arbitrary designation of but one point, A, is said to express a property of that point. Then, the truth of such proposition does not depend on what point A denotes.

*Axiom 2.* All pairs of points have the same properties.

*Corollary.* The sum of the three angles of a triangle is equal to two right angles. For it is well-known that this is the case unless different lengths have different properties.

*Axiom 3.* Space has three dimensions. That is to say, three numbers are sufficient to define the position of a point with reference to four known points.

*Axiom 4.* Space permits parallel motion. Two systems of points A, B, C etc. and A', B', C' etc. are said to be *similar* when every proposition which can be expressed without arbitrarily designating any points except those of the first series, holds equally true when the corresponding designations of the points in the second series are substituted everywhere for those of the first series. Equality in the relations of points is a relation such that 1st, if A equals B, B equals A; 2nd, if A equals B and C equals D, then A equals C and B equals D; and 3rd, there are not two points whose relation to one third are equal. A system of points A, B, C is said to receive a parallel motion, when the points are gradually changed the new set A', B', C' being always similar to the first and the relations of A to A', B to B', etc. being all equal. Geometry supposes that there is some means direct or indirect of recognizing parallel motion.

*Axiom 5.* Space permits rotation. If the point A can be carried to the point B and then by a continued parallel motion to C, A, B, and C are said to be in a line. If four points A, B, C, and D are such that a

combination of a parallel motion such as would carry A to B with a parallel motion which would carry B to C, will also carry C to D, then ABCD are said to lie in one plane. In any plane let one point remain fixed, while the others move in such a way that, the system of points remaining similar to what it was at first, and the motion always being such that the lines never go backward, every line returns into itself. Such a motion is called turning. If O is the fixed point, and A and B are so related to it that when the line of O and A is by the motion brought to coincide with the line of O and B, then the line of O and B coincides with the line of O and A but so that the fixed point lies between the two movable ones, then the lines OA and OB are said to be perpendicular to each other. If the line OA *turns* in the plane OAB about the fixed point O in such a manner that the direction of the motion of A is always perpendicular to the line OA, then OA is said to revolve about O.

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## *Logical Contraposition and Conversion*

*P 99: Mind 1 (July 1876): 424–25  
and MS 291: Spring 1876*

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On page 148 of *Mind*, the Editor proposes to resolve the inference,

All S is P,  
∴ No not-P is S,

into two steps, thus,

All S is P,  
∴ No S is not-P,  
∴ No not-P is S.

To this I object on the ground that both steps of the latter process depend on a property of the negative which is not essential to the validity of the inference proposed to be resolved. In the universal negative proposition, *homo non est animal*, the *non* qualifies the copula. The meaning of this qualification must, however, be defined to be such that the proposition is equivalent to *homo est non animal*, taken in such a sense that the existence of a man is not asserted. We may, therefore, substitute for the forms of inference in question,

All S is P,  
∴ All not-P is not-S;

and

All S is P,  
∴ All S is not-not-P,  
∴ All not-P is not-S.

The word *not* here has two properties. The first is that it is a relative term. To say that an animal is not a plant, is to say that it is *other than* every plant, just as we might say that it was *superior to* every plant. The second property is that the relative term *not*, like *cousin of*, *similar to*, etc. is its own converse. Now the first inference does not depend on this second property since it is of a form which holds good for all relative terms whatever. Thus we may reason,

All Negroes are men,  
 $\therefore$  Every lover of all men is a lover of all Negroes.

On the other hand both of the steps of the proposed resolution do depend on the convertible character of negation.

Here is a second refutation of the proposed resolution. The proposition 'No non-P is P' which means simply 'All non-P is non-P' is an identical one which depends on neither property of the negative. But grant this proposition, and we have the following syllogism in *Camestres*:—

All S is P,  
 No non-P is P,  
 $\therefore$  No non-P is S.

But this is the contrapositive inference; and since a syllogism is not resolvable into two steps of inference, the inference in question is not so.

When one of the premises of a syllogism is an identical proposition, the syllogism is equivalent to an immediate inference from the other premise alone. This affords not only a convenient means of studying immediate inference but is still more useful in showing what principles are involved in the different moods of syllogism. I propose to indicate briefly the results of such a procedure.

It must first be remarked with regard to the particular proposition, that 'Some A is B' means 'All  $v$ -A is B' where  $v$  is a certain existing class of A's. The particular 'some-A' or ' $v$ -A' has therefore two properties, viz., 1st,  $v$ -A is a class which includes certain unknown A's, and 2nd  $v$ -A includes nothing but A's. The first property alone is sufficient to justify the following inference, which I ask leave to call the *contraposition of A into I*:—

‘All A is B,  
 $\therefore$  Some B is v-A’.

On the other hand, the second property is essential to the conversion of I, or

v-A is B,  
 $\therefore$  v-B is A.

I now pass to the results of substituting identical propositions for premises in the various moods of syllogism. This procedure applied to the first figure gives nothing but repetitions of the other premise in the conclusion, which shows how simple and so to say pure from all admixture of special characters these moods are.

It should be observed by the way that the only identical propositions are,

All X is X,  
 No non-X is X,  
 Some X is v-X.

The propositions, No X is non-X, and Some v-X is X, are for the present purposes not to be regarded as identical ones, since they depend on properties of the negative and the particular.

In the second figure, we have just now seen that *Camestres* gives the contraposition of A into E, showing that it involves the first property of the negative. It does not involve the second property, for it can be reduced to the first figure without conversion; thus,

<i>Camestres</i>	<i>Reduction</i>
All M is P,	No not-P is M,
No S is P,	All S is not-P,
$\therefore$ No S is M.	$\therefore$ No S is M.

*Baroko* may be reduced in the same way. On the other hand *Cesare* and *Festino* involve the second property of negation, as is shown by such forms as,

No Y is X,	No non-X is X,
All X is X,	All Z is X,
∴ No X is Y.	∴ All Z is non-non-X.

In the third figure, *Disamis* and *Bocardo* only involve the first property of the particular. Thus we have,

Some X is v-X,	
All X is Y,	
∴ Some Y is v-X.	

The most philosophical way of reducing these moods to the first figure is, therefore, as follows:—

<i>Disamis</i>	<i>Reduction</i>
Some S is P,	Any v-S is P,
Any S is M,	Some M is v-S,
∴ Some M is P.	∴ Some M is P.

*Bocardo* is reduced in the same way except that not-P is substituted for P. On the other hand, *Datisi* and *Ferison* depend upon the second property of the particular and we have,

All X is X,	All X is Y,
Some X is Y,	Some X is v-X,
∴ Some Y is X.	∴ Some v-X is Y.

Of the moods of Theophrastus, *Celantes* depends on the properties of the negative, *Dabitis* on those of the particular, and *Frisesomorum* on the properties of both.

Inferences in *Darapti*, *Felapton*, *Baralipiton*, *Fapesmo* are resolvable into two steps.

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## Addition to the note for *Mind*

*MS 292: Spring 1876*

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The same method admits of a somewhat more general application. In every syllogism whatever, even including inductive and hypothetical inferences, the conclusion is reached by substituting one term for another in one of the premises, the substitution being justified by the other premise. In *Barbara*, the distinction of *substituent* and *substituend* premises disappears, for either of the two may be regarded as substituent or substituend. But in many cases, the distinction is real. Let us write 'A s B' for the substituent premise, where *s* is an abbreviation for 'may in certain known cases be substituted for'. We shall evidently have 'A s A', and also,

If A s B,  
and B s C;  
then A s C.

The substituend premise might be written in the form 'B  $\varphi$  C', and thus we should get as the general form of all inference

A s B,  
B  $\varphi$  C;  
 $\therefore$  A  $\varphi$  C.

Here there is no distinction of different forms of inference. If however we suppose the substituend premise to be of one of the two forms ' $\Phi$ B is C' or 'C is  $\Psi$ B', where  $\Phi$ B and  $\Psi$ B denote terms whose meaning depends upon the meaning of B in such a way as to sustain inference from the supposed substituent premise, we have the two forms of syllogism,

$$\begin{array}{ll} A \text{ s } B, & A \text{ s } B, \\ \Phi B \text{ is } C; & C \text{ is } \Psi B; \\ \therefore \Phi A \text{ is } C. & \therefore C \text{ is } \Psi A. \end{array}$$

Making the substituend premises identical propositions, we have the immediate inferences,

$$\begin{array}{ll} A \text{ s } B, & A \text{ s } B, \\ \therefore \Phi A \text{ is } \Phi B. & \therefore \Psi B \text{ is } \Psi A. \end{array}$$

These immediate inferences enable us also to reduce the forms of general syllogism to *Barbara*.

We might take ‘A s B’ in the special sense of ‘A is B’, or in that of ‘B is A’; as well as in many others. The senses which  $\Phi$  and  $\Psi$  could take would vary according to circumstances. There would be no difficulty in reducing *Camestres*, *Baroko*, *Disamis*, and *Bocardo* to these forms, but it could not properly be done for the other indirect moods. For these, we must use the distorted forms,

$$\begin{array}{ll} A \text{ s } B, & A \text{ s } B, \\ \Phi B \text{ is } C; & C \text{ is } \Psi B; \\ \therefore \Phi C \text{ is } A. & \therefore A \text{ is } \Psi C. \end{array}$$

Make the substituent premise ‘B s B’, and we have the immediate forms of inference,

$$(1) \quad \begin{array}{ll} \Phi B \text{ is } C & C \text{ is } \Psi B, \\ \therefore \Phi C \text{ is } B & \therefore B \text{ is } \Psi C. \end{array}$$

Putting ‘ $\Phi B$  is  $\Phi B$ ’ and ‘ $\Psi B$  is  $\Psi B$ ’ as the premises of (1) and we find as identical propositions ‘ $\Phi\Phi B$  is  $B$ ’, and ‘ $B$  is  $\Psi\Psi B$ ’. These last by means of *Barbara* establish the validity of the forms of immediate inference

$$(2) \quad \begin{array}{ll} B \text{ is } C, & A \text{ is } B, \\ \therefore \Phi\Phi B \text{ is } C. & \therefore A \text{ is } \Psi\Psi B. \end{array}$$

Transposing the conclusions of (2) according to the form (1) and we have,

$$(3) \quad \begin{array}{ll} B \text{ is } C, & A \text{ is } B, \\ \therefore \Phi C \text{ is } \Phi B. & \therefore \Psi B \text{ is } \Psi A. \end{array}$$

Transposing the substituend premises of the syllogisms according to the form (1), we have,

$$\begin{array}{ll} A \text{ s } B, & A \text{ s } B, \\ \Phi C \text{ is } B; & B \text{ is } \Psi C; \\ \therefore \Phi C \text{ is } A. & \therefore A \text{ is } \Psi C. \end{array}$$

Then putting 'B is B' for the substituend premises we have the forms of immediate inference,

$$(4) \quad \begin{array}{ll} A \text{ s } B, & A \text{ s } B, \\ \therefore B \text{ is } A. & \therefore A \text{ is } B. \end{array}$$

It is obvious that (1) and (4) together will enable us to reduce the above distorted forms of inference to *Barbara*. All such moods as *Cesare*, *Celantes*, *Datisi*, *Dabitis*, and *Frisesomorum* may be put into these forms.

This seems to me sufficient to illustrate the advantage of the substitution of identical propositions as a method of logical analysis.

## Sketch of the Theory of Non-Associative Multiplication

*MS 294: Spring-Summer 1876*

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1. Any factor  $i$  whatever may be conceived as consisting of a sum of elementary fractions like  $(A : B)$ , whose denominators are all the elements of quantities which being multiplied by  $i$  give a product other than zero and whose numerators are the result of the multiplication. We have then, by definition,

$$(A : B)B = A \quad (A : B)C = 0$$

The last equation holds because if  $i$  multiplied by  $C$  gives  $A$  or any quantity of which  $A$  is a part, it is necessary by our rule to include  $(A : C)$  among the elementary fractions which compose  $i$ . Then since  $(A : C)C = A$  and  $\{(A : B) + (A : C)\}C = A$ , it follows that  $(A : B)C = 0$ .

In the case of associative multiplication, since

$$\begin{aligned} \{(A : B)(B : C)\}C &= (A : B)\{(B : C)C\} = A, \\ \text{and } \{(A : B)(B : C)\}D &= (A : B)\{(B : C)D\} = 0, \end{aligned}$$

we have  $(A : B)(B : C) = (A : C)$ ; for it is the definition of  $(A : C)$  that multiplied by  $C$  it gives  $A$ , and multiplied by anything else it gives zero.

In the case of non-associative multiplication, this equation does not hold and, therefore, for the same reason that  $(A : B)C = 0$  we have,

$$(A : B)(B : C) = 0$$

2. The associative multiple algebra has attached to it an indefinite number of series of factors, such that all the factors of any one series follow the rule of association among each other, but give zero when multiplied by any other elementary quantity except one. Thus, the associative algebra being,

$$\begin{aligned} i_1 &= (A : A) & i_2 &= (B : B) & i_3 &= (C : C) & \text{etc.} \\ j_{12} &= (A : B) & j_{21} &= (B : A) & & \text{etc.} \end{aligned}$$

we have in the first place the supplementary series of elementary quantities,

$$\begin{aligned} \alpha_1 &= (i_1 : A) & \alpha_2 &= (i_2 : B) & \alpha_3 &= (i_3 : C) & \text{etc.} \\ \gamma_{12} &= (i_1 : B) & \beta_{13} &= (i_1 : C) & & \text{etc.} \\ \beta_{12} &= (j_{12} : A) & \gamma_{13} &= (j_{13} : A) & \gamma_{21} &= (j_{21} : B) & \text{etc.} \\ \delta_{12} &= (j_{12} : B) & \delta_{13} &= (j_{13} : C) & \delta_{21} &= (j_{21} : A) & \text{etc.} \\ \epsilon_{123} &= (j_{12} : C) & & \text{etc.} \\ \zeta_1 &= (A : i_1) & \zeta_2 &= (B : i_2) & & \text{etc.} \\ \eta_{12} &= (B : i_1) & \eta_{13} &= (C : i_1) & & \text{etc.} \\ \theta_{12} &= (A : j_{12}) & \theta_{21} &= (B : j_{21}) & & \text{etc.} \\ \iota_{12} &= (B : j_{12}) & \iota_{21} &= (A : j_{21}) & & \text{etc.} \\ \kappa_{123} &= (C : j_{12}) & & \text{etc.} \\ \lambda_1 &= \alpha_1 \zeta_1 = \gamma_{12} \eta_{12} = (i_1 : i_1) & \lambda_2 &= (i_2 : i_2) & & \text{etc.} \\ \mu_{12} &= \alpha_1 \eta_{21} = \gamma_{12} \zeta_2 = (i_1 : i_2) & \mu_{21} &= (i_2 : i_1) & & \text{etc.} \\ \nu_{12} &= (j_{12} : j_{12}) \text{ etc.} & \xi_{12} &= (j_{12} : j_{21}) \text{ etc.} \\ \sigma_{12} &= (i_1 : j_{12}) \text{ etc.} & \pi_{12} &= (i_1 : j_{21}) \text{ etc.} \\ \rho_{12} &= (j_{12} : i_1) \text{ etc.} & \sigma_{12} &= (j_{21} : i_1) \text{ etc.} \\ \tau_{123} &= (i_1 : j_{23}) & \nu_{123} &= (j_{23} : i_1) & \varphi_{123} &= (j_{12} : j_{13}) \\ \chi_{123} &= (j_{13} : j_{23}) & \psi_{1234} &= (j_{12} : j_{34}) \end{aligned}$$

All these quantities multiply *inter se* associatively, and they are connected with the primary vids of the associative algebra by various relations

$$\begin{aligned} i_1 &= \zeta_1 \alpha_1 = \eta_{21} \gamma_{21} = \theta_{12} \beta_{12} = \iota_{21} \delta_{21} = \kappa_{321} \epsilon_{321} \\ j_{12} &= \zeta_1 \gamma_{12} = \eta_{31} \gamma_{32} = \eta_{21} \alpha_2 = \theta_{12} \delta_{12} = \iota_{12} \beta_{21} \\ &= \theta_{13} \epsilon_{132} = \text{etc.} \end{aligned}$$

But the product of a secondary quantity into a primary one, and then into either a primary or secondary one, generally differs from its associative transformation.

Thus if  $x$  and  $y$  are primary quantities,  $\varphi$  and  $\psi$  secondary ones,  $(\varphi x)y$  and  $(\varphi x)\psi$  are generally distinct from  $\varphi(xy)$  and  $\varphi(x\psi)$ .

Thus, if  $\varphi = \chi_{432} + \chi_{431}$ ,

$$\varphi(j_{32}j_{21}) = j_{42}, \text{ but } (\varphi j_{32})j_{21} = j_{41}.$$

For a logical example, let  $\varphi$  denote *more intimate than; f, friend; w, wife*. Then of the two propositions,

$$A \prec \varphi(fw)B \quad \text{and} \quad A \prec (\varphi f)wB$$

the first implies that  $A$  has a more intimate relation with  $B$  than that of friend to his wife, and the second that  $A$  has a relation more intimate than that of a friend with the wife of  $B$ , which are different things.

All other combinations are associative. Thus,

$$\begin{aligned} (\varphi\psi)x &= \varphi(\psi x) \\ (a\varphi)x &= a(\varphi x) \\ a(\varphi\psi) &= (a\varphi)\psi \\ (ab)\varphi &= a(b\varphi) \end{aligned}$$

There is a series of tertiary quantities related to the secondary ones somewhat as these are to the primary ones, and so on *ad infinitum*.

3. These supplementary quantities appear in logic as “conjugative terms,” the existence and some of the properties of which I have signalized in my memoir on the Logic of Relatives.

In algebra, the secondary quantities are operations. We should take the first two of the four equations just given as definitions of  $(\varphi\psi)$  and of  $(a\varphi)$  respectively.

The idea of operations upon operations, or tertiary quantities, is familiar in algebra as when we compare the development by Taylor's

Theorem to raising the base to the  $\frac{d}{dx}$  power. Operations upon two variables are also tertiary quantities.

4. Terms which are definable by means of their form alone ought to be called *logical terms* if no operations peculiar to number are introduced, and in the opposite case *arithmetical terms*. Thus of primary quantities

$$\Sigma_A(A:A) = A:A + B:B + \text{etc.}$$

is a logical term which appears in logic as identity and in algebra as unity.

Of the supplementary quantities, there are evidently an infinite number of logical terms. In fact, while there are only two absolute logical terms, *being* and *nothing*, and of relatives or primary quantities there are only four, *identical with*, *other than*, *having a relation to*, and *having no relation to*, the number among secondary quantities mounts at once to thousands of millions.

Of these, decidedly the most interesting, without doubt, is the term  $\Sigma_{A,B}(A:B):(B:A)$  where B may be the same as A. This is the term to which De Morgan gave the name *converse of*. It plays a most important part in the theory of multiple algebra. It may be denoted by  $K = \Sigma_i \lambda_i + \Sigma_i \Sigma_j \xi_{ij}$ .

Another term of great importance is  $\Sigma_i \alpha_i = (A:A):A + (B:B):B + \text{etc}$ . It may be denoted by  $\alpha$ . Then according to my notation for the logic of relatives

$$x, = \alpha x.$$

Or "x that is ——" is the same as the Alpha of x.

Connected with Alpha is another term which may be called *Beta*. It is thus defined:

$$\begin{aligned} \beta = \Sigma_i \Sigma_j \beta_{ij} &= (A:B):A + (A:C):A + \text{etc.} \\ &+ (B:A):B + (B:C):B + \text{etc.} \\ &+ \text{etc.} \end{aligned}$$

The product  $\beta x$  is a certain relative term, which I overlooked in my logic of relatives, and which in words is "x which is not ——." Its property /is t/that  $\beta xy$  denotes whatever x is not y. So that,

$$\beta xy = \alpha x(1 - y) = x(1 - y).$$

5. Some formula for  $A:(B + C)$  may be demanded. In reference to this, it must be remembered that  $(A:B)$  does not denote simply that which multiplied into B gives A, but that which in addition to this property gives zero when multiplied into any other element. No value for  $A:(B + C)$  can therefore be assigned without some additional convention but we may naturally write

$$A:(B + C) = (A:B) + (A:C).$$

## The Principles of Mechanics

*MS 298: Summer-Fall 1876*

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Other expositions of the principles of mechanics have for their object to induce assent, but assent may be obtained by glossing over the real facts these principles assert. The object of the present statement is, on the contrary, to exhibit the facts implied in these principles.

The positions and velocities of bodies are subject to such conditions as are involved in the properties of time and space. Time has three independent properties, as follows.

1st. The fundamental temporal relation, being as late as \_\_\_\_\_, is a transitive one. That is, if A is as late as B, and B as late as C, then A is as late as C.

2nd. A temporal relation considered as a character of an object is a date. A date which is absolutely determinate is an instant. Now, *all instants are, in themselves, exactly alike*. That is to say, whatever can be truly predicated of any one instant without arbitrary designation, explicit or covert, of any other, is equally true of all instants. Thus, if one instant is followed by another which is later, so is every one, and thus time is infinite.

3rd. All pairs of different instants are, in themselves, exactly alike, except as to which is the later. That is to say whatever can be truly stated, by arbitrarily designating nothing but two instants, can also be truly stated [ . . . ]

## The Principles of Mechanics

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Down to this day, a vicious logic has affected expositions of the principles of mechanics. It was a dream of Leibniz that laws of nature could be reduced to identical propositions. Others have believed the principles of mechanics to be axiomatical, so that their falsity is inconceivable. Others have apparently thought it desirable to set them forth in such a way that the assent of the mind should be obtained as readily as possible. But a true logic teaches, first, that, since mechanical principles are real facts, they cannot be reduced to identical propositions which are mere formulas of words, and second that the truth of facts can only be established upon evidence. Axioms are propositions in favor of which we have a natural prejudice. If there be any such propositions, they are not proved by our prejudice. Besides, independently of the truth of them, in order to set them forth clearly, the real facts which they assert should be made to appear. To aim only at obtaining assent in the exposition of them, leads to so stating them that the mind does not clearly see all that they involve; and thus the powerful minds of great geometers have been directed towards so stating the laws of mechanics that the beginner shall be as little aware of what he is assenting to as possible. The following statement has precisely the contrary aim. I do not seek to show that the principles of mechanics are axiomatical but I seek to show precisely what they are.

All thought rolls upon one thing following from another. That which follows is inferred *deductively* from that which it follows. That from which something else follows is inferred inductively or by hypothesis, from the consequent. Thus the relation of antecedent to consequent is the most important of all relations to us, and naturally if we can find among real relations anything analogous, it will also have a high degree of interest for us and will become our guide and key to the study of things. Such a real system of relationship does

exist; it is *time*. The present time is the analogue of what is present to our mind, while the past and future are in the relation of antecedent and consequent, to this extent that the past absolutely determines the future. The other properties of time, viz., its continuity and its infinity *a parte ante* and *a parte post* do not certainly diminish its resemblance to the logical relation. On the whole this resemblance, if after all not of many elements, seems quite sufficient to account for the importance of time.

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## The Principles of Mechanics

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The positions and velocities of bodies are mere special facts, but their accelerations are determined by laws, and the law of accelerations is apparently the sole law of nature. What we have thus far made out of this law is as follows:—

*The acceleration of the motion of any body, at any instant, is composed of partial accelerations, compounded by geometrical addition, and determined by the relative momentary position of the bodies concerned.*

The fact that accelerations are compounded by geometrical addition is called the principle of the *parallelogram of forces*. The sum is called the *resultant*; its parts the *components*; and the principle is thus stated. To find the resultant of two accelerations, draw a parallelogram of which two adjacent sides shall be parallel and proportional to the components, then that diagonal will be parallel and proportional to the resultant, which on reducing the side representing one component to zero, reduces to the side representing the other.

The principle that the acceleration depends only on the *relative* position of the bodies has two parts. The first part which forbids a general acceleration of the acting bodies in one direction is called the *law of action and reaction*. It is expressed thus. Each particle of matter has a numerical value, called its *mass*, and the sums of the

masses into the accelerations produced by any independent element of the configuration of acting bodies vanishes.

The other part of the principle that the acceleration depends only on the *relative* position of the bodies, forbids the setting up of general rotation and is called the law of areas. According to it the sums of the masses of the particles into the rates at which they describe areas about any axis does not vary with the time.

In regard to the sort of configurations on which the accelerations depend, the most simple is configuration in pairs, or attractions and repulsions depending only on the distance. The best understood forces, gravity and electricity, produce accelerations varying inversely as the square of the distance. We know that there are attractions and repulsions proportional to other functions of the distance. The phenomena of friction, viscosity, and other resistance to shearing seem to indicate accelerations depending on the relative positions of three points or on the angle between two lines. Chemical forces are apparently attractive between unlikes and repulsive between likes so that saturation is reached. These forces seem to give rise to forces different in different directions. Until we know much more than we yet do on all these points, we cannot be said to have a clear idea of the law of mechanics.

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## Problem I. Isochronous Oscillations

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A material point is restricted to move upon a line and is always accelerated towards a fixed point on that line proportionally to its distance from that point, measured along the line.

Let  $t$  be the time,  $s$  the distance of the material point from the fixed point,  $G$  its acceleration toward that point when at the unit of distance. Then, the equation expressing the given condition is

$$\frac{d^2s}{dt^2} = -Gs$$

By the rules of differentiation

$$\begin{aligned}\frac{d}{ds} \left( \frac{ds}{dt} \right)^2 &= \frac{dt}{ds} \cdot \frac{d}{dt} \left( \frac{ds}{dt} \right)^2 \\ &= \frac{dt}{ds} \cdot \frac{d^2s}{dt^2} \cdot 2\frac{ds}{dt} = 2\frac{d^2s}{dt^2}\end{aligned}$$

The fundamental law of mechanics is as follows. *All material effects consist in accelerations (which are compounded by geometrical addition) and which are functions of the relative positions of the bodies.* That is to say, every mechanical condition is expressible by a system of equations of the form

$$\frac{d^2s_1}{dt^2} = F(s_1, s_2, s_3 \dots)$$

When, only, one body is movable

$$\frac{d^2s}{dt^2} = Fs$$

By the identity just made out, this is the same as

$$\frac{1}{2} \frac{d}{ds} \left( \frac{ds}{dt} \right)^2 = Fs$$

or

$$\frac{1}{2}v^2 = \int Fs \cdot ds + H$$

or putting

$$fs = \int Fs \cdot ds$$

$$\frac{1}{2}v^2 = fs + H$$

This is called the principle of living forces. It means that in every mechanical problem, a certain quantity  $H$ , dependent on initial conditions, and called the total energy, does not vary with the time; but it consists of two terms which do vary. One of these, called the actual energy or living force, is proportional to the square of the velocity and the other called the potential energy or the negative of the potential is a function of the position. We here consider the principle only with reference to a single point moving on a line. The law in its entirety would be deduced by considering, in a similar way, a system of equations representing a number of bodies each free to move in a number of ways.

The integration corresponding to the deduction of the principle of living forces applied to the particular problem on hand leads to the equation

$$v^2 = V_0^2 - Gs^2$$

Where  $V_0$  is the velocity with which the material point passes through the position of equilibrium. It is seen that  $v^2 = 0$  or the velocity ceases when

$$V_0^2 = Gs^2$$

or

$$s = \frac{V_0}{\sqrt{G}}$$

This position may be denoted by  $s = S$ . It might, on reaching this position, stop there and still satisfy all that the law of living force *directly* expresses, although as the acceleration toward the fixed point would be going on, we know that it must turn back. But on either differentiating or integrating the equation of living forces, it would be found that this was really implied therein.

In order to integrate the equation of living force we may again apply the principle that

$$\int v^2 dt = \int v \frac{ds}{dt} dt = \int v ds$$

## *Nicholas St. John Green*

*P 108: Proceedings of the American Academy of Arts and Sciences, n.s. 4 (1877): 289-91*

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Mr. Nicholas St. John Green, Professor at the Boston Law School, and formerly lecturer at the Harvard Law School, died at Cambridge, on the 8th of September last. Although his name was only beginning to be known to the public, yet, to the eyes of his associates, he occupied, at the time of his death, as important a position in the field of jurisprudence as did the equally lamented Chauncey Wright in that of philosophy; and in the sudden deaths so near together of these intimate friends in the prime of life, the Academy has lost two of its most gifted members.

In the early practice of his profession, Mr. Green acquired a critical knowledge of the criminal law; and he undoubtedly started with a superstitious respect for the technical element which still prevails in that part of the law. In fact, it would seem evident that, as a younger man, he must have held a good many of the prejudices, legal and political, which are natural to a strong nature unchastened by learning and reflection. But his reason was stronger even than his temperament; and as time went on, and he became a student of history, political economy, psychology, and logic, prejudice gave way to philosophy, and his convictions, without losing in strength, were tempered by an appreciation of the other side which powerful men do not always acquire. He handled a question of law not only with the mastery of a logician who easily reduced a case under established principles, but, also, and with equal power, in the light of the history which explains those principles, and the considerations of political science and human nature which justify them. The evidence of his ability was not confined to the lecture-room; for it is not too much to say, that no man at the Suffolk bar produced a greater effect upon the opinions of the Supreme Court, in the cases which he presented,

than he. His arguments, in addition to the qualities of substance which we have mentioned, had a terseness and simple beauty of form which it is impossible to compare with any less-distinguished models than those of Judge Curtis. Mr. Green did not live long enough to construct a systematic work; but as, with him, theory was not an excuse for ignorance of details, but was based as much on exact and practical knowledge as it was on broad and careful study outside the law, those who knew him best hoped and expected that, when he was satisfied with his patient preparation, he would produce results worthy of his talents. A few notes to his two volumes of criminal cases, two or three articles in the *American Law Review*, and three model volumes of reports, are all that the profession can judge him by; and they are, perhaps, enough. But those who have had the benefit of his conversation and criticism know that, although he had already justified the opinion of his friends, he gave promise of still greater achievements with which he might have enriched the world and honored his profession had he lived.

"He was such a philosopher as needs a Diogenes Laertius to portray him," writes, in a private letter, one who was familiar with his modes of thought. "The basis of his philosophy was, that every form of words that means any thing indicates some sensible fact on the existence of which its truth depends. You can hardly call this a doctrine: it is rather an intellectual tendency. But it was Green's mission to insist upon it and to illustrate it. This was his guide, I feel sure, in the study of law. Witness his essay on the doctrine of responsibility. And he desired to apply the same principle to other branches of philosophy,—to Logic, to Psychology, &c. But these subjects he did not choose to follow out for himself into detail. He cared for them chiefly as fields to assert his ruling principle in: beyond that, he was more or less out of his province. He rather undervalued systems; prizing more highly brochures which put some single principle in a strong light. Bentham's refreshing manner of searching for realities, and contemptuously tossing aside formal doctrines of the law in rummaging down to the very pleasures and pains which result from different legal arrangements, greatly pleased him. But he did not much care for Bentham's systematic works: it was rather his horde of pamphlets, raiding like Cossacks into the legal realm, which delighted him. So, of political economists, he most admired Jean Baptiste Say, perhaps because he was a great pamphleteer.

"Green carried the same keen scent for sensible facts and con-

tempt for every thing else into his affections and his tastes. He was a most warm-hearted man, with an abounding sympathy for all sorts of people, a great fondness for children, and a love for animals. He had also a fine taste for poetry, of which he had read a great deal. But one did not at first so much note his delicate appreciation of what was real, as his scorn for all that was unreal. He had a quality, which was certainly not roughness, but which, for want of a better appellation, might be called a Socratic coarseness. It was well fitted to be the sturdy support of his realism, and gave one a positive pleasure when one knew him, as if it had been an artistic study. He had an overflowing spirit of good-fellowship, and a Rabelaisian humor, without the Rabelaisian cynicism. I see him now, as he draws back from a game of whist, his genial nature shining through the merry twinkle of his eye. But, as he speaks, one perceives that it is not pure mirth that moves him, but sympathetic amusement; for his talk is generally of some fine observation of human or animal nature. . . . He was wont to take up prostrate or hopeless causes with a zeal, unwise and Quixotic from a worldly point of view, but which exemplified some of his highest traits."

## *Note on the Sensation of Color*

*P 100: American Journal of Science and Arts,  
3d ser. 13 (April 1877): 247-51*

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It may, perhaps, be worth while to notice a few consequences of three theories concerning color which are usually regarded with some favor.

*First hypothesis.*—The appearance of every mixture of lights depends solely on the appearances of the constituents, without distinction of their physical constitution. This I believe is established.

*Second hypothesis.*—Every sensation of light is compounded of not more than three independent sensations, which do not influence one another. This is Young's theory. It follows that, if we denote the units of the three elementary sensations by  $i$ ,  $j$ , and  $k$ , every sensation of light may be represented by an expression of the form,

$$Xi + Yj + Zk.$$

*Third hypothesis.*—The intensity of a sensation is proportional to the logarithm of the strength of the excitation, the barely perceptible excitation being taken of unit strength. Negative logarithms are to be taken as *zero*. This is Fechner's law. It is known to be approximately and only approximately true, for the sensation of light. From this it follows that, if  $x$ ,  $y$ ,  $z$  be the relative proportions of a mixture of three lights giving the elementary sensations  $i$ ,  $j$ ,  $k$ , the sensation produced by the mixture is

$$I \log x \cdot i + J \log y \cdot j + K \log z \cdot k,$$

where  $I$ ,  $J$ ,  $K$ , are three constants.

From these principles, it follows that if a light giving any sensation such as that just written have its intensity increased in any ratio  $r$ , the resulting sensation will be,

$$\begin{aligned} & I \log rx \cdot i + J \log ry \cdot j + K \log rz \cdot k \\ & = I \log x \cdot i + J \log y \cdot j + K \log z \cdot k + \log r (Ii + Jj + Kk). \end{aligned}$$

Thus, the result of increasing the brilliancy of any light must be to add to the sensation a variable amount of a constant sensation,  $Ii + Jj + Kk$ . And all very bright light will tend toward the same color, which may therefore be called the *color of brightness*. Moreover, if the three primary colors be mixed in the proportions which each by itself is just perceptible, the sensation produced will be

$$\log r (Ii + Jj + Kk),$$

and can only differ by more or less.

Now I find, in fact, that all colors are yellower when brighter. If two contiguous rectangular spaces be illuminated with the same homogeneous light, uniformly over each, but unequally in the two, they will appear of different colors.

If both are <i>red</i>	the brighter will appear <i>scarlet</i> ;
" " <i>green</i> "	" <i>yellowish</i> ;
" " <i>blue</i> "	" <i>greenish</i> ;
" " <i>violet</i> "	" <i>blue</i> .

If we have the means of varying the wave-length of the light which illuminates the fainter rectangle, we can improve the match between the two, by bringing the fainter toward the yellow. Such motions will converge toward a certain point of the spectrum which they will never cross,—a point a little more refrangible than D and having a wave length of  $582.10^{-6}$  mm., according to Ångström's map. If both rectangles be illuminated with this light, the fainter appears white or even violet, but if it be varied in wave-length with a view of improving the match, it will be found to return to the same point with the utmost precision.

It appears, therefore, that, if our hypotheses are correct, the color  $\log r (Ii + Jj + Kk)$  is like that of the spectrum at  $\lambda = 582$ , only that it contains less blue or violet and is consequently of greater chromatic intensity.

It further follows from Fechner's law that, if any light be gradually reduced in brightness, one element of the sensation will disappear after another; and that when very faint it will exhibit only one primary color, which is the one which it contains in greatest proportion relatively to the proportion in the light which has the color of brightness. Now, although this does not seem to be exactly the case,

yet we do get some approximation to it. It is true that any light whatever, when sufficiently faint, appears white, owing to the self-luminosity of the retina. We cannot, therefore, unfortunately, get sight of the primary colors by reducing the light of three parts of the spectrum. But we may, as has often been suggested, make use of the principles of contrast. If any red spectral light be sufficiently reduced, it will perfectly match any less refrangible light. We may, therefore, say that a faint spectral red in contrast with a bright light of the same kind, excites with approximate purity one of the elementary sensations. The same thing is true of the violet; and therefore a rich violet may be taken as another primary color. In my book entitled *Photometric Researches*, the printing of which is nearly complete, I show reason to think that the pure green has a wave length intermediate between E and b. A faint green of this sort contrasted with a bright one appears as a very bluish green, and this may therefore be supposed to be the third primary color.

We have seen that it results from the theory that an increase in the brilliancy of any light adds to the sensation nothing of the peculiar color of that light, but only a certain amount of the color of brightness. If this be the fact, then the photometric sensibility of the eye should be the same for all colors. In order to ascertain whether this is so or not, I have made a series of determinations of my photometric probable error. Each determination was based on twenty-eight comparisons of two parts of the same colored disk. Since there were two unknown quantities, namely, the relative brightness of the two surfaces compared, and an instrumental constant, it follows that only twenty-six observations were effective for determining the probable error. Let R be my photometric probable error of a single comparison. Then the probable error of a single determination of R (which we may denote by  $r$ ) would be  $\frac{.51}{\sqrt{26}} \times R$ , or say  $\frac{1}{10}R$ . Having made a considerable number of such determinations of R, with different colored disks, let us ascertain their probable error from their discrepancies, considering them as so many independent observations of the same unknown quantity, and denote this probable error by  $r'$ . If, then, R really is the same for all colors, we should have

$$r' = r,$$

or, at least, the difference should not exceed  $\rho$ , the probable error of  $r'$ ; which may be calculated by the formula

$$\rho = \frac{.51}{\sqrt{mr'}} ,$$

where  $m$  is the number of sets of experiments diminished by 1. If, on the other hand, R varies with the different colors, and not merely accidentally,  $r'$  should have a larger value. The following are the values I obtained for R, the sum of the brightness of the two surfaces compared being taken as unity.

	R.	Diff. from mean
Feb. 6.	White . . . . .	.0041
	Red, just before C . . . . .	.0046
	Chrome-yellow, A 2 . . . . .	.0032
Feb. 7.	Red, just before C . . . . .	.0040
	Staat's emerald green . . . . .	.0046
	Carmine, B . . . . .	.0044
Feb. 13.	Chrome-yellow, A 1 . . . . .	.0037
	Purple, Hoffmann's violet RRR . . . . .	.0033
	Red, just before C . . . . .	.0048
	Green, complementary to carmine . . . . .	.0034
	Blue violet, No. 2 . . . . .	.0048
	Yellow, A 1, mixed with black . . . . .	.0032
Mean . . . . .		<hr/>
		.0040

After these experiments, the method of observing was changed, and I obtained the following:

Feb. 14.	White window-shade, ill. by sun . . . . .	.0030	-.0002
	Brown . . . . .	.0030	-.0002
	Greenish sky-blue . . . . .	.0037	+.0005
	Very reddish purple . . . . .	.0028	-.0004
	Yellow orange . . . . .	.0032	±.0000
Feb. 15.	"Fundamental green of Müller" . . . . .	.0030	-.0002
	Vermilion, half between C and D . . . . .	.0034	+.0002
	Violet . . . . .	.0032	±.0000
	Yellow . . . . .	.0036	+.0002
Mean . . . . .		<hr/>	
		.0032	

We thus get from the

first twelve determinations,  $r = .00040$ ,  $r' = .00048$ ,  $\frac{r'}{r} = 1.2$

last nine determinations,  $r = .00032$ ,  $r' = .00019$ ,  $\frac{r'}{r} = 0.6$

and from the weighted mean,  $\frac{r'}{r} = .96$ , so that it appears from these experiments that the photometric susceptibility of the eye is the same for all colors. The result is, however, uncertain, because it may be that  $R$  is chiefly due to other sources of error than the limitation of sensibility; still, the experiments show as small a value of  $R$  as is usually obtained. I shall endeavor, by further observations, to obtain a conclusive result.

A further consequence of our hypotheses will be reached by differentiating the expression for a light-sensation. We have

$$\begin{aligned} & d(I \log x \cdot i + J \log y \cdot j + K \log z \cdot k) \\ &= \frac{1}{x} dx \cdot Ii + \frac{1}{y} dy \cdot Jj + \frac{1}{z} dz \cdot Kk. \end{aligned}$$

Now, as  $x$ ,  $y$ , and  $z$  all exceed unity, the differential is greater the nearer unity  $x$ ,  $y$ , and  $z$  are. Hence, since the variation of the proportions of the primary colors with a variation of position in the normal spectrum is uniform,<sup>1</sup> it follows that the change of color of the normal spectrum should be most rapid about  $\lambda = 582$ , as it of course is. It is also obvious that if the total quantities of the three colors are nearly the same in different parts of the spectrum (I here refer to these colors not as really objective, but as measured in the usual objective way) then the part about  $\lambda = 582$  must be the brightest, another familiar fact.

I may observe that there is a modification of our formula for a sensation of light, which probably better represents the relations of the sensations. Writing, in the first place,

$$i = Ii \quad j = Jj \quad k = Kk$$

1. I will show this in a note in the next number of this Journal.

the formula is

$$\log x \cdot i + \log y \cdot j + \log z \cdot k.$$

This loses its validity when any of the logarithms become negative. If  $z$  is the smallest of the three quantities, we may substitute

$$X = \frac{x}{z} \quad Y = \frac{y}{z}$$

and the formula becomes

$$\log X \cdot i + \log Y \cdot j + \log z(i + j + k).$$

When  $x$  or  $y$  is smallest there will be two other formulæ. Now, as the variation in the brilliancy of the light affects only the last term of the last formula, and not the first two depending on  $X$  and  $Y$ , it is more than probable that the eye is habituated to separating the element of sensation which this last term represents, and which is continually changing its values, from the rest which remains constant. It is, therefore, likely that the classification of light into three kinds, according as the *violet*, the *red*, or the *green*, is contained in the smallest proportion, is one which has a relation to the natural powers of discrimination.

My observations have been made with an instrument for which I am indebted to the liberality of the trustees of the Bache Fund. I shall describe it on the occasion of publishing some work of a more serious character. The colored disks made use of were very kindly lent me by Professor Rood.

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*On the Influence of the Flexibility  
of the Support on the Oscillation  
of a Pendulum*

*P 253: Coast Survey Report 1881, 427-36*

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NEW YORK, July 13, 1877.

DEAR SIR: On taking charge of the Coast Survey researches upon gravity, I ordered of Messrs. Repsold a reversible pendulum, to be a copy of that of the Prussian Geodetical Institute. But the instrument makers were at that time so taken up with the construction of instruments for the Transit of Venus, that the pendulum was only ready in the spring of 1875. I then went to Hamburg to receive it; and from Hamburg I went on to Berlin, where I found General Baeyer rather dissatisfied with the results obtained with the Prussian instrument. He specially mentioned the flexibility of the tripod, a source of error which pendulum experimenters have surely never overlooked. The pendulum apparatus that I had carried with me from America having been ruined in transportation, I was under the necessity of employing the new instrument, and therefore undertook to measure and take account of the error in question.

A pendulum support might be rickety, so that the pendulum in its oscillations should throw the knife-edge plane from one position to another, without its undergoing any resistance to the motion other than inertia and friction, between two fixed points. This, however, does not happen in the case of any of the supports that I have examined; for, upon observing their behavior under a high-power microscope, I have always found that they spring back exactly to their original position after every flexure that I have applied to them. In short, the movement with which we have to do is the oscillatory flexure of an elastic body. The amplitude of the oscillation is, at most,

about  $\frac{1}{5000}$  of that of the lower knife-edge of the pendulum, so that its square may be neglected.

The plane of support of the knife is itself undoubtedly bent during the movement; but I neglect this and limit myself to the consideration of the movement of its middle point. When to this middle point is applied a horizontal force perpendicular to the knife-edge, the latter describes a movement of revolution around an axis which, in the case of the Repsold apparatus, is situated behind and above the tripod at a distance of about a meter from the knife-edge. We can neglect the difference between this movement and a translation, until we come to measure its amount. There is also a minute variation in the vertical pressure of the pendulum on the support, but this is very far from producing any sensible effect on the period of oscillation.

Let us denote by

- $m$  the mass of a particle,
- $r$  its distance from the knife-edge,
- $\omega$  the inclination, at rest, to the vertical of the perpendicular let fall from the particle on to the knife-edge,
- $M$  the mass of the pendulum,
- $l$  the length of the corresponding simple pendulum,
- $h$  the distance of the center of mass from the knife-edge,
- $T$  the period of the oscillation,
- $g$  the acceleration of gravity,
- $\epsilon$  the elasticity of the support,
- $\varphi$  the instantaneous inclination of the pendulum to its position of rest,
- $s$  the instantaneous displacement of the middle point of the knife-edge from its position of rest,
- $t$  the time.

Then, the horizontal velocity of a particle will be

$$r \cos(\varphi + \omega) D_t \varphi + D_t s$$

and its vertical velocity will be

$$r \sin(\varphi + \omega) D_t \varphi.$$

Its living force will, therefore, be

$$\frac{1}{2}mr^2(D_t\varphi)^2 + mr \cos(\varphi + \omega)D_t\varphi \cdot D_ts + \frac{1}{2}m(D_ts)^2,$$

and that of the pendulum will be

$$\frac{1}{2}Mlh(D_t\varphi)^2 + Mh \cos \varphi \cdot D_t\varphi \cdot D_ts + \frac{1}{2}M(D_ts)^2.$$

The living force of the motion of the support itself may be left out of account since it involves the square of an excessively small velocity.<sup>1</sup>

The differential of the potential energy is

$$Mgh \sin \varphi \cdot d\varphi + \epsilon s \cdot ds.$$

There is really a third term to be added to this expression dependent on the molecular friction of the matter of the support. But I think we may neglect this term; for its effect cannot be very great, and its coefficient is, at any rate, unknown.<sup>2</sup>

From the expressions for the living force and potential we deduce the Lagrangian equations

$$lD_t^2\varphi + \cos \varphi \cdot D_t^2s = -g \sin \varphi$$

$$-h \sin \varphi \cdot (D_t\varphi)^2 + h \cos \varphi \cdot D_t^2\varphi + D_t^2s = -\frac{\epsilon}{M}s,$$

or, neglecting terms of the second degree,

$$lD_t^2\varphi + D_t^2s = -g\varphi$$

$$hD_t^2\varphi + D_t^2s = -\frac{\epsilon}{M}s.$$

1. It is easy to see that the effect of this would be to increase the last term of the living force; this would affect the second of the differential equations just as if  $M$  had been multiplied and  $h$  divided by the same quantity. But this would not affect the final result. [1882.]

2. This is the point to which the greatest objection to my work has been made. [1882.]

[NOTE.—1882, July 24. I omit the solution of these equations as originally given, and substitute the following, which is perhaps less inelegant. Subtracting the second equation from the first, we get

$$(l - h)D_t^2\varphi + g\varphi = \frac{\epsilon}{M}s$$

or

$$D_t^2s = \frac{M}{\epsilon}(l - h)D_t^4\varphi + \frac{Mg}{\epsilon}D_t^2\varphi$$

Substituting this value in the first differential equation, we have

$$\frac{M}{\epsilon}(l - h)D_t^4\varphi + \left(l + \frac{Mg}{\epsilon}\right)D_t^2\varphi + g\varphi = 0.$$

Separating the operator into factors, and using the abbreviation

$$i = 4 \frac{Mg}{\epsilon l} \cdot \frac{1 - \frac{h}{l}}{\left(1 + \frac{Mg}{\epsilon l}\right)^2}$$

we get

$$\begin{aligned} & \left[ D_t^2 + \frac{\epsilon l + Mg}{2M(l - h)} (1 + \sqrt{1 - i}) \right] \\ & \cdot \left[ D_t^2 + \frac{\epsilon l + Mg}{2M(l - h)} (1 - \sqrt{1 - i}) \right] \varphi = 0. \end{aligned}$$

The solution of this is

$$\begin{aligned} \varphi = & A_1 \cos \left( \sqrt{\frac{\epsilon l + Mg}{2M(l - h)}} (1 - \sqrt{1 - i}) \cdot t + \eta_1 \right) \\ & + A_2 \cos \left( \sqrt{\frac{\epsilon l + Mg}{2M(l - h)}} (1 + \sqrt{1 - i}) \cdot t + \eta_2 \right) \end{aligned}$$

where  $A_1$ ,  $A_2$ ,  $\eta_1$ ,  $\eta_2$  are arbitrary constants. On neglecting the square of  $\frac{Mg}{\epsilon l}$ , this reduces to

$$\varphi = A_1 \cos \left( \sqrt{\frac{g}{l} \left( 1 - \frac{Mg}{\epsilon l} \right)} \cdot t + \eta_1 \right)$$

$$+ A_2 \cos \left( \sqrt{\frac{\epsilon}{M} \cdot \frac{1 + \frac{Mgh}{\epsilon l^2}}{1 - \frac{h}{l}}} \cdot t + \eta_2 \right)$$

The second term represents a mere tremor, for its period is very short, owing to the large value of  $\epsilon$ . The period of the first harmonic constituent is

$$T = \sqrt{\frac{l}{g} + \frac{M}{\epsilon}}$$

From the value of  $\varphi$  and the first equation of this note, we deduce the following value of  $s$ :

$$s = \frac{Mg}{2\epsilon} \left( -\frac{\epsilon l}{Mg} (1 - \sqrt{1-i}) + 1 + \sqrt{1-i} \right)$$

$$\cdot A_1 \cos \left( \sqrt{\frac{\epsilon l + Mg}{2M(l-h)}} (1 - \sqrt{1-i}) \cdot t + \eta_1 \right)$$

$$+ \frac{Mg}{2\epsilon} \left( -\frac{\epsilon l}{Mg} (1 + \sqrt{1-i}) + 1 - \sqrt{1-i} \right)$$

$$A_2 \cos \left( \sqrt{\frac{\epsilon l + Mg}{2M(l-h)}} (1 + \sqrt{1-i}) \cdot t + \eta_2 \right)$$

It thus appears that the amplitude of the principal constituent of  $s$  is nearly

$$h \frac{Mg}{\epsilon l} A_1,$$

while that of the other constituent is nearly  $-l A_2$ .

To find the best way of starting the pendulum so as to make the

ratio of  $A_2$  to  $A_1$  as small as possible, we must consider how to make the initial value of  $s$  as nearly as possible  $h \frac{Mg}{\epsilon l}$  times the initial value of  $\varphi$ . Now, it is easy to see that if the pendulum is supported at a point at a distance  $x$  from the knife-edge, any yielding of the support will diminish the value of  $\varphi$  as expressed by the equation

$$ds = -x \sec \varphi \cdot d\varphi.$$

Substituting this in the expression for the differential of the potential energy, this last becomes

$$Mgh \sin \varphi \cdot d\varphi - \epsilon s x \sec \varphi \cdot d\varphi.$$

Equating this to zero, we find

$$s = h \frac{Mg}{\epsilon x} \sin \varphi \cdot \cos \varphi.$$

In order that this should be equal to  $h \frac{Mg}{\epsilon l} \varphi$ , it is only necessary to put  $x = l$ , so that in starting the pendulum the finger or trigger should be applied at the lower knife-edge or center of gyration.]

The elasticity,  $\epsilon$ , may be measured by observing the deflection,  $S$ , of the support produced by a horizontal force equal to the unit of weight. For

$$\epsilon = \frac{g}{S}$$

Substituting this value, we find

$$\varphi = \frac{A}{h} \cos \left( \sqrt{\frac{g}{(l + MS \frac{h}{l})}} \cdot t \right)$$

Accordingly, the effect on the pendulum is to give it a virtual length greater than what it would have on a rigid support by  $MS \frac{h}{l}$ .

Let us denote the duration of an oscillation by  $T$ , and let  $\Delta$  be used to indicate the effects of flexure. Then, since

$$T^2 = \frac{\Theta^2}{g} l$$

we have

$$\Delta T^2 = \frac{\Theta^2}{g} MS \frac{h}{l}.$$

If we distinguish by subjacent letters the two positions of a reversible pendulum, we have

$$\frac{\Theta^2 l}{g} = \frac{T_d^2 h_d - T_u^2 h_u}{h_d - h_u}$$

and

$$\Delta l = MS,$$

or putting  $\lambda$  for the length of the second's pendulum

$$\Delta \lambda = MS \frac{\lambda}{l}.$$

To determine the flexure, I fasten in the slot in the plane of suspension of the Repsold apparatus a fish-line passing horizontally in the direction of the pendulum's movement over an Atwood's machine pulley, and on the end of this cord I hang a kilogramme. [With a stronger support, the pendulum itself may conveniently replace the kilogramme.] On the extremity of the plane of suspension, or at the end of an arm attached thereto,<sup>3</sup> I stick a glass stage micrometer, turned so as to measure in a direction parallel to the impressed force. This scale is looked at by a microscope carrying a filar micrometer, and solidly mounted upon an independent support, the standard of which is a piece of gas pipe about 10 centimeters in diameter.

I now give a brief *résumé* of my results, beginning with the experiments to determine the position of the fixed axis about which the head of the Repsold support rotates during flexure.

3. This arm is best made of brass tubing, which may be cut out to make it lighter. [1882.]

A.—*Experiments made on a level with the suspension plane*

HOBOKEN, March 10, 1877. Temperature 13° C

+ = forward; - = back

Distance of scale from end of plane	Flexure in revolutions of the micrometer screw	
	Observed	Calculated
<i>m.</i>		
-0.496	+0.211	+0.209
+0.053	+0.356	+0.358
+0.318	+0.436	+0.431

The calculated quantities suppose that the axis pierces the suspension plane at a distance of 1<sup>m</sup>.355 behind the forward end of the suspension plane.

B.—*Experiments in the vertical of the forward end*HOBOKEN, March 12, 1877. Temperature 14° C. Observer,  
Sub-assistant SMITH

+ = below; - = above

Position of the scale relative to the suspension plane	Flexure in revolutions of the micrometer screw	
	Observed	Calculated
<i>m.</i>		
-0.44	+0.196	+0.196
0.000	+0.340	+0.332
+0.395	+0.446	+0.454

The calculated quantities suppose the axis to pierce the vertical of the forward end of the suspension plane 1<sup>m</sup>.07 above this plane. It is not at all surprising that the instantaneous axis is above the suspension plane. Let us suppose that the flexure existed exclusively in three feet of the support. In this case the movement of the upper end of each foot would be perpendicular to the general direction of the foot, and at the same time perpendicular to the radius of the

circle of revolution, so that the foot would be directed directly towards the fixed axis. The axis is without doubt behind the support, on account of the flexure of the plane itself.

I made experiments at Geneva, Paris, Berlin, and New York, in order to determine S numerically. The experiment at Geneva, made the 13th of September, was only a trial. But I had a good pulley which I had borrowed from the workshop of the Geneva society for the construction of physical instruments, and I got as an approximate value—

$$S = 0^{\text{mm}}.034$$

The pulley that I used at Paris had considerable friction, to which can be attributed the fact that the numbers found differ sensibly from those obtained with the aid of better apparatus.

These are the figures—

January 18, 1876, at Messrs. Brünner, Temp.  $1^{\circ}$  C S =  $0^{\text{mm}}.0363$   
 March 7, 1876, at the Paris Observatory, Temp.  $9^{\circ}$  C S =  $0^{\text{mm}}.0371$

At Berlin I used a very delicate pulley which turned on friction-wheels, in order to diminish the friction. It belonged to the Physical Cabinet of the Institute of Technology of Berlin, and was put at my disposition by the kindness of Professor Paalzow. The micrometric readings were made alternately with and without the weight, making but one reading each time, in order to avoid any error arising from the support of the micrometer, this being made of wood. In the readings made alternately with and without the weight, I ended with the arrangement with which I began (11 for one, and 10 for the other), in order that the mean instant of the observations should be the same for the two arrangements. The value of 1 division of the micrometer screw was measured separately.

Below are the results of the different series—

May 24, 1876, a. m.,	S = $0^{\text{mm}}.0340$
Temp. $13^{\circ}$ C., p. m.,	$0^{\text{mm}}.0339$
	$0^{\text{mm}}.0340$
	$0^{\text{mm}}.0341$
May 25, 1876, Temp. $13^{\circ}$ ,	$0^{\text{mm}}.0337$
	$0^{\text{mm}}.0336$

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Mean,	S = $0^{\text{mm}}.0339 \pm 0^{\text{mm}}.0001$
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At Hoboken (near New York) I obtained, through the kindness of Professor Morton, an excellent pulley, made in the workshop of the Stevens Institute of Technology. I always made a reading on each one of the lines of the scale before changing the disposition of the weight.

The results of the separate series are—

March 7, 1877, Temp. 15° C.,	$S = 0^{\text{mm}}.0342$
March 10, 1877, Temp. 12°.	$0^{\text{mm}}.0332$
	$0^{\text{mm}}.0337$
	$0^{\text{mm}}.0343$
	$0^{\text{mm}}.0342$
	$0^{\text{mm}}.0339$
	$0^{\text{mm}}.0334$
These two series should have double weight in the reduction,	$\left\{ \begin{array}{l} 0^{\text{mm}}.0342 \\ 0^{\text{mm}}.0342 \end{array} \right.$
Mean,	$S = \overline{0^{\text{mm}}.0340} \pm 0^{\text{mm}}.0001$

In all the experiments made in the different positions of the scale the flexure obtained has been reduced to the center of the knife, and this last is what is called  $S$ .

It is to this last value that I give the preference.

It follows, from the experiments described on pages 223-25, made to determine the position of the axis of rotation, that the forward end of the suspension plane is distant from that axis by  $\sqrt{1^{\text{m}}.355 \times 1^{\text{m}}.07} = 1^{\text{m}}.20$ . And, since the movement of this end with the weight of a kilogramme is  $S + 0^{\text{mm}}.0008$ , the correction  $+0.0008$  arising from the reduction from the center of knife to the forward end, it follows that the torsion of the support by that force is  $\frac{0^{\text{mm}}.0348}{1^{\text{m}}.20} = 0.0000290 = 5.^{\circ}98$ . Although there is nothing to be suspected in this result, I wished to check it by a direct experiment. I attached a mirror at the extremity of the suspension plane, and, with the aid of a telescope, I measured the torsion by the reflection of a scale, and I found it  $6^{\circ}$ . This method, of course, is not as exact as the other.

In order to arrive at another confirmation of the theory, I made the following observations on the flexure produced by the oscillation

of the pendulum itself in its two positions, using a tolerably high-power microscope (*i.e.*, magnifying 500 diameters). The scale used was made by Mr. Rogers, of Harvard College Observatory. It is divided with extreme exactness, the interval between two lines being  $\frac{1}{4000}$  of an English inch. It was fixed 70 millimeters before the center of the knife, which gives a correction to S of +.0019.

If  $\Phi$  is the amplitude of oscillation on each side of the vertical, the double amplitude of the vibration of the scale should be

$$2M(S + 0^{\text{mm}}.0019) \frac{h}{l} \Phi$$

in which  $M = 6.308$  and  $\frac{h}{l} = \frac{17}{56}$  or  $\frac{39}{56}$  according as the pendulum is suspended by the knife nearest or farthest from the center of gravity. I used this formula in calculating the quantities now given.

## DYNAMICAL FLEXURE

A.—*Pendulum suspended by the knife farthest from the center of gravity*

HOBOKEN, March 20, 1877

$\Phi$	Amplitude of the movement of the scale 1 div. = $\frac{1}{4000}$ inch	
	Observed	Calculated
° ,	<i>Divisions</i>	<i>Divisions</i>
2 32	2.2	2.2
2 30	2.1	2.1
2 24	2.0	2.1
2 22	1.9	2.0
2 20	1.9	2.0
2 19	1.95	2.0
1 43	1.5	1.5
0 47	0.8	0.7

B.—*Pendulum suspended by the knife nearest the center of gravity*

$\Phi$	Amplitude of the movement of the scale 1 div. = $\frac{1}{4000}$ in.	
	Observed	Calculated
° ′	<i>Divisions</i>	<i>Divisions</i>
2 39	1.0	1.0
2 34	0.9	1.0
2 29	0.9	0.9
2 25	0.9	0.9
2 22	0.8	0.9
2 14	0.8	0.8
2 12	0.8	0.8
2 06	0.7	0.8
2 04	0.75	0.8
1 57	0.75	0.7
1 51	0.75	0.7

In making these observations, I saw distinctly the little subsidiary vibration at the end of each oscillation arising from the second term of the formula.

Finally, I swung the pendulum on two supports of different flexibility—one was the metallic tripod, by Repsold, to which refer the flexure measurements given above; the other was made by fixing the upper part of the Repsold tripod to a thick wooden plank by means of bronze bolts passing through the three holes through which the feet pass. These holes are conical, and the bolts fit exactly. I put on each bolt between the head of the support and the plank a leaden washer, so that, in tightening the bolts and compressing the washers, great stability was obtained and at the same time a horizontal position. The plank (which was 5 centimeters thick) was cut in order to make a place for the pendulum, and it was placed by force between a stone wall and a brick pillar. A slit was then cut, in which a pulley of an Atwood machine was placed to measure the flexure.

## Experiments on the flexure of this support

HOBOKEN, May 21, 1877

Distance of scale before +, behind – of the center of knife in English inches	Distance of scale to suspension plane in English inches + above, – below	Flexure in millimeters under a weight 1 kilogramme	Temperature C.	Observer
Inches	Inches	mm.	°	
+ 1.2	– 1.3	+0.0052	18.3	E.S.
+ 1.2	– 1.3	+ .0052	18.9	E.S.
+ 1.2	+39.5	– .0425	20.0	C.S.P.
+13.2	+39.5	– .0367		C.S.P.

It follows that for this apparatus  $S' = 0^{\text{mm}}.0031$ , and that the difference between the values of  $S$  for the two supports is  $0^{\text{mm}}.0309$ .

Now I find  $\frac{\pi^2 l}{g} = 1.0125$  sidereal seconds and  $l = 1^{\text{m}}$ . Hence, we conclude that the difference of  $\frac{\pi^2 l}{g}$  according as the pendulum oscillates on one or the other supports must be equal to

$$\frac{\pi^2 l}{g} \frac{M(S - S')}{l} = \frac{81}{80} \times 6.308 \times 0.0309 = 0.000197$$

I swung the pendulum three times on the less solid support and once on the most solid to verify the theory. I observed 10 consecutive passages of the pendulum across the vertical at intervals of 5 minutes, using a relay that I invented for this purpose.

A.—*Oscillations on the Repsold metallic support*

HOBOKEN, April 1, 1877

PENDULUM SUSPENDED BY THE KNIFE NEAREST THE CENTER OF GRAVITY

Number of oscillations	Interval by chronometer	Reduction to infinitely small arc	Corrected interval	Period
	s.	s.	s.	s.
300	301.9652	-0.0130	301.9522	1.006507
296	297.9408	-0.0084	297.9324	528
298	299.9533	-0.0060	299.9473	535
<sup>4</sup> Mean . . . . .				1.0065238

PENDULUM SUSPENDED BY THE KNIFE FARTHEST FROM THE CENTER OF GRAVITY

	s.	s.	s.	s.
296	297.9094	-0.0092	297.9002	1.006420
302	303.9376	-0.0081	303.9295	389
296	297.9060	-0.0066	297.8994	417
Mean . . . . .				1.0064067

Hence, we have

$$\begin{aligned} T_1^2 &= 1^s.0128544 \\ T_2^2 &= 1^s.0130902 \end{aligned}$$

And since  $h_1 : h_2 = 101 : 44$  we have

$$\begin{aligned} \frac{\pi^2 l}{g} &= \frac{T_1^2 h_1 - T_2^2 h_2}{h_1 - h_2} = 1.013 \\ \cdot \left(1 - \frac{101 \times 0.0001456 + 44 \times 0.0000902}{57}\right) &= 1.012672 \end{aligned}$$

This value is to be corrected for rate of chronometer and temperature. The chronometer lost  $0^s.86$  per day, which gives a correction to  $T^2$  of  $+0^s.000020$ . The temperature during the time the heaviest mass was above was  $12^\circ.7$  in the mean, and  $12^\circ.9$  when this mass was below. Hence, to reduce to  $13^\circ$  we must apply a correction of

4. When we have a series of equal consecutive intervals if  $n$  is the number of intervals and  $i$  is the position of one of them we should, in taking the mean, give to this interval the weight  $in - i(i - 1)$ .

$$\frac{0.1 \times 101 - 0.3 \times 44}{57} \times 0.0000186 = -0.000001$$

Hence we conclude

$$\frac{\pi^2 l}{g} \text{ at } 13^\circ \text{ C.} = 1.012691$$

April 7, 1877

PENDULUM SUSPENDED BY THE KNIFE FARTHEST FROM THE CENTER OF GRAVITY

Number of oscillations	Interval by chronometer	Reduction to infinitely small arc	Corrected interval	Period
290	291.8794	-0.0103	291.8691	1.006445
296	297.9131	-0.0086	297.9045	434
298	299.9241	-0.0073	299.9168	432
298	299.9241	-0.0060	299.9181	437
358	360.3090	-0.0058	360.3032	434
Mean . . . . .	. . . . .	. . . . .	. . . . .	1.0064357

PENDULUM SUSPENDED BY THE KNIFE NEAREST THE CENTER OF GRAVITY

	s.	s.	s.	s.
288	289.9026	-0.0132	289.8894	1.006560
298	299.9648	-0.0092	299.9556	562
300	301.9760	-0.0067	301.9693	564
298	299.9564	-0.0051	299.9513	548
298	299.9561	-0.0037	299.9524	552
Mean . . . . .	. . . . .	. . . . .	. . . . .	1.0065578

Hence we have

$$T_1^2 = 1^s.0129128$$

$$T_2^2 = 1^s.0131586$$

$$\frac{\pi^2 l}{g} = 1^s.012723$$

$$\text{Daily correction, } +0^s.44 \quad + .000010$$

$$\text{Temp. } 15^\circ.8 \text{ (both positions)} \quad - .000052$$

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$$\frac{\pi^2 l}{g} \text{ at } 13^\circ \text{ C.} = 1.012681$$

April 8, 1877

## PENDULUM SUSPENDED BY THE KNIFE NEAREST THE CENTER OF GRAVITY

Number of oscillations	Interval by chronometer	Reduction to infinitely small arc	Corrected time	Period
298	s. 299.9647	s. -0.0175	s. 299.9472	s. 1.006534
298	299.9549	-0.0111	299.9438	523
298	299.9539	-0.0080	299.9459	530
298	299.9484	-0.0055	299.9429	520
298	299.9481	-0.0039	299.9442	526
Mean . . . . .	. . . . .	. . . . .	. . . . .	1.0065261

## PENDULUM SUSPENDED BY KNIFE FARTHEST FROM CENTER OF GRAVITY

	s.	s.	s.	s.
298	299.9229	-0.0066	299.9163	1.006431
298	299.9213	-0.0058	299.9155	426
297	298.9125	-0.0049	298.9076	423
299	300.9236	-0.0042	300.9194	419
298	299.9174	-0.0035	299.9136	422
Mean . . . . .	. . . . .	. . . . .	. . . . .	1.0064246

$$\begin{aligned} s. \\ T_1^2 &= 1.0128905 \\ T_2^2 &= 1.0130948 \\ \frac{\pi^2 l}{g} &= 1.012733 \end{aligned}$$

Daily correction,  $-0^s.41$        $- .000009$ Temp. heavy end up,  $13^\circ.2$  }       $- .000016$ Temp. heavy end down,  $13^\circ.5$  }       $- .000016$ 

$$\frac{\pi^2 l}{g} \text{ at } 13^\circ = 1.012708$$

Hence the three experiments on the Repsold support give for the value of  $\frac{\pi^2 l}{g}$  at  $13^\circ$  C.

*s.*  
April 1, 1.012691  
April 7, 1.012681  
April 8, 1.012708

Mean, 1.012693

B.—*Experiments made on the stiffest support*

HOBOKEN, May 14, 1877

## PENDULUM SUSPENDED BY THE KNIFE FARTHEST FROM THE CENTER OF GRAVITY

Mean instant of 10 transits			Interval of 298 oscillations	Reduction to infinitely small arc	Corrected interval	Interval of 298 oscillations	Reduction to infinitely small arc	Corrected interval
<i>h.</i>	<i>m.</i>	<i>s.</i>	<i>s.</i>	<i>s.</i>	<i>s.</i>	<i>s.</i>	<i>s.</i>	<i>s.</i>
14	6	22.4307	.....	.....	.....	.....	.....	.....
..	7	22.8245	.....	.....	.....	.....	.....	.....
..	11	22.3337	299.9030	-0.0132	299.8898	.....	.....	.....
..	12	22.7213	.....	.....	.....	299.8968	-0.0126	299.8842
..	16	22.2313	299.8976	-0.0110	299.8866	.....	.....	.....
..	17	22.6209	.....	.....	.....	299.8996	-0.0106	299.8890
..	22	22.5145	.....	.....	.....	299.8936	-0.0087	299.8849
..	23	22.9017	....	.....	.....	.....	.....	.....
..	27	22.4055	.....	.....	.....	299.8910	-0.0074	299.8836
..	28	22.7949	299.8932	-0.0072	299.8860	.....	.....	.....
..	33	22.6896	299.8947	-0.0060	299.8887	.....	.....	.....

Mean  $T_1 = 1^{\circ}.0063371$ .

## PENDULUM SUSPENDED BY THE KNIFE NEAREST THE CENTER OF GRAVITY

Mean instant of 10 transits			Intervals of 298 oscillations	Reduction to infinitely small arc	Corrected intervals
<i>h.</i>	<i>m.</i>	<i>s.</i>	<i>s.</i>	<i>s.</i>	<i>s.</i>
15	53	22.4041	.....	.....	.....
..	58	22.3579	299.9538	-0.0198	299.9340
..	3	22.3058	299.9479	-0.0131	299.9348
..	8	22.2531	299.9473	-0.0087	299.9386
..	13	22.2119	299.9588	-0.0062	299.9526
..	18	22.1554	299.9435	-0.0044	299.9391
..	23	22.1011	299.9457	-0.0031	299.9426

Mean  $T_2 = 1^s.0065104$ .

Hence we find—

<i>s.</i>	
$T_1^2$	= 1.0127144
$T_2^2$	= 1.0130632
$\frac{\pi^2 l}{g}$	= 1.012445
Daily corr. to chron. + 2 <sup>s</sup> .59	- .000060
Temp. heavy end down, 14°.18 }	- .000010
Temp. heavy end up,      15°.00 }	
<hr/>	
$\frac{\pi^2 l}{g}$ at 13°	= 1.012495

Comparing this value with the one obtained with the other support we find a difference of 0.000198. The difference, according to the computations of the experiments on flexure, ought to have been 0.000197,<sup>5</sup> which shows a sufficient agreement.

Yours, most faithfully,

[Signed]

C. S. PEIRCE,  
*Assistant United States Coast Survey.*

5. In the original publication, owing to an erroneous value for the mass of the pendulum, this is erroneously calculated as 0.000191. The agreement of the experiments with theory is, therefore, much better than was supposed.

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## On a New Class of Observations, suggested by the principles of Logic

*MS 311: Summer 1877*

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It is usually admitted that there are two classes of mental representations, Immediate Representations or Sensations and Mediate Representations or Conceptions. The former are completely determinate or individual objects of thought; the latter are partially indeterminate or general objects. Granting that both these classes of objects exist, the question of the principle of Individuation or of the respect in which the individual differs from the general becomes one of extreme difficulty. Duns Scotus after a masterly criticism of all the attempts at answering it, puts forward the theory that the distinction is a peculiar one without any general character, and therefore itself presenting this peculiar aspect of individuality. Occam denies that any general objects of thought exist, which implies that no objects of thought have any resemblances, differences, or relations of any kind. I on the other hand have undertaken to show that just the reverse of this is the case. That no object is individual but that the things the most concrete have still a certain amount of indeterminacy. Take, Phillip of Macedon for example. This object is logically divisible into Phillip drunk and Phillip sober; and so on; and you do not get down to anything completely determinate till you specify an indivisible instant of time, which is an ideal limit not attained in thought or in *re*.

It follows from this doctrine that we have no pure sensations, but only sensational elements of thought. Thus, the difference between *blue* and *red*, since it contains a sensational element, cannot be fully represented by any general description. As the sensational element is in this case very large, the failure of any attempt to describe the difference between *blue* & *red* in general terms is very striking. But, according to my theory of logic, since no pure sensations or individ-

ual objects exist, it follows that there must be some relation between *blue* & *red* & some general respect in which they differ, & therefore a step can be made towards a general description of their difference and if that general description is unsatisfactory, as it must be, then another relation must exist between the two colors, & an addition based on it may be made to the general description, & so on *ad infinitum*.

Here, then, are two metaphysical theories; the ordinary one and mine. According to the former, there are ultimate sensations without any general relations between them; according to the latter, although the differences between different sensations can never be completely covered by a general description, yet we may make an indefinite progress toward such a result. Which is true? I have no need to make any special observations to determine that, any more than I would in a question of a Perpetual Motion. I rest upon general principles which are deduced by irrefragable reasoning, from facts so general as to be admitted by all the world. But, for those who cannot understand this reasoning, I point to some facts which are not far to seek.

Different sensations resemble one another. That is sufficiently patent, & there is already a relation between sensations which at once totally & irretrievably overthrows the ordinary theory. Different sensations also differ in intensity. There is another class of relations between them, irreconcileable with their individual character. But it is commonly said that of relations of *kind* between different elementary sensations there are none. What none? When our ordinary language classifies them according to their kinds into colors, sounds, tastes, smells, feelings? This is not commonly noticed, but it is commonly said (I mean by physicists) that there is no *meaning* in the comparison of the intensity of a red and green light. Here I have 74 pieces of different coloured ribbons each one numbered upon which I have made frequent photometric experiments extending over a period of 12 months. Now I say that a red and green can be compared in intensity with a considerable degree of accuracy. On another occasion, when the figures are relevant, I will give them. They are not so now, because you can all see that that red is darker than that blue & that that blue is darker than that red. There is an uncertainty in the judgment, a probable error. But that probable error is only another fact, another numerically determinate relation between the two sensations. So with a light & a sound. They can also

be compared in intensity. Consider with equal attention the sound of a cannon or the appearance of a sixth magnitude star. Which is most intense? Can there be any doubt. Consider the light of the sun, & the sound of a falling pin. In the laws of these relations of intensity between different sensations there is an immense research, a branch of science. These are not the only relations between sensations but they are the most tangible and the natural starting point.

Here then is a whole world of observation, to which we have been systematically blind, simply because of a wrong metaphysical prejudice. One of the most accomplished men of science in the country went so far as to say to me the other day, that there was no reason to suppose that the sensations of color of one person had *any resemblance* to those of another! Violently as this flies in the face of the principles of inductive reasoning, I was not surprized to hear it said, because as Aristotle says *εἴτε φιλοσοφητέον φιλοσοφητέον εἴτε μὴ φιλοσοφητέον φιλοσοφητέον, πάντως δὲ φιλοσοφητέον*. Whether we have an antimetaphysical metaphysics or a pro-metaphysical metaphysics, a metaphysics we are sure to have. And the less pains we take with it the more crudely metaphysical it will be.

## Note on Grassmann's Calculus of Extension

P 125: Presented 10 October 1877

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The last *Mathematische Annalen* contains a paper by H. Grassmann, on the application of his calculus of extension to Mechanics.

He adopts the quaternion addition of vectors. But he has two multiplications, internal and external, just as the principles of logic require.

The *internal* product of two vectors,  $v_1$  and  $v_2$ , is simply what is written in quaternions as —  $S. v_1 v_2$ . He writes it  $[v_1 | v_2]$ . So that

$$[v_1 | v_2] = [v_2 | v_1],$$

$$v^2 = (Tv)^2.$$

The *external* product of two vectors is the parallelogram they form, account being taken of its plane and the direction of running round it, which is equivalent to its *aspect*. We therefore have:—

$$[v_1 v_2] = v_1 v_2 \sin < \frac{v_1}{v_2} \cdot I.$$

$$[v_1 v_2] = - [v_2 v_1], \quad v^2 = o,$$

where I is a new unit. This reminds me strongly of what is written in quaternions as —  $V(v_1 v_2)$ . But it is not the same thing in fact, because  $[v_1 v_2]v_3$  is a solid, and therefore a new kind of quantity. In truth, Grassmann has got hold (though he did not say so) of an eight-fold algebra, which may be written in my system as follows:—

## 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 Planes

$$\begin{aligned}I &= M : X + A : N \\J &= M : Y + B : N \\K &= M : Z + C : N\end{aligned}$$

## One Solid

$$V = M : N$$

## Unity

$$\begin{aligned}1 &= M : M + A : A + B : B + C : C \\&\quad + N : N + X : X + Y : Y + Z : Z\end{aligned}$$

This unity might be omitted.

The relation of the two multiplications is exceedingly interesting. The system seems to me more suitable to three dimensional space, and also more natural than that of quaternions. The simplification of mechanical formulæ is striking, but not more than quaternions would effect, that I see.

By means of eight rotations through two-thirds of a circumference, around four symmetrically placed axes, together with unity, all distortions of a particle would be represented linearly. I have therefore thought of the nine-fold algebra thus resulting.

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**ILLUSTRATIONS OF THE LOGIC OF SCIENCE**

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## *The Fixation of Belief*

P 107: Popular Science Monthly  
12 (November 1877): 1-15

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### I

Few persons care to study logic, because everybody conceives himself to be proficient enough in the art of reasoning already. But I observe that this satisfaction is limited to one's own ratiocination, and does not extend to that of other men.

We come to the full possession of our power of drawing inferences the last of all our faculties, for it is not so much a natural gift as a long and difficult art. The history of its practice would make a grand subject for a book. The mediaeval schoolmen, following the Romans, made logic the earliest of a boy's studies after grammar, as being very easy. So it was, as they understood it. Its fundamental principle, according to them, was, that all knowledge rests on either authority or reason; but that whatever is deduced by reason depends ultimately on a premise derived from authority. Accordingly, as soon as a boy was perfect in the syllogistic procedure, his intellectual kit of tools was held to be complete.

To Roger Bacon, that remarkable mind who in the middle of the thirteenth century was almost a scientific man, the schoolmen's conception of reasoning appeared only an obstacle to truth. He saw that experience alone teaches anything—a proposition which to us seems easy to understand, because a distinct conception of experience has been handed down to us from former generations; which to him also seemed perfectly clear, because its difficulties had not yet unfolded themselves. Of all kinds of experience, the best, he thought, was interior illumination, which teaches many things about Nature which the external senses could never discover, such as the transubstantiation of bread.

Four centuries later, the more celebrated Bacon, in the first book of his *Novum Organum*, gave his clear account of experience as something which must be open to verification and reëxamination. But, superior as Lord Bacon's conception is to earlier notions, a modern reader who is not in awe of his grandiloquence is chiefly struck by the inadequacy of his view of scientific procedure. That we have only to make some crude experiments, to draw up briefs of the results in certain blank forms, to go through these by rule, checking off everything disproved and setting down the alternatives, and that thus in a few years physical science would be finished up—what an idea! "He wrote on science like a Lord Chancellor," indeed.

The early scientists, Copernicus, Tycho Brahe, Kepler, Galileo, and Gilbert, had methods more like those of their modern brethren. Kepler undertook to draw a curve through the places of Mars;<sup>1</sup> and his greatest service to science was in impressing on men's minds that this was the thing to be done if they wished to improve astronomy; that they were not to content themselves with inquiring whether one system of epicycles was better than another, but that they were to sit down to the figures and find out what the curve, in truth, was. He accomplished this by his incomparable energy and courage, blundering along in the most inconceivable way (to us), from one irrational hypothesis to another, until, after trying twenty-two of these, he fell, by the mere exhaustion of his invention, upon the orbit which a mind well furnished with the weapons of modern logic would have tried almost at the outset.

In the same way, every work of science great enough to be remembered for a few generations affords some exemplification of the defective state of the art of reasoning of the time when it was written; and each chief step in science has been a lesson in logic. It was so when Lavoisier and his contemporaries took up the study of chemistry. The old chemist's maxim had been, "*Lege, lege, lege, labora, ora, et relege.*" Lavoisier's method was not to read and pray, not to dream that some long and complicated chemical process would have a certain effect, to put it into practice with dull patience, after its inevitable failure to dream that with some modification it would have another result, and to end by publishing the last dream as a fact: his way was to carry his mind into his laboratory, and to make of his alembics and cucurbit instruments of thought, giving a

1. Not quite so, but as nearly so as can be told in a few words.

new conception of reasoning, as something which was to be done with one's eyes open, by manipulating real things instead of words and fancies.

The Darwinian controversy is, in large part, a question of logic. Mr. Darwin proposed to apply the statistical method to biology. The same thing had been done in a widely different branch of science, the theory of gases. Though unable to say what the movements of any particular molecule of a gas would be on a certain hypothesis regarding the constitution of this class of bodies, Clausius and Maxwell were yet able, by the application of the doctrine of probabilities, to predict that in the long run such and such a proportion of the molecules would, under given circumstances, acquire such and such velocities; that there would take place, every second, such and such a number of collisions, etc.; and from these propositions were able to deduce certain properties of gases, especially in regard to their heat-relations. In like manner, 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. Whether or not existing animal forms are due to such action, or what position the theory ought to take, forms the subject of a discussion in which questions of fact and questions of logic are curiously interlaced.

## II

The object of reasoning is to find out, from the consideration of what we already know, something else which we do not know. Consequently, reasoning is good if it be such as to give a true conclusion from true premises, and not otherwise. Thus, the question of its validity is purely one of fact and not of thinking. A being the premises and B the conclusion, the question is, whether these facts are really so related that if A is B is. If so, the inference is valid; if not, not. It is not in the least the question whether, when the premises are accepted by the mind, we feel an impulse to accept the conclusion also. It is true that we do generally reason correctly by nature. But that is an accident; the true conclusion would remain true if we had no impulse to accept it; and the false one would remain false, though we could not resist the tendency to believe in it.

We are, doubtless, in the main logical animals, but we are not perfectly so. Most of us, for example, are naturally more sanguine and

hopeful than logic would justify. We seem to be so constituted that in the absence of any facts to go upon we are happy and self-satisfied; so that the effect of experience is continually to contract our hopes and aspirations. Yet a lifetime of the application of this corrective does not usually eradicate our sanguine disposition. Where hope is unchecked by any experience, it is likely that our optimism is extravagant. Logicality in regard to practical matters is the most useful quality an animal can possess, and might, therefore, result from the action of natural selection; but outside of these it is probably of more advantage to the animal to have his mind filled with pleasing and encouraging visions, independently of their truth; and thus, upon unpractical subjects, natural selection might occasion a fallacious tendency of thought.

That which determines us, from given premises, to draw one inference rather than another, is some habit of mind, whether it be constitutional or acquired. The habit is good or otherwise, according as it produces true conclusions from true premises or not; and an inference is regarded as valid or not, without reference to the truth or falsity of its conclusion specially, but according as the habit which determines it is such as to produce true conclusions in general or not. The particular habit of mind which governs this or that inference may be formulated in a proposition whose truth depends on the validity of the inferences which the habit determines; and such a formula is called a *guiding principle* of inference. Suppose, for example, that we observe that a rotating disk of copper quickly comes to rest when placed between the poles of a magnet, and we infer that this will happen with every disk of copper. The guiding principle is, that what is true of one piece of copper is true of another. Such a guiding principle with regard to copper would be much safer than with regard to many other substances—brass, for example.

A book might be written to signalize all the most important of these guiding principles of reasoning. It would probably be, we must confess, of no service to a person whose thought is directed wholly to practical subjects, and whose activity moves along thoroughly-beaten paths. The problems which present themselves to such a mind are matters of routine which he has learned once for all to handle in learning his business. But let a man venture into an unfamiliar field, or where his results are not continually checked by experience, and all history shows that the most masculine intellect will oftentimes lose his orientation and waste his efforts in directions

which bring him no nearer to his goal, or even carry him entirely astray. He is like a ship in the open sea, with no one on board who understands the rules of navigation. And in such a case some general study of the guiding principles of reasoning would be sure to be found useful.

The subject could hardly be treated, however, without being first limited; since almost any fact may serve as a guiding principle. But it so happens that there exists a division among facts, such that in one class are all those which are absolutely essential as guiding principles, while in the others are all which have any other interest as objects of research. This division is between those which are necessarily taken for granted in asking whether a certain conclusion follows from certain premises, and those which are not implied in that question. A moment's thought will show that a variety of facts are already assumed when the logical question is first asked. It is implied, for instance, that there are such states of mind as doubt and belief—that a passage from one to the other is possible, the object of thought remaining the same, and that this transition is subject to some rules which all minds are alike bound by. As these are facts which we must already know before we can have any clear conception of reasoning at all, it cannot be supposed to be any longer of much interest to inquire into their truth or falsity. On the other hand, it is easy to believe that those rules of reasoning which are deduced from the very idea of the process are the ones which are the most essential; and, indeed, that so long as it conforms to these it will, at least, not lead to false conclusions from true premises. In point of fact, the importance of what may be deduced from the assumptions involved in the logical question turns out to be greater than might be supposed, and this for reasons which it is difficult to exhibit at the outset. The only one which I shall here mention is, that conceptions which are really products of logical reflection, without being readily seen to be so, mingle with our ordinary thoughts, and are frequently the causes of great confusion. This is the case, for example, with the conception of quality. A quality as such is never an object of observation. We can see that a thing is blue or green, but the quality of being blue and the quality of being green are not things which we see; they are products of logical reflection. The truth is, that common-sense, or thought as it first emerges above the level of the narrowly practical, is deeply imbued with that bad logical quality to which the epithet *metaphysical* is commonly applied; and nothing can clear it up but a severe course of logic.

## III

We generally know when we wish to ask a question and when we wish to pronounce a judgment, for there is a dissimilarity between the sensation of doubting and that of believing.

But this is not all which distinguishes doubt from belief. There is a practical difference. Our beliefs guide our desires and shape our actions. The Assassins, or followers of the Old Man of the Mountain, used to rush into death at his least command, because they believed that obedience to him would insure everlasting felicity. Had they doubted this, they would not have acted as they did. So it is with every belief, according to its degree. The feeling of believing is a more or less sure indication of there being established in our nature some habit which will determine our actions. Doubt never has such an effect.

Nor must we overlook a third point of difference. Doubt is an uneasy and dissatisfied state from which we struggle to free ourselves and pass into the state of belief; while the latter is a calm and satisfactory state which we do not wish to avoid, or to change to a belief in anything else.<sup>2</sup> On the contrary, we cling tenaciously, not merely to believing, but to believing just what we do believe.

Thus, both doubt and belief have positive effects upon us, though very different ones. Belief does not make us act at once, but puts us into such a condition that we shall behave in a certain way, when the occasion arises. Doubt has not the least effect of this sort, but stimulates us to action until it is destroyed. This reminds us of the irritation of a nerve and the reflex action produced thereby; while for the analogue of belief, in the nervous system, we must look to what are called nervous associations—for example, to that habit of the nerves in consequence of which the smell of a peach will make the mouth water.

## IV

The irritation of doubt causes a struggle to attain a state of belief. I shall term this struggle *inquiry*, though it must be admitted that this is sometimes not a very apt designation.

The irritation of doubt is the only immediate motive for the struggle to attain belief. It is certainly best for us that our beliefs should be such as may truly guide our actions so as to satisfy our desires; and this reflection will make us reject any belief which does not seem to

2. I am not speaking of secondary effects occasionally produced by the interference of other impulses.

have been so formed as to insure this result. But it will only do so by creating a doubt in the place of that belief. With the doubt, therefore, the struggle begins, and with the cessation of doubt it ends. Hence, the sole object of inquiry is the settlement of opinion. We may fancy that this is not enough for us, and that we seek, not merely an opinion, but a true opinion. But put this fancy to the test, and it proves groundless; for as soon as a firm belief is reached we are entirely satisfied, whether the belief be true or false. And it is clear that nothing out of the sphere of our knowledge can be our object, for nothing which does not affect the mind can be the motive for a mental effort. The most that can be maintained is, that we seek for a belief that we shall *think* to be true. But we think each one of our beliefs to be true, and, indeed, it is mere tautology to say so.

That the settlement of opinion is the sole end of inquiry is a very important proposition. It sweeps away, at once, various vague and erroneous conceptions of proof. A few of these may be noticed here.

1. Some philosophers have imagined that to start an inquiry it was only necessary to utter a question or set it down upon paper, and have even recommended us to begin our studies with questioning everything! But the mere putting of a proposition into the interrogative form does not stimulate the mind to any struggle after belief. There must be a real and living doubt, and without this all discussion is idle.

2. It is a very common idea that a demonstration must rest on some ultimate and absolutely indubitable propositions. These, according to one school, are first principles of a general nature; according to another, are first sensations. But, in point of fact, an inquiry, to have that completely satisfactory result called demonstration, has only to start with propositions perfectly free from all actual doubt. If the premises are not in fact doubted at all, they cannot be more satisfactory than they are.

3. Some people seem to love to argue a point after all the world is fully convinced of it. But no further advance can be made. When doubt ceases, mental action on the subject comes to an end; and, if it did go on, it would be without a purpose.

## V

If the settlement of opinion is the sole object of inquiry, and if belief is of the nature of a habit, why should we not attain the desired end, by taking any answer to a question which we may fancy, and

constantly reiterating it to ourselves, dwelling on all which may conduce to that belief, and learning to turn with contempt and hatred from anything which might disturb it? This simple and direct method is really pursued by many men. I remember once being entreated not to read a certain newspaper lest it might change my opinion upon free-trade. "Lest I might be entrapped by its fallacies and misstatements," 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 upon the subject. You might, then, if you read this paper, be led to believe in protection. But you admit that free-trade is the true 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 to 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 that 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 irrational, for that only amounts to saying that his method of settling belief is not ours. He does not propose to himself to be rational, and, indeed, will often talk with scorn of man's weak and illusive reason. So let him think as he pleases.

But this method of fixing belief, which may be called the method of tenacity, will be unable to hold its ground in practice. The social impulse is against it. The man who adopts it will find that other men think differently from him, and it will be apt to occur to him, in some saner moment, that their opinions are quite as good as his own, and this will shake his confidence in his belief. This conception, that another man's thought or sentiment may be equivalent to one's own, is a distinctly new step, and a highly important one. It arises from an impulse too strong in man to be suppressed, without danger of destroying the human species. Unless we make ourselves hermits, we shall necessarily influence each other's opinions; so that the problem becomes how to fix belief, not in the individual merely, but in the community.

Let the will of the state act, then, instead of that of the individual. Let an institution be created which shall have for its object to keep correct doctrines before the attention of the people, to reiterate them perpetually, and to teach them to the young; having at the same time power to prevent contrary doctrines from being taught, advocated, or expressed. Let all possible causes of a change of mind be removed from men's apprehensions. Let them be kept ignorant, lest they should learn of some reason to think otherwise than they do. Let their passions be enlisted, so that they may regard private and unusual opinions with hatred and horror. Then, let all men who reject the established belief be terrified into silence. Let the people turn out and tar-and-feather such men, or let inquisitions be made into the manner of thinking of suspected persons, and, when they are found guilty of forbidden beliefs, let them be subjected to some signal punishment. When complete agreement could not otherwise be reached, a general massacre of all who have not thought in a certain way has proved a very effective means of settling opinion in a country. If the power to do this be wanting, let a list of opinions be drawn up, to which no man of the least independence of thought can assent, and let the faithful be required to accept all these propositions, in order to segregate them as radically as possible from the influence of the rest of the world.

This method has, from the earliest times, been one of the chief

means of upholding correct theological and political doctrines, and of preserving their universal or catholic character. In Rome, especially, it has been practised from the days of Numa Pompilius to those of Pius Nonus. This is the most perfect example in history; but wherever there is a priesthood—and no religion has been without one—this method has been more or less made use of. Wherever there is an aristocracy, or a guild, or any association of a class of men whose interests depend or are supposed to depend on certain propositions, there will be inevitably found some traces of this natural product of social feeling. Cruelties always accompany this system; and when it is consistently carried out, they become atrocities of the most horrible kind in the eyes of any rational man. Nor should this occasion surprise, for the officer of a society does not feel justified in surrendering the interests of that society for the sake of mercy, as he might his own private interests. It is natural, therefore, that sympathy and fellowship should thus produce a most ruthless power.

In judging this method of fixing belief, which may be called the method of authority, we must, in the first place, allow its immeasurable mental and moral superiority to the method of tenacity. Its success is proportionately greater; and, in fact, it has over and over again worked the most majestic results. The mere structures of stone which it has caused to be put together—in Siam, for example, in Egypt, and in Europe—have many of them a sublimity hardly more than rivaled by the greatest works of Nature. And, except the geological epochs, there are no periods of time so vast as those which are measured by some of these organized faiths. If we scrutinize the matter closely, we shall find that there has not been one of their creeds which has remained always the same; yet the change is so slow as to be imperceptible during one person's life, so that individual belief remains sensibly fixed. For the mass of mankind, then, there is perhaps no better method than this. If it is their highest impulse to be intellectual slaves, then slaves they ought to remain.

But no institution can undertake to regulate opinions upon every subject. Only the most important ones can be attended to, and on the rest men's minds must be left to the action of natural causes. This imperfection will be no source of weakness so long as men are in such a state of culture that one opinion does not influence another—that is, so long as they cannot put two and two together. But in the most priestridden states some individuals will be found who are raised above that condition. These men possess a wider sort of social feeling;

they see that men in other countries and in other ages have held to very different doctrines from those which they themselves have been brought up to believe; and they cannot help seeing that it is the mere accident of their having been taught as they have, and of their having been surrounded with the manners and associations they have, that has caused them to believe as they do and not far differently. And their candor cannot resist the reflection that there is no reason to rate their own views at a higher value than those of other nations and other centuries; and this gives rise to doubts in their minds.

They will further perceive that such doubts as these must exist in their minds with reference to every belief which seems to be determined by the caprice either of themselves or of those who originated the popular opinions. The willful adherence to a belief, and the arbitrary forcing of it upon others, must, therefore, both be given up, and a new method of settling opinions must be adopted, which shall not only produce an impulse to believe, but shall also decide what proposition it is which is to be believed. Let the action of natural preferences be unimpeded, then, and under their influence let men, conversing together and regarding matters in different lights, gradually develop beliefs in harmony with natural causes. This method resembles that by which conceptions of art have been brought to maturity. The most perfect example of it is to be found in the history of metaphysical philosophy. Systems of this sort have not usually rested upon any observed facts, at least not in any great degree. They have been chiefly adopted because their fundamental propositions seemed "agreeable to reason." This is an apt expression; it does not mean that which agrees with experience, but that which we find ourselves inclined to believe. Plato, for example, finds it agreeable to reason that the distances of the celestial spheres from one another should be proportional to the different lengths of strings which produce harmonious chords. Many philosophers have been led to their main conclusions by considerations like this; but this is the lowest and least developed form which the method takes, for it is clear that another man might find Kepler's theory, that the celestial spheres are proportional to the inscribed and circumscribed spheres of the different regular solids, more agreeable to *his* reason. But the shock of opinions will soon lead men to rest on preferences of a far more universal nature. Take, for example, the doctrine that man only acts selfishly—that is, from the consideration that acting in one way will afford him more pleasure than acting in another. This rests on no fact in the world, but it has had a wide acceptance as being the only reasonable theory.

This method is far more intellectual and respectable from the point of view of reason than either of the others which we have noticed. But its failure has been the most manifest. It makes of inquiry something similar to the development of taste; but taste, unfortunately, is always more or less a matter of fashion, and accordingly metaphysicians have never come to any fixed agreement, but the pendulum has swung backward and forward between a more material and a more spiritual philosophy, from the earliest times to the latest. And so from this, which has been called the *a priori* method, we are driven, in Lord Bacon's phrase, to a true induction. We have examined into this *a priori* method as something which promised to deliver our opinions from their accidental and capricious element. But development, while it is a process which eliminates the effect of some casual circumstances, only magnifies that of others. This method, therefore, does not differ in a very essential way from that of authority. The government may not have lifted its finger to influence my convictions; I may have been left outwardly quite free to choose, we will say, between monogamy and polygamy, and, appealing to my conscience only, I may have concluded that the latter practice is in itself licentious. But when I come to see that the chief obstacle to the spread of Christianity among a people of as high culture as the Hindoos has been a conviction of the immorality of our way of treating women, I cannot help seeing that, though governments do not interfere, sentiments in their development will be very greatly determined by accidental causes. Now, there are some people, among whom I must suppose that my reader is to be found, who, when they see that any belief of theirs is determined by any circumstance extraneous to the facts, will from that moment not merely admit in words that that belief is doubtful, but will experience a real doubt of it, so that it ceases to be a belief.

To satisfy our doubts, therefore, it is necessary that a method should be found by which our beliefs may be caused by nothing human, but by some external permanency—by something upon which our thinking has no effect. Some mystics imagine that they have such a method in a private inspiration from on high. But that is only a form of the method of tenacity, in which the conception of truth as something public is not yet developed. Our external permanency would not be external, in our sense, if it was restricted in its influence to one individual. It must be something which affects, or might affect, every man. And, though these affections are necessarily as various as are individual conditions, yet the method must be such

that the ultimate conclusion of every man shall be the same. Such is the method of science. Its fundamental hypothesis, restated in more familiar language, is this: There are real things, whose characters are entirely independent of our opinions about them; those realities affect our senses according to regular laws, and, though our sensations are as different as our relations to the objects, yet, by taking advantage of the laws of perception, we can ascertain by reasoning how things really are, and any man, if he have sufficient experience and reason enough about it, will be led to the one true conclusion. The new conception here involved is that of reality. It may be asked how I know that there are any realities. If this hypothesis is the sole support of my method of inquiry, my method of inquiry must not be used to support my hypothesis. The reply is this: 1. If investigation cannot be regarded as proving that there are real things, it at least does not lead to a contrary conclusion; but the method and the conception on which it is based remain ever in harmony. No doubts of the method, therefore, necessarily arise from its practice, as is the case with all the others. 2. The feeling which gives rise to any method of fixing belief is a dissatisfaction at two repugnant propositions. But here already is a vague concession that there is some *one* thing to which a proposition should conform. Nobody, therefore, can really doubt that there are realities, or, if he did, doubt would not be a source of dissatisfaction. The hypothesis, therefore, is one which every mind admits. So that the social impulse does not cause me to doubt it. 3. Everybody uses the scientific method about a great many things, and only ceases to use it when he does not know how to apply it. 4. Experience of the method has not led me to doubt it, but, on the contrary, scientific investigation has had the most wonderful triumphs in the way of settling opinion. These afford the explanation of my not doubting the method or the hypothesis which it supposes; and not having any doubt, nor believing that anybody else whom I could influence has, it would be the merest babble for me to say more about it. If there be anybody with a living doubt upon the subject, let him consider it.

To describe the method of scientific investigation is the object of this series of papers. At present I have only room to notice some points of contrast between it and other methods of fixing belief.

This is the only one of the four methods which presents any distinction of a right and a wrong way. If I adopt the method of tenacity and shut myself out from all influences, whatever I think necessary to doing this is necessary according to that method. So with

the method of authority: the state may try to put down heresy by means which, from a scientific point of view, seem very ill-calculated to accomplish its purposes; but the only test *on that method* is what the state thinks, so that it cannot pursue the method wrongly. So with the *a priori* method. The very essence of it is to think as one is inclined to think. All metaphysicians will be sure to do that, however they may be inclined to judge each other to be perversely wrong. The Hegelian system recognizes every natural tendency of thought as logical, although it be certain to be abolished by counter-tendencies. Hegel thinks there is a regular system in the succession of these tendencies, in consequence of which, after drifting one way and the other for a long time, opinion will at last go right. And it is true that metaphysicians get the right ideas at last; Hegel's system of Nature represents tolerably the science of that day; and one may be sure that whatever scientific investigation has put out of doubt will presently receive *a priori* demonstration on the part of the metaphysicians. But with the scientific method the case is different. I may start with known and observed facts to proceed to the unknown; and yet the rules which I follow in doing so may not be such as investigation would approve. The test of whether I am truly following the method is not an immediate appeal to my feelings and purposes, but, on the contrary, itself involves the application of the method. Hence it is that bad reasoning as well as good reasoning is possible; and this fact is the foundation of the practical side of logic.

It is not to be supposed that the first three methods of settling opinion present no advantage whatever over the scientific method. On the contrary, each has some peculiar convenience of its own. The *a priori* method is distinguished for its comfortable conclusions. It is the nature of the process to adopt whatever belief we are inclined to, and there are certain flatteries to the vanity of man which we all believe by nature, until we are awakened from our pleasing dream by some rough facts. The method of authority will always govern the mass of mankind; and those who wield the various forms of organized force in the state will never be convinced that dangerous reasoning ought not to be suppressed in some way. If liberty of speech is to be untrammeled from the grosser forms of constraint, then uniformity of opinion will be secured by a moral terrorism to which the respectability of society will give its thorough approval. Following the method of authority is the path of peace. Certain non-conformities are permitted; certain others (considered unsafe) are forbidden. These are different in different countries and in different ages; but,

wherever you are, let it be known that you seriously hold a tabooed belief, and you may be perfectly sure of being treated with a cruelty less brutal but more refined than hunting you like a wolf. Thus, the greatest intellectual benefactors of mankind have never dared, and dare not now, to utter the whole of their thought; and thus a shade of *prima facie* doubt is cast upon every proposition which is considered essential to the security of society. Singularly enough, the persecution does not all come from without; but a man torments himself and is oftentimes most distressed at finding himself believing propositions which he has been brought up to regard with aversion. The peaceful and sympathetic man will, therefore, find it hard to resist the temptation to submit his opinions to authority. But most of all I admire the method of tenacity for its strength, simplicity, and directness. Men who pursue it are distinguished for their decision of character, which becomes very easy with such a mental rule. They do not waste time in trying to make up their minds what they want, but, fastening like lightning upon whatever alternative comes first, they hold to it to the end, whatever happens, without an instant's irresolution. This is one of the splendid qualities which generally accompany brilliant, unlasting success. It is impossible not to envy the man who can dismiss reason, although we know how it must turn out at last.

Such are the advantages which the other methods of settling opinion have over scientific investigation. A man should consider well of them; and then he should consider that, after all, he wishes his opinions to coincide with the fact, and that there is no reason why the results of these three methods should do so. To bring about this effect is the prerogative of the method of science. Upon such considerations he has to make his choice—a choice which is far more than the adoption of any intellectual opinion, which is one of the ruling decisions of his life, to which, when once made, he is bound to adhere. The force of habit will sometimes cause a man to hold on to old beliefs, after he is in a condition to see that they have no sound basis. But reflection upon the state of the case will overcome these habits, and he ought to allow reflection its full weight. People sometimes shrink from doing this, having an idea that beliefs are wholesome which they cannot help feeling rest on nothing. But let such persons suppose an analogous though different case from their own. Let them ask themselves what they would say to a reformed Mussulman who should hesitate to give up his old notions in regard to the relations of the sexes; or to a reformed Catholic who should still shrink from reading the Bible. Would they not say that these persons ought to

consider the matter fully, and clearly understand the new doctrine, and then ought to embrace it, in its entirety? But, above all, let it be considered that what is more wholesome than any particular belief is integrity of belief, and that to avoid looking into the support of any belief from a fear that it may turn out rotten is quite as immoral as it is disadvantageous. The person who confesses that there is such a thing as truth, which is distinguished from falsehood simply by this, that if acted on it will carry us to the point we aim at and not astray, and then, though convinced of this, dares not know the truth and seeks to avoid it, is in a sorry state of mind indeed.

Yes, the other methods do have their merits: a clear logical conscience does cost something—just as any virtue, just as all that we cherish, costs us dear. But we should not desire it to be otherwise. The genius of a man's logical method should be loved and reverenced as his bride, whom he has chosen from all the world. He need not contemn the others; on the contrary, he may honor them deeply, and in doing so he only honors her the more. But she is the one that he has chosen, and he knows that he was right in making that choice. And having made it, he will work and fight for her, and will not complain that there are blows to take, hoping that there may be as many and as hard to give, and will strive to be the worthy knight and champion of her from the blaze of whose splendors he draws his inspiration and his courage.

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## *How to Make Our Ideas Clear*

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### I

Whoever has looked into a modern treatise on logic of the common sort, will doubtless remember the two distinctions between *clear* and *obscure* conceptions, and between *distinct* and *confused*

conceptions. They have lain in the books now for nigh two centuries, unimproved and unmodified, and are generally reckoned by logicians as among the gems of their doctrine.

A clear idea is defined as one which is so apprehended that it will be recognized wherever it is met with, and so that no other will be mistaken for it. If it fails of this clearness, it is said to be obscure.

This is rather a neat bit of philosophical terminology; yet, since it is clearness that they were defining, I wish the logicians had made their definition a little more plain. Never to fail to recognize an idea, and under no circumstances to mistake another for it, let it come in how recondite a form it may, would indeed imply such prodigious force and clearness of intellect as is seldom met with in this world. On the other hand, merely to have such an acquaintance with the idea as to have become familiar with it, and to have lost all hesitancy in recognizing it in ordinary cases, hardly seems to deserve the name of clearness of apprehension, since after all it only amounts to a subjective feeling of mastery which may be entirely mistaken. I take it, however, that when the logicians speak of "clearness," they mean nothing more than such a familiarity with an idea, since they regard the quality as but a small merit, which needs to be supplemented by another, which they call *distinctness*.

A distinct idea is defined as one which contains nothing which is not clear. This is technical language; by the *contents* of an idea logicians understand whatever is contained in its definition. So that an idea is *distinctly* apprehended, according to them, when we can give a precise definition of it, in abstract terms. Here the professional logicians leave the subject; and I would not have troubled the reader with what they have to say, if it were not such a striking example of how they have been slumbering through ages of intellectual activity, listlessly disregarding the enginery of modern thought, and never dreaming of applying its lessons to the improvement of logic. It is easy to show that the doctrine that familiar use and abstract distinctness make the perfection of apprehension has its only true place in philosophies which have long been extinct; and it is now time to formulate the method of attaining to a more perfect clearness of thought, such as we see and admire in the thinkers of our own time.

When Descartes set about the reconstruction of philosophy, his first step was to (theoretically) permit skepticism and to discard the practice of the schoolmen of looking to authority as the ultimate source of truth. That done, he sought a more natural fountain of true

principles, and professed to find it in the human mind; thus passing, in the directest way, from the method of authority to that of apriority, as described in my first paper. Self-consciousness was to furnish us with our fundamental truths, and to decide what was agreeable to reason. But since, evidently, not all ideas are true, he was led to note, as the first condition of infallibility, that they must be clear. The distinction between an idea *seeming* clear and really being so, never occurred to him. Trusting to introspection, as he did, even for a knowledge of external things, why should he question its testimony in respect to the contents of our own minds? But then, I suppose, seeing men, who seemed to be quite clear and positive, holding opposite opinions upon fundamental principles, he was further led to say that clearness of ideas is not sufficient, but that they need also to be distinct, i.e., to have nothing unclear about them. What he probably meant by this (for he did not explain himself with precision) was, that they must sustain the test of dialectical examination; that they must not only seem clear at the outset, but that discussion must never be able to bring to light points of obscurity connected with them.

Such was the distinction of Descartes, and one sees that it was precisely on the level of his philosophy. It was somewhat developed by Leibnitz. This great and singular genius was as remarkable for what he failed to see as for what he saw. That a piece of mechanism could not do work perpetually without being fed with power in some form, was a thing perfectly apparent to him; yet he did not understand that the machinery of the mind can only transform knowledge, but never originate it, unless it be fed with facts of observation. He thus missed the most essential point of the Cartesian philosophy, which is, that to accept propositions which seem perfectly evident to us is a thing which, whether it be logical or illogical, we cannot help doing. Instead of regarding the matter in this way, he sought to reduce the first principles of science to formulas which cannot be denied without self-contradiction, and was apparently unaware of the great difference between his position and that of Descartes. So he reverted to the old formalities of logic, and, above all, abstract definitions played a great part in his philosophy. It was quite natural, therefore, that on observing that the method of Descartes labored under the difficulty that we may seem to ourselves to have clear apprehensions of ideas which in truth are very hazy, no better remedy occurred to him than to require an abstract definition of every

important term. Accordingly, in adopting the distinction of *clear* and *distinct* notions, he described the latter quality as the clear apprehension of everything contained in the definition; and the books have ever since copied his words. There is no danger that his chimerical scheme will ever again be over-valued. Nothing new can ever be learned by analyzing definitions. Nevertheless, our existing beliefs can be set in order by this process, and order is an essential element of intellectual economy, as of every other. It may be acknowledged, therefore, that the books are right in making familiarity with a notion the first step toward clearness of apprehension, and the defining of it the second. But in omitting all mention of any higher perspicuity of thought, they simply mirror a philosophy which was exploded a hundred years ago. That much-admired "ornament of logic"—the doctrine of clearness and distinctness—may be pretty enough, but it is high time to relegate to our cabinet of curiosities the antique *bijou*, and to wear about us something better adapted to modern uses.

The very first lesson that we have a right to demand that logic shall teach us is, how to make our ideas clear; and a most important one it is, depreciated only by minds who stand in need of it. To know what we think, to be masters of our own meaning, will make a solid foundation for great and weighty thought. It is most easily learned by those whose ideas are meagre and restricted; and far happier they than such as wallow helplessly in a rich mud of conceptions. A nation, it is true, may, in the course of generations, overcome the disadvantage of an excessive wealth of language and its natural concomitant, a vast, unfathomable deep of ideas. We may see it in history, slowly perfecting its literary forms, sloughing at length its metaphysics, and, by virtue of the untirable patience which is often a compensation, attaining great excellence in every branch of mental acquirement. The page of history is not yet unrolled which is to tell us whether such a people will or will not in the long-run prevail over one whose ideas (like the words of their language) are few, but which possesses a wonderful mastery over those which it has. For an individual, however, there can be no question that a few clear ideas are worth more than many confused ones. A young man would hardly be persuaded to sacrifice the greater part of his thoughts to save the rest; and the muddled head is the least apt to see the necessity of such a sacrifice. Him we can usually only commiserate, as a person with a congenital defect. Time will help him, but intellectual maturity with regard to clearness comes rather late, an unfortunate arrangement of Nature, inasmuch as clearness is of less use to a man settled in life, whose

errors have in great measure had their effect, than it would be to one whose path lies before him. It is terrible to see how a single unclear idea, a single formula without meaning, lurking in a young man's head, will sometimes act like an obstruction of inert matter in an artery, hindering the nutrition of the brain, and condemning its victim to pine away in the fullness of his intellectual vigor and in the midst of intellectual plenty. Many a man has cherished for years as his hobby some vague shadow of an idea, too meaningless to be positively false; he has, nevertheless, passionately loved it, has made it his companion by day and by night, and has given to it his strength and his life, leaving all other occupations for its sake, and in short has lived with it and for it, until it has become, as it were, flesh of his flesh and bone of his bone; and then he has waked up some bright morning to find it gone, clean vanished away like the beautiful Melusina of the fable, and the essence of his life gone with it. I have myself known such a man; and who can tell how many histories of circle-squarers, metaphysicians, astrologers, and what not, may not be told in the old German story?

## II

The principles set forth in the first of these papers lead, at once, to a method of reaching a clearness of thought of a far higher grade than the "distinctness" of the logicians. We have there found that the action of thought is excited by the irritation of doubt, and ceases when belief is attained; so that the production of belief is the sole function of thought. All these words, however, are too strong for my purpose. It is as if I had described the phenomena as they appear under a mental microscope. Doubt and Belief, as the words are commonly employed, relate to religious or other grave discussions. But here I use them to designate the starting of any question, no matter how small or how great, and the resolution of it. If, for instance, in a horse-car, I pull out my purse and find a five-cent nickel and five coppers, I decide, while my hand is going to the purse, in which way I will pay my fare. To call such a question Doubt, and my decision Belief, is certainly to use words very disproportionate to the occasion. To speak of such a doubt as causing an irritation which needs to be appeased, suggests a temper which is uncomfortable to the verge of insanity. Yet, looking at the matter minutely, it must be admitted that, if there is the least hesitation as to whether I shall pay the five coppers or the nickel (as there will be sure to be, unless I act

from some previously contracted habit in the matter), though irritation is too strong a word, yet I am excited to such small mental activity as may be necessary to deciding how I shall act. Most frequently doubts arise from some indecision, however momentary, in our action. Sometimes it is not so. I have, for example, to wait in a railway-station, and to pass the time I read the advertisements on the walls, I compare the advantages of different trains and different routes which I never expect to take, merely fancying myself to be in a state of hesitancy, because I am bored with having nothing to trouble me. Feigned hesitancy, whether feigned for mere amusement or with a lofty purpose, plays a great part in the production of scientific inquiry. However the doubt may originate, it stimulates the mind to an activity which may be slight or energetic, calm or turbulent. Images pass rapidly through consciousness, one incessantly melting into another, until at last, when all is over—it may be in a fraction of a second, in an hour, or after long years—we find ourselves decided as to how we should act under such circumstances as those which occasioned our hesitation. In other words, we have attained belief.

In this process we observe two sorts of elements of consciousness, the distinction between which may best be made clear by means of an illustration. In a piece of music there are the separate notes, and there is the air. A single tone may be prolonged for an hour or a day, and it exists as perfectly in each second of that time as in the whole taken together; so that, as long as it is sounding, it might be present to a sense from which everything in the past was as completely absent as the future itself. But it is different with the air, the performance of which occupies a certain time, during the portions of which only portions of it are played. It consists in an orderliness in the succession of sounds which strike the ear at different times; and to perceive it there must be some continuity of consciousness which makes the events of a lapse of time present to us. We certainly only perceive the air by hearing the separate notes; yet we cannot be said to directly hear it, for we hear only what is present at the instant, and an orderliness of succession cannot exist in an instant. These two sorts of objects, what we are *immediately* conscious of and what we are *mediately* conscious of, are found in all consciousness. Some elements (the sensations) are completely present at every instant so long as they last, while others (like thought) are actions having beginning, middle, and end, and consist in a congruence in the succession of sensations which flow through the

mind. They cannot be immediately present to us, but must cover some portion of the past or future. Thought is a thread of melody running through the succession of our sensations.

We may add that just as a piece of music may be written in parts, each part having its own air, so various systems of relationship of succession subsist together between the same sensations. These different systems are distinguished by having different motives, ideas, or functions. Thought is only one such system, for its sole motive, idea, and function, is to produce belief, and whatever does not concern that purpose belongs to some other system of relations. The action of thinking may incidentally have other results; it may serve to amuse us, for example, and among *dilettanti* it is not rare to find those who have so perverted thought to the purposes of pleasure that it seems to vex them to think that the questions upon which they delight to exercise it may ever get finally settled; and a positive discovery which takes a favorite subject out of the arena of literary debate is met with ill-concealed dislike. This disposition is the very debauchery of thought. But the soul and meaning of thought, abstracted from the other elements which accompany it, though it may be voluntarily thwarted, can never be made to direct itself toward anything but the production of belief. Thought in action has for its only possible motive the attainment of thought at rest; and whatever does not refer to belief is no part of the thought itself.

And what, then, is belief? It is the demi-cadence which closes a musical phrase in the symphony of our intellectual life. We have seen that it has just three properties: First, it is something that we are aware of; second, it appeases the irritation of doubt; and, third, it involves the establishment in our nature of a rule of action, or, say for short, a *habit*. As it appeases the irritation of doubt, which is the motive for thinking, thought relaxes, and comes to rest for a moment when belief is reached. But, since belief is a rule for action, the application of which involves further doubt and further thought, at the same time that it is a stopping-place, it is also a new starting-place for thought. That is why I have permitted myself to call it thought at rest, although thought is essentially an action. The *final* upshot of thinking is the exercise of volition, and of this thought no longer forms a part; but belief is only a stadium of mental action, an effect upon our nature due to thought, which will influence future thinking.

The essence of belief is the establishment of a habit, and different

beliefs are distinguished by the different modes of action to which they give rise. If beliefs do not differ in this respect, if they appease the same doubt by producing the same rule of action, then no mere differences in the manner of consciousness of them can make them different beliefs, any more than playing a tune in different keys is playing different tunes. Imaginary distinctions are often drawn between beliefs which differ only in their mode of expression;—the wrangling which ensues is real enough, however. To believe that any objects are arranged as in Fig. 1, and to believe that they are arranged in Fig. 2, are one and the same belief; yet it is conceivable that a man should assert one proposition and deny the other. Such false distinctions do as much harm as the confusion of beliefs really different, and are among the pitfalls of which we ought constantly to beware, especially when we are upon metaphysical ground. One singular deception of this sort, which often occurs, is to mistake the sensation produced by our own uncleanness of thought for a character of the object we are thinking. Instead of perceiving that the obscurity is purely subjective, we fancy that we contemplate a quality of the object which is essentially mysterious; and if our conception be afterward presented to us in a clear form we do not recognize it as the same, owing to the absence of the feeling of unintelligibility. So long as this deception lasts, it obviously puts an impassable barrier in the way of perspicuous thinking; so that it equally interests the opponents of rational thought to perpetuate it, and its adherents to guard against it.

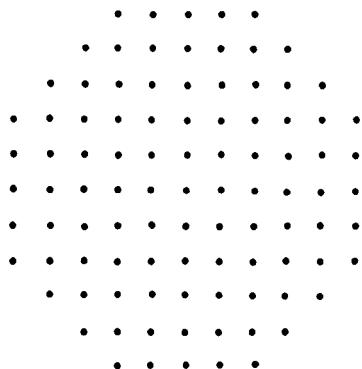


Fig. 1.

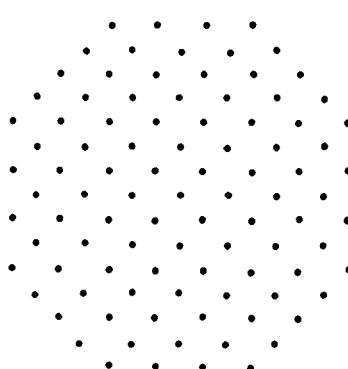


Fig. 2.

Another such deception is to mistake a mere difference in the grammatical construction of two words for a distinction between the ideas they express. In this pedantic age, when the general mob of writers attend so much more to words than to things, this error is common enough. When I just said that thought is an *action*, and that it consists in a *relation*, although a person performs an action but not a relation, which can only be the result of an action, yet there was no inconsistency in what I said, but only a grammatical vagueness.

From all these sophisms we shall be perfectly safe so long as we reflect that the whole function of thought is to produce habits of action; and that whatever there is connected with a thought, but irrelevant to its purpose, is an accretion to it, but no part of it. If there be a unity among our sensations which has no reference to how we shall act on a given occasion, as when we listen to a piece of music, why we do not call that thinking. To develop its meaning, we have, therefore, simply to determine what habits it produces, for what a thing means is simply what habits it involves. Now, the identity of a habit depends on how it might lead us to act, not merely under such circumstances as are likely to arise, but under such as might possibly occur, no matter how improbable they may be. What the habit is depends on *when* and *how* it causes us to act. As for the *when*, every stimulus to action is derived from perception; as for the *how*, every purpose of action is to produce some sensible result. Thus, we come down to what is tangible and practical, as the root of every real distinction of thought, no matter how subtile it may be; and there is no distinction of meaning so fine as to consist in anything but a possible difference of practice.

To see what this principle leads to, consider in the light of it such a doctrine as that of transubstantiation. The Protestant churches generally hold that the elements of the sacrament are flesh and blood only in a tropical sense; they nourish our souls as meat and the juice of it would our bodies. But the Catholics maintain that they are literally just that; although they possess all the sensible qualities of wafer-cakes and diluted wine. But we can have no conception of wine except what may enter into a belief, either—

1. That this, that, or the other, is wine; or,
2. That wine possesses certain properties.

Such beliefs are nothing but self-notifications that we should, upon occasion, act in regard to such things as we believe to be wine according to the qualities which we believe wine to possess. The occasion

of such action would be some sensible perception, the motive of it to produce some sensible result. Thus our action has exclusive reference to what affects the senses, our habit has the same bearing as our action, our belief the same as our habit, our conception the same as our belief; and we can consequently mean nothing by wine but what has certain effects, direct or indirect, upon our senses; and to talk of something as having all the sensible characters of wine, yet being in reality blood, is senseless jargon. Now, it is not my object to pursue the theological question; and having used it as a logical example I drop it, without caring to anticipate the theologian's reply. I only desire to point out how impossible it is that we should have an idea in our minds which relates to anything but conceived sensible effects of things. Our idea of anything *is* our idea of its sensible effects; and if we fancy that we have any other we deceive ourselves, and mistake a mere sensation accompanying the thought for a part of the thought itself. It is absurd to say that thought has any meaning unrelated to its only function. It is foolish for Catholics and Protestants to fancy themselves in disagreement about the elements of the sacrament, if they agree in regard to all their sensible effects, here or hereafter.

It appears, then, that the rule for attaining the third grade of clearness of apprehension is as follows: Consider what effects, which might conceivably have practical bearings, we conceive the object of our conception to have. Then, our conception of these effects is the whole of our conception of the object.

### III

Let us illustrate this rule by some examples; and, to begin with the simplest one possible, let us ask what we mean by calling a thing *hard*. Evidently that it will not be scratched by many other substances. The whole conception of this quality, as of every other, lies in its conceived effects. There is absolutely no difference between a hard thing and a soft thing so long as they are not brought to the test. Suppose, then, that a diamond could be crystallized in the midst of a cushion of soft cotton, and should remain there until it was finally burned up. Would it be false to say that that diamond was soft? This seems a foolish question, and would be so, in fact, except in the realm of logic. There such questions are often of the greatest utility as serving to bring logical principles into sharper relief than real discussions ever could. In studying logic we must not put them aside with hasty answers, but must consider them with attentive care, in order

to make out the principles involved. We may, in the present case, modify our question, and ask what prevents us from saying that all hard bodies remain perfectly soft until they are touched, when their hardness increases with the pressure until they are scratched. Reflection will show that the reply is this: there would be no *falsity* in such modes of speech. They would involve a modification of our present usage of speech with regard to the words hard and soft, but not of their meanings. For they represent no fact to be different from what it is; only they involve arrangements of facts which would be exceedingly maladroit. This leads us to remark that the question of what would occur under circumstances which do not actually arise is not a question of fact, but only of the most perspicuous arrangement of them. For example, the question of free-will and fate in its simplest form, stripped of verbiage, is something like this: I have done something of which I am ashamed; could I, by an effort of the will, have resisted the temptation, and done otherwise? The philosophical reply is, that this is not a question of fact, but only of the arrangement of facts. Arranging them so as to exhibit what is particularly pertinent to my question—namely, that I ought to blame myself for having done wrong—it is perfectly true to say that, if I had willed to do otherwise than I did, I should have done otherwise. On the other hand, arranging the facts so as to exhibit another important consideration, it is equally true that, when a temptation has once been allowed to work, it will, if it has a certain force, produce its effect, let me struggle how I may. There is no objection to a contradiction in what would result from a false supposition. The *reductio ad absurdum* consists in showing that contradictory results would follow from a hypothesis which is consequently judged to be false. Many questions are involved in the free-will discussion, and I am far from desiring to say that both sides are equally right. On the contrary, I am of opinion that one side denies important facts, and that the other does not. But what I do say is, that the above single question was the origin of the whole doubt; that, had it not been for this question, the controversy would never have arisen; and that this question is perfectly solved in the manner which I have indicated.

Let us next seek a clear idea of Weight. This is another very easy case. To say that a body is heavy means simply that, in the absence of opposing force, it will fall. This (neglecting certain specifications of how it will fall, etc., which exist in the mind of the physicist who uses the word) is evidently the whole conception of weight. It is a fair question whether some particular facts may not account for gravity;

but what we mean by the force itself is completely involved in its effects.

This leads us to undertake an account of the idea of Force in general. This is the great conception which, developed in the early part of the seventeenth century from the rude idea of a cause, and constantly improved upon since, has shown us how to explain all the changes of motion which bodies experience, and how to think about all physical phenomena; which has given birth to modern science, and changed the face of the globe; and which, aside from its more special uses, has played a principal part in directing the course of modern thought, and in furthering modern social development. It is, therefore, worth some pains to comprehend it. According to our rule, we must begin by asking what is the immediate use of thinking about force; and the answer is, that we thus account for changes of motion. If bodies were left to themselves, without the intervention of forces, every motion would continue unchanged both in velocity and in direction. Furthermore, change of motion never takes place abruptly; if its direction is changed, it is always through a curve without angles; if its velocity alters, it is by degrees. The gradual changes which are constantly taking place are conceived by geometers to be compounded together according to the rules of the parallelogram of forces. If the reader does not already know what this is, he will find it, I hope, to his advantage to endeavor to follow the following explanation; but if mathematics are insupportable to him, pray let him skip three paragraphs rather than that we should part company here.

A *path* is a line whose beginning and end are distinguished. Two paths are considered to be equivalent, which, beginning at the same point, lead to the same point. Thus the two paths, *ABCDE* and *AFGHE*, are equivalent. Paths which do *not* begin at the same point are considered to be equivalent, provided that, on moving either of them without turning it, but keeping it always parallel to its original position, when its beginning coincides with that of the other path, the ends also coincide. Paths are considered as geometrically added together, when one begins where the other ends; thus the path *AE* is conceived to be a sum of *AB*, *BC*, *CD*, and *DE*. In the parallelogram of Fig. 4 the diagonal *AC* is the sum of *AB* and *BC*; or, since *AD* is geometrically equivalent to *BC*, *AC* is the geometrical sum of *AB* and *AD*.

All this is purely conventional. It simply amounts to this: that we choose to call paths having the relations I have described equal or

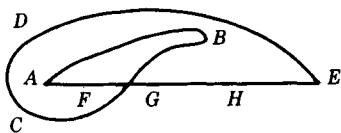


Fig. 3.

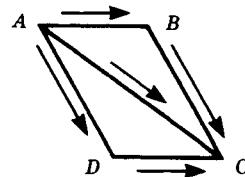


Fig. 4.

added. But, though it is a convention, it is a convention with a good reason. The rule for geometrical addition may be applied not only to paths, but to any other things which can be represented by paths. Now, as a path is determined by the varying direction and distance of the point which moves over it from the starting-point, it follows that anything which from its beginning to its end is determined by a varying direction and a varying magnitude is capable of being represented by a line. Accordingly, *velocities* may be represented by lines, for they have only directions and rates. The same thing is true of *accelerations*, or changes of velocities. This is evident enough in the case of velocities; and it becomes evident for accelerations if we consider that precisely what velocities are to positions—namely, states of change of them—that accelerations are to velocities.

The so-called “parallelogram of forces” is simply a rule for compounding accelerations. The rule is, to represent the accelerations by paths, and then to geometrically add the paths. The geometers, however, not only use the “parallelogram of forces” to compound different accelerations, but also to resolve one acceleration into a sum of several. Let  $AB$  (Fig. 5) be the path which represents a certain acceleration—say, such a change in the motion of a body that at the end of one second the body will, under the influence of that change, be in a position different from what it would have had if its motion had

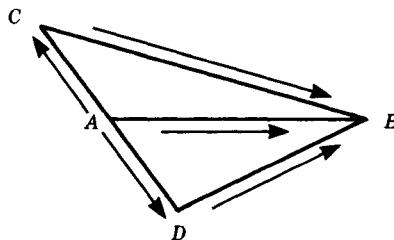


Fig. 5.

continued unchanged such that a path equivalent to  $AB$  would lead from the latter position to the former. This acceleration may be considered as the sum of the accelerations represented by  $AC$  and  $CB$ . It may also be considered as the sum of the very different accelerations represented by  $AD$  and  $DB$ , where  $AD$  is almost the opposite of  $AC$ . And it is clear that there is an immense variety of ways in which  $AB$  might be resolved into the sum of two accelerations.

After this tedious explanation, which I hope, in view of the extraordinary interest of the conception of force, may not have exhausted the reader's patience, we are prepared at last to state the grand fact which this conception embodies. This fact is that if the actual changes of motion which the different particles of bodies experience are each resolved in its appropriate way, each component acceleration is precisely such as is prescribed by a certain law of Nature, according to which bodies in the relative positions which the bodies in question actually have at the moment,<sup>1</sup> always receive certain accelerations, which, being compounded by geometrical addition, give the acceleration which the body actually experiences.

This is the only fact which the idea of force represents, and whoever will take the trouble clearly to apprehend what this fact is, perfectly comprehends what force is. Whether we ought to say that a force *is* an acceleration, or that it *causes* an acceleration, is a mere question of propriety of language, which has no more to do with our real meaning than the difference between the French idiom "*Il fait froid*" and its English equivalent "*It is cold.*" Yet it is surprising to see how this simple affair has muddled men's minds. In how many profound treatises is not force spoken of as a "mysterious entity," which seems to be only a way of confessing that the author despairs of ever getting a clear notion of what the word means! In a recent admired work on "Analytic Mechanics" it is stated that we understand precisely the effect of force, but what force itself is we do not understand! This is simply a self-contradiction. The idea which the word force excites in our minds has no other function than to affect our actions, and these actions can have no reference to force otherwise than through its effects. Consequently, if we know what the effects of force are, we are acquainted with every fact which is implied in saying that a force exists, and there is nothing more to know. The truth is, there is some vague notion afloat that a question may

1. Possibly the velocities also have to be taken into account.

mean something which the mind cannot conceive; and when some hair-splitting philosophers have been confronted with the absurdity of such a view, they have invented an empty distinction between positive and negative conceptions, in the attempt to give their non-idea a form not obviously nonsensical. The nullity of it is sufficiently plain from the considerations given a few pages back; and, apart from those considerations, the quibbling character of the distinction must have struck every mind accustomed to real thinking.

## IV

Let us now approach the subject of logic, and consider a conception which particularly concerns it, that of *reality*. Taking clearness in the sense of familiarity, no idea could be clearer than this. Every child uses it with perfect confidence, never dreaming that he does not understand it. As for clearness in its second grade, however, it would probably puzzle most men, even among those of a reflective turn of mind, to give an abstract definition of the real. Yet such a definition may perhaps be reached by considering the points of difference between reality and its opposite, fiction. A figment is a product of somebody's imagination; it has such characters as his thought impresses upon it. That whose characters are independent of how you or I think is an external reality. There are, however, phenomena within our own minds, dependent upon our thought, which are at the same time real in the sense that we really think them. But though their characters depend on how we think, they do not depend on what we think those characters to be. Thus, a dream has a real existence as a mental phenomenon, if somebody has really dreamt it; that he dreamt so and so, does not depend on what anybody thinks was dreamt, but is completely independent of all opinion on the subject. On the other hand, considering, not the fact of dreaming, but the thing dreamt, it retains its peculiarities by virtue of no other fact than that it was dreamt to possess them. Thus we may define the real as that whose characters are independent of what anybody may think them to be.

But, however satisfactory such a definition may be found, it would be a great mistake to suppose that it makes the idea of reality perfectly clear. Here, then, let us apply our rules. According to them, reality, like every other quality, consists in the peculiar sensible effects which things partaking of it produce. The only effect which real things have is to cause belief, for all the sensations which they excite emerge into

consciousness in the form of beliefs. The question therefore is, how is true belief (or belief in the real) distinguished from false belief (or belief in fiction). Now, as we have seen in the former paper, the ideas of truth and falsehood, in their full development, appertain exclusively to the scientific method of settling opinion. A person who arbitrarily chooses the propositions which he will adopt can use the word truth only to emphasize the expression of his determination to hold on to his choice. Of course, the method of tenacity never prevailed exclusively; reason is too natural to men for that. But in the literature of the dark ages we find some fine examples of it. When Scotus Erigena is commenting upon a poetical passage in which hellebore is spoken of as having caused the death of Socrates, he does not hesitate to inform the inquiring reader that Helleborus and Socrates were two eminent Greek philosophers, and that the latter having been overcome in argument by the former took the matter to heart and died of it! What sort of an idea of truth could a man have who could adopt and teach, without the qualification of a perhaps, an opinion taken so entirely at random? The real spirit of Socrates, who I hope would have been delighted to have been "overcome in argument," because he would have learned something by it, is in curious contrast with the naïve idea of the glossist, for whom discussion would seem to have been simply a struggle. When philosophy began to awake from its long slumber, and before theology completely dominated it, the practice seems to have been for each professor to seize upon any philosophical position he found unoccupied and which seemed a strong one, to intrench himself in it, and to sally forth from time to time to give battle to the others. Thus, even the scanty records we possess of those disputes enable us to make out a dozen or more opinions held by different teachers at one time concerning the question of nominalism and realism. Read the opening part of the "*Historia Calamitatum*" of Abelard, who was certainly as philosophical as any of his contemporaries, and see the spirit of combat which it breathes. For him, the truth is simply his particular stronghold. When the method of authority prevailed, the truth meant little more than the Catholic faith. All the efforts of the scholastic doctors are directed toward harmonizing their faith in Aristotle and their faith in the Church, and one may search their ponderous folios through without finding an argument which goes any further. It is noticeable that where different faiths flourish side by side, renegades are looked upon with contempt even by the party whose belief they adopt; so completely has the idea of loyalty replaced that of truth-seeking. Since the

time of Descartes, the defect in the conception of truth has been less apparent. Still, it will sometimes strike a scientific man that the philosophers have been less intent on finding out what the facts are, than on inquiring what belief is most in harmony with their system. It is hard to convince a follower of the *a priori* method by adducing facts; but show him that an opinion he is defending is inconsistent with what he has laid down elsewhere, and he will be very apt to retract it. These minds do not seem to believe that disputation is ever to cease; they seem to think that the opinion which is natural for one man is not so for another, and that belief will, consequently, never be settled. In contenting themselves with fixing their own opinions by a method which would lead another man to a different result, they betray their feeble hold of the conception of what truth is.

On the other hand, all the followers of science are fully persuaded that the processes of investigation, if only pushed far enough, will give one certain solution to every question to which they can be applied. One man may investigate the velocity of light by studying the transits of Venus and the aberration of the stars; another by the oppositions of Mars and the eclipses of Jupiter's satellites; a third by the method of Fizeau; a fourth by that of Foucault; a fifth by the motions of the curves of Lissajous; a sixth, a seventh, an eighth, and a ninth, may follow the different methods of comparing the measures of statical and dynamical electricity. They may at first obtain different results, but, as each perfects his method and his processes, the results will move steadily together toward a destined centre. So with all scientific research. Different minds may set out with the most antagonistic views, but the progress of investigation carries them by a force outside of themselves to one and the same conclusion. This activity of thought by which we are carried, not where we wish, but to a foreordained goal, is like the operation of destiny. No modification of the point of view taken, no selection of other facts for study, no natural bent of mind even, can enable a man to escape the predestinate opinion. This great law is embodied in the conception of truth and reality. The opinion which is fated<sup>2</sup> to be ultimately agreed to by all who investigate, is what we mean by the truth, and the object represented in this opinion is the real. That is the way I would explain reality.

But it may be said that this view is directly opposed to the abstract

2. Fate means merely that which is sure to come true, and can nohow be avoided. It is a superstition to suppose that a certain sort of events are ever fated, and it is another to suppose that the word fate can never be freed from its superstitious taint. We are all fated to die.

definition which we have given of reality, inasmuch as it makes the characters of the real to depend on what is ultimately thought about them. But the answer to this is that, on the one hand, reality is independent, not necessarily of thought in general, but only of what you or I or any finite number of men may think about it; and that, on the other hand, though the object of the final opinion depends on what that opinion is, yet what that opinion is does not depend on what you or I or any man thinks. Our perversity and that of others may indefinitely postpone the settlement of opinion; it might even conceivably cause an arbitrary proposition to be universally accepted as long as the human race should last. Yet even that would not change the nature of the belief, which alone could be the result of investigation carried sufficiently far; and if, after the extinction of our race, another should arise with faculties and disposition for investigation, that true opinion must be the one which they would ultimately come to. "Truth crushed to earth shall rise again," and the opinion which would finally result from investigation does not depend on how anybody may actually think. But the reality of that which is real does depend on the real fact that investigation is destined to lead, at last, if continued long enough, to a belief in it.

But I may be asked what I have to say to all the minute facts of history, forgotten never to be recovered, to the lost books of the ancients, to the buried secrets.

Full many a gem of purest ray serene  
 The dark, unfathomed caves of ocean bear;  
 Full many a flower is born to blush unseen,  
 And waste its sweetness on the desert air.

Do these things not really exist because they are hopelessly beyond the reach of our knowledge? And then, after the universe is dead (according to the prediction of some scientists), and all life has ceased forever, will not the shock of atoms continue though there will be no mind to know it? To this I reply that, though in no possible state of knowledge can any number be great enough to express the relation between the amount of what rests unknown to the amount of the known, yet it is unphilosophical to suppose that, with regard to any given question (which has any clear meaning), investigation would not bring forth a solution of it, if it were carried far enough. Who would have said, a few years ago, that we could ever know of what substances stars are made whose light may have been longer in reaching us than the human race has existed? Who can be sure of

what we shall not know in a few hundred years? Who can guess what would be the result of continuing the pursuit of science for ten thousand years, with the activity of the last hundred? And if it were to go on for a million, or a billion, or any number of years you please, how is it possible to say that there is any question which might not ultimately be solved?

But it may be objected, "Why make so much of these remote considerations, especially when it is your principle that only practical distinctions have a meaning?" Well, I must confess that it makes very little difference whether we say that a stone on the bottom of the ocean, in complete darkness, is brilliant or not—that is to say, that it *probably* makes no difference, remembering always that that stone *may* be fished up to-morrow. But that there are gems at the bottom of the sea, flowers in the untraveled desert, etc., are propositions which, like that about a diamond being hard when it is not pressed, concern much more the arrangement of our language than they do the meaning of our ideas.

It seems to me, however, that we have, by the application of our rule, reached so clear an apprehension of what we mean by reality, and of the fact which the idea rests on, that we should not, perhaps, be making a pretension so presumptuous as it would be singular, if we were to offer a metaphysical theory of existence for universal acceptance among those who employ the scientific method of fixing belief. However, as metaphysics is a subject much more curious than useful, the knowledge of which, like that of a sunken reef, serves chiefly to enable us to keep clear of it, I will not trouble the reader with any more Ontology at this moment. I have already been led much further into that path than I should have desired; and I have given the reader such a dose of mathematics, psychology, and all that is most abstruse, that I fear he may already have left me, and that what I am now writing is for the compositor and proof-reader exclusively. I trusted to the importance of the subject. There is no royal road to logic, and really valuable ideas can only be had at the price of close attention. But I know that in the matter of ideas the public prefer the cheap and nasty; and in my next paper I am going to return to the easily intelligible, and not wander from it again. The reader who has been at the pains of wading through this month's paper, shall be rewarded in the next one by seeing how beautifully what has been developed in this tedious way can be applied to the ascertainment of the rules of scientific reasoning.

We have, hitherto, not crossed the threshold of scientific logic. It

is certainly important to know how to make our ideas clear, but they may be ever so clear without being true. How to make them so, we have next to study. How to give birth to those vital and procreative ideas which multiply into a thousand forms and diffuse themselves everywhere, advancing civilization and making the dignity of man, is an art not yet reduced to rules, but of the secret of which the history of science affords some hints.

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## *The Doctrine of Chances*

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### I

It is a common observation that a science first begins to be exact when it is quantitatively treated. What are called the exact sciences are no others than the mathematical ones. Chemists reasoned vaguely until Lavoisier showed them how to apply the balance to the verification of their theories, when chemistry leaped suddenly into the position of the most perfect of the classificatory sciences. It has thus become so precise and certain that we usually think of it along with optics, thermotics, and electrics. But these are studies of general laws, while chemistry considers merely the relations and classification of certain objects; and belongs, in reality, in the same category as systematic botany and zoölogy. Compare it with these last, however, and the advantage that it derives from its quantitative treatment is very evident.

The rudest numerical scales, such as that by which the mineralogists distinguish the different degrees of hardness, are found useful. The mere counting of pistils and stamens sufficed to bring botany out of total chaos into some kind of form. It is not, however, so much from *counting* as from *measuring*, not so much from the conception of

number as from that of continuous quantity, that the advantage of mathematical treatment comes. Number, after all, only serves to pin us down to a precision in our thoughts which, however beneficial, can seldom lead to lofty conceptions, and frequently descends to pettiness. Of those two faculties of which Bacon speaks, that which marks differences and that which notes resemblances, the employment of number can only aid the lesser one; and the excessive use of it must tend to narrow the powers of the mind. But the conception of continuous quantity has a great office to fulfill, independently of any attempt at precision. Far from tending to the exaggeration of differences, it is the direct instrument of the finest generalizations. When a naturalist wishes to study a species, he collects a considerable number of specimens more or less similar. In contemplating them, he observes certain ones which are more or less alike in some particular respect. They all have, for instance, a certain S-shaped marking. He observes that they are not *precisely* alike, in this respect; the S has not precisely the same shape, but the differences are such as to lead him to believe that forms could be found intermediate between any two of those he possesses. He, now, finds other forms apparently quite dissimilar—say a marking in the form of a C—and the question is, whether he can find intermediate ones which will connect these latter with the others. This he often succeeds in doing in cases where it would at first be thought impossible; whereas, he sometimes finds those which differ, at first glance, much less, to be separated in Nature by the non-occurrence of intermediaries. In this way, he builds up from the study of Nature a new general conception of the character in question. He obtains, for example, an idea of a leaf which includes every part of the flower, and an idea of a vertebra which includes the skull. I surely need not say much to show what a logical engine there is here. It is the essence of the method of the naturalist. How he applies it first to one character, and then to another, and finally obtains a notion of a species of animals, the differences between whose members, however great, are confined within limits, is a matter which does not here concern us. The whole method of classification must be considered later; but, at present, I only desire to point out that it is by taking advantage of the idea of continuity, or the passage from one form to another by insensible degrees, that the naturalist builds his conceptions. Now, the naturalists are the great builders of conceptions; there is no other branch of science where so much of this work is done as in theirs; and we must, in great

measure, take them for our teachers in this important part of logic. And it will be found everywhere that the idea of continuity is a powerful aid to the formation of true and fruitful conceptions. By means of it, the greatest differences are broken down and resolved into differences of degree, and the incessant application of it is of the greatest value in broadening our conceptions. I propose to make a great use of this idea in the present series of papers; and the particular series of important fallacies, which, arising from a neglect of it, have desolated philosophy, must further on be closely studied. At present, I simply call the reader's attention to the utility of this conception.

In studies of numbers, the idea of continuity is so indispensable, that it is perpetually introduced even where there is no continuity in fact, as where we say that there are in the United States 10.7 inhabitants per square mile, or that in New York 14.72 persons live in the average house.<sup>1</sup> Another example is that law of the distribution of errors which Quetelet, Galton, and others, have applied with so much success to the study of biological and social matters. This application of continuity to cases where it does not really exist illustrates, also, another point which will hereafter demand a separate study, namely, the great utility which fictions sometimes have in science.

## II

The theory of probabilities is simply the science of logic quantitatively treated. There are two conceivable certainties with reference to any hypothesis, the certainty of its truth and the certainty of its falsity. The numbers *one* and *zero* are appropriated, in this calculus, to marking these extremes of knowledge; while fractions having values intermediate between them indicate, as we may vaguely say, the degrees in which the evidence leans toward one or the other. The general problem of probabilities is, from a given state of facts, to determine the numerical probability of a possible fact. This is the same as to inquire how much the given facts are worth, considered as evidence to prove the possible fact. Thus the problem of probabilities is simply the general problem of logic.

1. This mode of thought is so familiarly associated with all exact numerical consideration, that the phrase appropriate to it is imitated by shallow writers in order to produce the appearance of exactitude where none exists. Certain newspapers which affect a learned tone talk of "the average man," when they simply mean *most men*, and have no idea of striking an average.

Probability is a continuous quantity, so that great advantages may be expected from this mode of studying logic. Some writers have gone so far as to maintain that, by means of the calculus of chances, every solid inference may be represented by legitimate arithmetical operations upon the numbers given in the premises. If this be, indeed, true, the great problem of logic, how it is that the observation of one fact can give us knowledge of another independent fact, is reduced to a mere question of arithmetic. It seems proper to examine this pretension before undertaking any more recondite solution of the paradox.

But, unfortunately, writers on probabilities are not agreed in regard to this result. This branch of mathematics is the only one, I believe, in which good writers frequently get results entirely erroneous. In elementary geometry the reasoning is frequently fallacious, but erroneous conclusions are avoided; but it may be doubted if there is a single extensive treatise on probabilities in existence which does not contain solutions absolutely indefensible. This is partly owing to the want of any regular method of procedure; for the subject involves too many subtleties to make it easy to put its problems into equations without such an aid. But, beyond this, the fundamental principles of its calculus are more or less in dispute. In regard to that class of questions to which it is chiefly applied for practical purposes, there is comparatively little doubt; but in regard to others to which it has been sought to extend it, opinion is somewhat unsettled.

This last class of difficulties can only be entirely overcome by making the idea of probability perfectly clear in our minds in the way set forth in our last paper.

### III

To get a clear idea of what we mean by probability, we have to consider what real and sensible difference there is between one degree of probability and another.

The character of probability belongs primarily, without doubt, to certain inferences. Locke explains it as follows: 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 thus 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; i.e., 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.

The celebrated *Essay concerning Humane Understanding* contains many passages which, like this one, make the first steps in profound analyses which are not further developed. It was shown in the first of these papers that the validity of an inference does not depend on any tendency of the mind to accept it, however strong such tendency may be; but consists in the real fact that, when premises like those of the argument in question are true, conclusions related to them like that of this argument are also true. It was remarked that in a logical mind an argument is always conceived as a member of a *genus* of arguments all constructed in the same way, and such that, when their premises are real facts, their conclusions are so also. If the argument is demonstrative, then this is always so; if it is only probable, then it is for the most part so. As Locke says, the probable argument is "*such as* for the most part carries truth with it."

According to this, that real and sensible difference between one degree of probability and another, in which the meaning of the distinction lies, is that in the frequent employment of two different modes of inference, one will carry truth with it oftener than the other. It is evident that this is the only difference there is in the existing fact. Having certain premises, a man draws a certain conclusion, and as far as this inference alone is concerned the only possible practical question is whether that conclusion is true or not, and between existence and non-existence there is no middle term. "Being only is and nothing is altogether not," said Parmenides; and this is in strict accordance with the analysis of the conception of reality given in the last paper. For we found that the distinction of reality and fiction depends on the supposition that sufficient investigation would cause one opinion to be universally received and all others to be rejected. That presupposition involved in the very conceptions of reality and figment involves a complete sundering of the two. It is the heaven-and-hell idea in the domain of thought. But, in the long run, there is a real fact which corresponds to the idea of probability, and it is that a given mode of inference sometimes proves successful and sometimes not, and that in a ratio ultimately fixed. As we go on drawing inference after inference of the given kind, during the first ten or hundred cases the ratio of successes may

be expected to show considerable fluctuations; but when we come into the thousands and millions, these fluctuations become less and less; and if we continue long enough, the ratio will approximate toward a fixed limit. We may therefore define the probability of a mode of argument as the proportion of cases in which it carries truth with it.

The inference from the premise, A, to the conclusion, B, depends, as we have seen, on the guiding principle, that if a fact of the class A is true, a fact of the class B is true. The probability consists of the fraction whose numerator is the number of times in which both A and B are true, and whose denominator is the total number of times in which A is true, whether B is so or not. Instead of speaking of this as the probability of the inference, there is not the slightest objection to calling it the probability that, if A happens, B happens. But to speak of the probability of the event B, without naming the condition, really has no meaning at all. It is true that when it is perfectly obvious what condition is meant, the ellipsis may be permitted. But we should avoid contracting the habit of using language in this way (universal as the habit is), because it gives rise to a vague way of thinking, as if the action of causation might either determine an event to happen or determine it not to happen, or leave it more or less free to happen or not, so as to give rise to an *inherent* chance in regard to its occurrence. It is quite clear to me that some of the worst and most persistent errors in the use of the doctrine of chances have arisen from this vicious mode of expression.<sup>2</sup>

#### IV

But there remains an important point to be cleared up. According to what has been said, the idea of probability essentially belongs to a kind of inference which is repeated indefinitely. An individual inference must be either true or false, and can show no effect of probability; and, therefore, in reference to a single case considered in itself, probability can have no meaning. Yet if a man had to choose between drawing a card from a pack containing twenty-five red cards and a black one, or from a pack containing twenty-five black cards and a red one, and if the drawing of a red card were destined

2. The conception of probability here set forth is substantially that first developed by Mr. Venn, in his *Logic of Chance*. Of course, a vague apprehension of the idea had always existed, but the problem was to make it perfectly clear, and to him belongs the credit of first doing this.

to transport him to eternal felicity, and that of a black one to consign him to everlasting woe, it would be folly to deny that he ought to prefer the pack containing the larger proportion of red cards, although, from the nature of the risk, it could not be repeated. It is not easy to reconcile this with our analysis of the conception of chance. But suppose he should choose the red pack, and should draw the wrong card, what consolation would he have? He might say that he had acted in accordance with reason, but that would only show that his reason was absolutely worthless. And if he should choose the right card, how could he regard it as anything but a happy accident? He could not say that if he had drawn from the other pack, he might have drawn the wrong one, because an hypothetical proposition such as, "if A, then B," means nothing with reference to a single case. Truth consists in the existence of a real fact corresponding to the true proposition. Corresponding to the proposition, "if A, then B," there may be the fact that *whenever* such an event as A happens such an event as B happens. But in the case supposed, which has no parallel as far as this man is concerned, there would be no real fact whose existence could give any truth to the statement that, if he had drawn from the other pack, he might have drawn a black card. Indeed, since the validity of an inference consists in the truth of the hypothetical proposition that *if* the premises be true the conclusion will also be true, and since the only real fact which can correspond to such a proposition is that whenever the antecedent is true the consequent is so also, it follows that there can be no sense in reasoning in an isolated case, at all.

These considerations appear, at first sight, to dispose of the difficulty mentioned. Yet the case of the other side is not yet exhausted. Although probability will probably manifest its effect in, say, a thousand risks, by a certain proportion between the numbers of successes and failures, yet this, as we have seen, is only to say that it certainly will, at length, do so. Now the number of risks, the number of probable inferences, which a man draws in his whole life, is a finite one, and he cannot be absolutely *certain* that the mean result will accord with the probabilities at all. Taking all his risks collectively, then, it cannot be certain that they will not fail, and his case does not differ, except in degree, from the one last supposed. It is an indubitable result of the theory of probabilities that every gambler, if he continues long enough, must ultimately be ruined. Suppose he tries the martingale, which some believe infallible, and which is, as I am

informed, disallowed in the gambling-houses. In this method of playing, he first bets say \$1; if he loses it he bets \$2; if he loses that he bets \$4; if he loses that he bets \$8; if he then gains he has lost  $1 + 2 + 4 = 7$ , and he has gained \$1 more; and no matter how many bets he loses, the first one he gains will make him \$1 richer than he was in the beginning. In that way, he will probably gain at first; but, at last, the time will come when the run of luck is so against him that he will not have money enough to double, and must therefore let his bet go. This will *probably* happen before he has won as much as he had in the first place, so that this run against him will leave him poorer than he began; some time or other it will be sure to happen. It is true that there is always a possibility of his winning any sum the bank can pay, and we thus come upon a celebrated paradox that, though he is certain to be ruined, the value of his expectation calculated according to the usual rules (which omit this consideration) is large. But, whether a gambler plays in this way or any other, the same thing is true, namely, that if he plays long enough he will be sure some time to have such a run against him as to exhaust his entire fortune. The same thing is true of an insurance company. Let the directors take the utmost pains to be independent of great conflagrations and pestilences, their actuaries can tell them that, according to the doctrine of chances, the time must come, at last, when their losses will bring them to a stop. They may tide over such a crisis by extraordinary means, but then they will start again in a weakened state, and the same thing will happen again all the sooner. An actuary might be inclined to deny this, because he knows that the expectation of his company is large, or perhaps (neglecting the interest upon money) is infinite. But calculations of expectations leave out of account the circumstance now under consideration, which reverses the whole thing. However, I must not be understood as saying that insurance is on this account unsound, more than other kinds of business. All human affairs rest upon probabilities, and the same thing is true everywhere. If man were immortal he could be perfectly sure of seeing the day when everything in which he had trusted should betray his trust, and, in short, of coming eventually to hopeless misery. He would break down, at last, as every great fortune, as every dynasty, as every civilization does. In place of this we have death.

But what, without death, would happen to every man, with death must happen to some man. At the same time, death makes the number of our risks, of our inferences, finite, and so makes their mean

result uncertain. The very idea of probability and of reasoning rests on the assumption that this number is indefinitely great. We are thus landed in the same difficulty as before, and I can see but one solution of it. It seems to me that we are driven to this, that logicality inexorably requires that our interests shall *not* be limited. They must not stop at our own fate, but must embrace the whole community. This community, again, must not be limited, but must extend to all races of beings with whom we can come into immediate or mediate intellectual relation. It must reach, however vaguely, beyond this geological epoch, beyond all bounds. He who would not sacrifice his own soul to save the whole world, is, as it seems to me, illogical in all his inferences, collectively. Logic is rooted in the social principle.

To be logical men should not be selfish; and, in point of fact, they are not so selfish as they are thought. The willful prosecution of one's desires is a different thing from selfishness. The miser is not selfish; his money does him no good, and he cares for what shall become of it after his death. We are constantly speaking of *our* possessions on the Pacific, and of *our* destiny as a republic, where no personal interests are involved, in a way which shows that we have wider ones. We discuss with anxiety the possible exhaustion of coal in some hundreds of years, or the cooling-off of the sun in some millions, and show in the most popular of all religious tenets that we can conceive the possibility of a man's descending into hell for the salvation of his fellows.

Now, it is not necessary for logicality that a man should himself be capable of the heroism of self-sacrifice. It is sufficient that he should recognize the possibility of it, should perceive that only that man's inferences who has it are really logical, and should consequently regard his own as being only so far valid as they would be accepted by the hero. So far as he thus refers his inferences to that standard, he becomes identified with such a mind.

This makes logicality attainable enough. Sometimes we can personally attain to heroism. The soldier who runs to scale a wall knows that he will probably be shot, but that is not all he cares for. He also knows that if all the regiment, with whom in feeling he identifies himself, rush forward at once, the fort will be taken. In other cases we can only imitate the virtue. The man whom we have supposed as having to draw from the two packs, who if he is not a logician will draw from the red pack from mere habit, will see, if he is logician enough, that he cannot be logical so long as he is concerned only with his own fate, but that that man who should care equally for what was

to happen in all possible cases of the sort could act logically, and would draw from the pack with the most red cards, and thus, though incapable himself of such sublimity, our logician would imitate the effect of that man's courage in order to share his logicality.

But all this requires a conceived identification of one's interests with those of an unlimited community. Now, there exist no reasons, and a later discussion will show that there can be no reasons, for thinking that the human race, or any intellectual race, will exist forever. On the other hand, there can be no reason against it;<sup>3</sup> and, fortunately, as the whole requirement is that we should have certain sentiments, there is nothing in the facts to forbid our having a *hope*, or calm and cheerful wish, that the community may last beyond any assignable date.

It may seem strange that I should put forward three sentiments, namely, interest in an indefinite community, recognition of the possibility of this interest being made supreme, and hope in the unlimited continuance of intellectual activity, as indispensable requirements of logic. Yet, when we consider that logic depends on a mere struggle to escape doubt, which, as it terminates in action, must begin in emotion, and that, furthermore, the only cause of our planting ourselves on reason is that other methods of escaping doubt fail on account of the social impulse, why should we wonder to find social sentiment presupposed in reasoning? As for the other two sentiments which I find necessary, they are so only as supports and accessories of that. It interests me to notice that these three sentiments seem to be pretty much the same as that famous trio of Charity, Faith, and Hope, which, in the estimation of St. Paul, are the finest and greatest of spiritual gifts. Neither Old nor New Testament is a text-book of the logic of science, but the latter is certainly the highest existing authority in regard to the dispositions of heart which a man ought to have.

## V

Such average statistical numbers as the number of inhabitants per square mile, the average number of deaths per week, the number of convictions per indictment, or, generally speaking, the number of  $x$ 's per  $y$ , where the  $x$ 's are a class of things some or all of which are

3. I do not here admit an absolutely unknowable. Evidence could show us what would probably be the case after any given lapse of time; and though a subsequent time might be assigned which that evidence might not cover, yet further evidence would cover it.

connected with another class of things, their  $y$ 's, I term *relative numbers*. Of the two classes of things to which a relative number refers, that one of which it is a number may be called its *relate*, and that one *per* which the numeration is made may be called its *correlate*.

Probability is a kind of relative number; namely, it is the ratio of the number of arguments of a certain genus which carry truth with them to the total number of arguments of that genus, and the rules for the calculation of probabilities are very easily derived from this consideration. They may all be given here, since they are extremely simple, and it is sometimes convenient to know something of the elementary rules of calculation of chances.

**RULE I. Direct Calculation.**—To calculate, directly, any relative number, say for instance the number of passengers in the average trip of a street-car, we must proceed as follows:

Count the number of passengers for each trip; add all these numbers, and divide by the number of trips. There are cases in which this rule may be simplified. Suppose we wish to know the number of inhabitants to a dwelling in New York. The same person cannot inhabit two dwellings. If he divide his time between two dwellings he ought to be counted a half-inhabitant of each. In this case we have only to divide the total number of the inhabitants of New York by the number of their dwellings, without the necessity of counting separately those which inhabit each one. A similar proceeding will apply wherever each individual relate belongs to one individual correlate exclusively. If we want the number of  $x$ 's per  $y$ , and no  $x$  belongs to more than one  $y$ , we have only to divide the whole number of  $x$ 's of  $y$ 's by the number of  $y$ 's. Such a method would, of course, fail if applied to finding the average number of street-car passengers per trip. We could not divide the total number of travelers by the number of trips, since many of them would have made many passages.

To find the probability that from a given class of premises, A, a given class of conclusions, B, follow, it is simply necessary to ascertain what proportion of the times in which premises of that class are true, the appropriate conclusions are also true. In other words, it is the number of cases of the occurrence of both the events A and B, divided by the total number of cases of the occurrence of the event A.

**RULE II. Addition of Relative Numbers.**—Given two relative numbers having the same correlate, say the number of  $x$ 's per  $y$ , and

the number of  $z$ 's per  $y$ ; it is required to find the number of  $x$ 's and  $z$ 's together per  $y$ . If there is nothing which is at once an  $x$  and a  $z$  to the same  $y$ , the sum of the two given numbers would give the required number. Suppose, for example, that we had given the average number of friends that men have, and the average number of enemies, the sum of these two is the average number of persons interested in a man. On the other hand, it plainly would not do to add the average number of persons having constitutional diseases to the average number over military age, and to the average number exempted by each special cause from military service, in order to get the average number exempt in any way, since many are exempt in two or more ways at once.

This rule applies directly to probabilities. Given the probability that two different and mutually exclusive events will happen under the same supposed set of circumstances. Given, for instance, the probability that if A then B, and also the probability that if A then C, then the sum of these two probabilities is the probability that if A then either B or C, so long as there is no event which belongs at once to the two classes B and C.

**RULE III. Multiplication of Relative Numbers.**—Suppose that we have given the relative number of  $x$ 's per  $y$ ; also the relative number of  $z$ 's per  $x$  of  $y$ ; or, to take a concrete example, suppose that we have given, first, the average number of children in families living in New York; and, second, the average number of teeth in the head of a New York child—then the product of these two numbers would give the average number of children's teeth in a New York family. But this mode of reckoning will only apply in general under two restrictions. In the first place, it would not be true if the same child could belong to different families, for in that case those children who belonged to several different families might have an exceptionally large or small number of teeth, which would affect the average number of children's teeth in a family more than it would affect the average number of teeth in a child's head. In the second place, the rule would not be true if different children could share the same teeth, the average number of children's teeth being in that case evidently something different from the average number of teeth belonging to a child.

In order to apply this rule to probabilities, we must proceed as follows: Suppose that we have given the probability that the conclusion B follows from the premise A, B and A representing as usual certain classes of propositions. Suppose that we also knew the proba-

bility of an inference in which B should be the premise, and a proposition of a third kind, C, the conclusion. Here, then, we have the materials for the application of this rule. We have, first, the relative number of B's per A. We next should have the relative number of C's per B following from A. But the classes of propositions being so selected that the probability of C following from any B in general is just the same as the probability of C's following from one of those B's which is deducible from an A, the two probabilities may be multiplied together, in order to give the probability of C following from A. The same restrictions exist as before. It might happen that the probability that B follows from A was affected by certain propositions of the class B following from several different propositions of the class A. But, practically speaking, all these restrictions are of very little consequence, and it is usually recognized as a principle universally true that the probability that, if A is true, B is, multiplied by the probability that, if B is true, C is, gives the probability that, if A is true, C is.

There is a rule supplementary to this, of which great use is made. It is not universally valid, and the greatest caution has to be exercised in making use of it—a double care, first, never to use it when it will involve serious error; and, second, never to fail to take advantage of it in cases in which it can be employed. This rule depends upon the fact that in very many cases the probability that C is true if B is, is substantially the same as the probability that C is true if A is. Suppose, for example, we have the average number of males among the children born in New York; suppose that we also have the average number of children born in the winter months among those born in New York. Now, we may assume without doubt, at least as a closely approximate proposition (and no very nice calculation would be in place in regard to probabilities), that the proportion of males among all the children born in New York is the same as the proportion of males born in summer in New York, and, therefore, if the names of all the children born during a year were put into an urn, we might multiply the probability that any name drawn would be the name of a male child by the probability that it would be the name of a child born in summer, in order to obtain the probability that it would be the name of a male child born in summer. The questions of probability, in the treatises upon the subject, have usually been such as relate to balls drawn from urns, and games of cards, and so on, in which the question of the *independence* of events, as it is called—that is to say,

the question of whether the probability of C, under the hypothesis B, is the same as its probability under the hypothesis A, has been very simple; but, in the application of probabilities to the ordinary questions of life, it is often an exceedingly nice question whether two events may be considered as independent with sufficient accuracy or not. In all calculations about cards it is assumed that the cards are thoroughly shuffled, which makes one deal quite independent of another. In point of fact the cards seldom are, in practice, shuffled sufficiently to make this true; thus, in a game of whist, in which the cards have fallen in suits of four of the same suit, and are so gathered up, they will lie more or less in sets of four of the same suit, and this will be true even after they are shuffled. At least some traces of this arrangement will remain, in consequence of which the number of "short suits," as they are called—that is to say, the number of hands in which the cards are very unequally divided in regard to suits—is smaller than the calculation would make it to be; so that, when there is a misdeal, where the cards, being thrown about the table, get very thoroughly shuffled, it is a common saying that in the hands next dealt out there are generally short suits. A few years ago a friend of mine, who plays whist a great deal, was so good as to count the number of spades dealt to him in 165 hands, in which the cards had been, if anything, shuffled better than usual. According to calculation, there should have been 85 of these hands in which my friend held either three or four spades, but in point of fact there were 94, showing the influence of imperfect shuffling.

According to the view here taken, these are the only fundamental rules for the calculation of chances. An additional one, derived from a different conception of probability, is given in some treatises, which if it be sound might be made the basis of a theory of reasoning. Being, as I believe it is, absolutely absurd, the consideration of it serves to bring us to the true theory; and it is for the sake of this discussion, which must be postponed to the next number, that I have brought the doctrine of chances to the reader's attention at this early stage of our studies of the logic of science.

## *The Probability of Induction*

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### I

We have found that every argument derives its force from the general truth of the class of inferences to which it belongs; and that probability is the proportion of arguments carrying truth with them among those of any *genus*. This is most conveniently expressed in the nomenclature of the mediaeval logicians. They called the fact expressed by a premise an *antecedent*, and that which follows from it its *consequent*; while the leading principle, that every (or almost every) such antecedent is followed by such a consequent, they termed the *consequence*. Using this language, we may say that probability belongs exclusively to *consequences*, and the probability of any consequence is the number of times in which antecedent and consequent both occur divided by the number of all the times in which the antecedent occurs. From this definition are deduced the following rules for the addition and multiplication of probabilities:

*Rule for the Addition of Probabilities.*—Given the separate probabilities of two consequences having the same antecedent and incompatible consequents. Then the sum of these two numbers is the probability of the consequence, that from the same antecedent one or other of those consequents follows.

*Rule for the Multiplication of Probabilities.*—Given the separate probabilities of the two consequences, "If A then B," and "If both A and B, then C." Then the product of these two numbers is the probability of the consequence, "If A, then both B and C."

*Special Rule for the Multiplication of Independent Probabilities.*—Given the separate probabilities of two consequences having the same antecedents, "If A, then B," and "If A, then C." Suppose that these consequences are such that the probability of the second is

equal to the probability of the consequence, "If both A and B, then C." Then the product of the two given numbers is equal to the probability of the consequence, "If A, then both B and C."

To show the working of these rules we may examine the probabilities in regard to throwing dice. What is the probability of throwing a six with one die? The antecedent here is the event of throwing a die; the consequent, its turning up a six. As the die has six sides, all of which are turned up with equal frequency, the probability of turning up any one is  $\frac{1}{6}$ . Suppose two dice are thrown, what is the probability of throwing sixes? The probability of either coming up six is obviously the same when both are thrown as when one is thrown—namely,  $\frac{1}{6}$ . The probability that either will come up six when the other does is also the same as that of its coming up six whether the other does or not. The probabilities are, therefore, independent; and, by our rule, the probability that both events will happen together is the product of their several probabilities, or  $\frac{1}{6} \times \frac{1}{6}$ . What is the probability of throwing deuce-ace? The probability that the first die will turn up ace and the second deuce is the same as the probability that both will turn up sixes—namely,  $\frac{1}{36}$ ; the probability that the *second* will turn up ace and the *first* deuce is likewise  $\frac{1}{36}$ ; these two events—first, ace; second, deuce; and, second, ace; first, deuce—are incompatible. Hence the rule for addition holds, and the probability that either will come up ace and the other deuce is  $\frac{1}{36} + \frac{1}{36}$ , or  $\frac{1}{18}$ .

In this way all problems about dice, etc., may be solved. When the number of dice thrown is supposed very large, mathematics (which may be defined as the art of making groups to facilitate numeration) comes to our aid with certain devices to reduce the difficulties.

## II

The conception of probability as a matter of *fact*, i.e., as the proportion of times in which an occurrence of one kind is accompanied by an occurrence of another kind, is termed by Mr. Venn the materialistic view of the subject. But probability has often been regarded as being simply the degree of belief which ought to attach to a proposition; and this mode of explaining the idea is termed by Venn the conceptualistic view. Most writers have mixed the two conceptions together. They, first, define the probability of an event as the reason we have to believe that it has taken place, which is conceptualistic; but shortly after they state that it is the ratio of the number

of cases favorable to the event to the total number of cases favorable or contrary, and all equally possible. Except that this introduces the thoroughly unclear idea of cases equally possible in place of cases equally frequent, this is a tolerable statement of the materialistic view. The pure conceptualistic theory has been best expounded by Mr. De Morgan in his *Formal Logic: or, the Calculus of Inference, Necessary and Probable*.

The great difference between the two analyses is, that the conceptualists refer probability to an event, while the materialists make it the ratio of frequency of events of a *species* to those of a *genus* over that *species*, thus *giving it two terms instead of one*. The opposition may be made to appear as follows:

Suppose that we have two rules of inference, such that, of all the questions to the solution of which both can be applied, the first yields correct answers to  $\frac{81}{100}$ , and incorrect answers to the remaining  $\frac{19}{100}$ ; while the second yields correct answers to  $\frac{93}{100}$ , and incorrect answers to the remaining  $\frac{7}{100}$ . Suppose, further, that the two rules are entirely independent as to their truth, so that the second answers correctly  $\frac{93}{100}$  of the questions which the first answers correctly, and also  $\frac{93}{100}$  of the questions which the first answers incorrectly, and answers incorrectly the remaining  $\frac{7}{100}$  of the questions which the first answers correctly, and also the remaining  $\frac{7}{100}$  of the questions which the first answers incorrectly. Then, of all the questions to the solution of which both rules can be applied—

both answer correctly . . . . .	$\frac{93}{100}$ of $\frac{81}{100}$ , or $\frac{93 \times 81}{100 \times 100}$ ;
the second answers correctly and the first incorrectly . . . . .	$\frac{93}{100}$ of $\frac{19}{100}$ , or $\frac{93 \times 19}{100 \times 100}$ ;
the second answers incorrectly and the first correctly . . . . .	$\frac{7}{100}$ of $\frac{81}{100}$ , or $\frac{7 \times 81}{100 \times 100}$ ;
and both answer incorrectly . . . . .	$\frac{7}{100}$ of $\frac{19}{100}$ , or $\frac{7 \times 19}{100 \times 100}$ .

Suppose, now, that, in reference to any question, both give the same answer. Then (the questions being always such as are to be answered by *yes* or *no*), those in reference to which their answers agree are the same as those which both answer correctly together with those which both answer falsely, or  $\frac{93 \times 81}{100 \times 100} + \frac{7 \times 19}{100 \times 100}$  of all. The proportion of those which both answer correctly out of those their answers to which agree is, therefore—

$$\frac{\frac{93 \times 81}{100 \times 100}}{\frac{93 \times 81}{100 \times 100} + \frac{7 \times 19}{100 \times 100}} \quad \text{or} \quad \frac{93 \times 81}{(93 \times 81) + (7 \times 19)}.$$

This is, therefore, the probability that, if both modes of inference yield the same result, that result is correct. We may here conveniently make use of another mode of expression. *Probability* is the ratio of the favorable cases to all the cases. Instead of expressing our result in terms of this ratio, we may make use of another—the ratio of favorable to unfavorable cases. This last ratio may be called the *chance* of an event. Then the chance of a true answer by the first mode of inference is  $\frac{81}{19}$  and by the second is  $\frac{93}{7}$ ; and the chance of a correct answer from both, when they agree, is—

$$\frac{81 \times 93}{19 \times 7} \quad \text{or} \quad \frac{81}{19} \times \frac{93}{7},$$

or the product of the chances of each singly yielding a true answer.

It will be seen that a chance is a quantity which may have any magnitude, however great. An event in whose favor there is an even chance, or  $\frac{1}{2}$ , has a probability of  $\frac{1}{2}$ . An argument having an even chance can do nothing toward reënforcing others, since according to the rule its combination with another would only multiply the chance of the latter by 1.

Probability and chance undoubtedly belong primarily to consequences, and are relative to premises; but we may, nevertheless, speak of the chance of an event absolutely, meaning by that the chance of the combination of all arguments in reference to it which exist for us in the given state of our knowledge. Taken in this sense it is incontestable that the chance of an event has an intimate connection with the degree of our belief in it. Belief is certainly something more than a mere feeling; yet there is a feeling of believing, and this feeling does and ought to vary with the chance of the thing believed, as deduced from all the arguments. Any quantity which varies with the chance might, therefore, it would seem, serve as a thermometer for the proper intensity of belief. Among all such quantities there is one which is peculiarly appropriate. When there is a very great chance, the feeling of belief ought to be very intense. Absolute certainty, or an infinite chance, can never be attained by mortals, and

this may be represented appropriately by an infinite belief. As the chance diminishes the feeling of believing should diminish, until an even chance is reached, where it should completely vanish and not incline either toward or away from the proposition. When the chance becomes less, then a contrary belief should spring up and should increase in intensity as the chance diminishes, and as the chance almost vanishes (which it can never quite do) the contrary belief should tend toward an infinite intensity. Now, there is one quantity which, more simply than any other, fulfills these conditions; it is the *logarithm* of the chance. But there is another consideration which must, if admitted, fix us to this choice for our thermometer. It is that our belief ought to be proportional to the weight of evidence, in this sense, that two arguments which are entirely independent, neither weakening nor strengthening each other, ought, when they concur, to produce a belief equal to the sum of the intensities of belief which either would produce separately. Now, we have seen that the chances of independent concurrent arguments are to be multiplied together to get the chance of their combination, and therefore the quantities which best express the intensities of belief should be such that they are to be *added* when the *chances* are multiplied in order to produce the quantity which corresponds to the combined chance. Now, the logarithm is the only quantity which fulfills this condition. There is a general law of sensibility, called Fechner's psycho-physical law. It is that the intensity of any sensation is proportional to the logarithm of the external force which produces it. It is entirely in harmony with this law that the feeling of belief should be as the logarithm of the chance, this latter being the expression of the state of facts which produces the belief.

The rule for the combination of independent concurrent arguments takes a very simple form when expressed in terms of the intensity of belief, measured in the proposed way. It is this: Take the sum of all the feelings of belief which would be produced separately by all the arguments *pro*, subtract from that the similar sum for arguments *con*, and the remainder is the feeling of belief which we ought to have on the whole. This is a proceeding which men often resort to, under the name of *balancing reasons*.

These considerations constitute an argument in favor of the conceptualistic view. The kernel of it is that the conjoint probability of all the arguments in our possession, with reference to any fact, must be intimately connected with the just degree of our belief in that fact; and this point is supplemented by various others showing the

consistency of the theory with itself and with the rest of our knowledge.

But probability, to have any value at all, must express a fact. It is, therefore, a thing to be inferred upon evidence. Let us, then, consider for a moment the formation of a belief of probability. Suppose we have a large bag of beans from which one has been secretly taken at random and hidden under a thimble. We are now to form a probable judgment of the color of that bean, by drawing others singly from the bag and looking at them, each one to be thrown back, and the whole well mixed up after each drawing. Suppose the first drawing is white and the next black. We conclude that there is not an immense preponderance of either color, and that there is something like an even chance that the bean under the thimble is black. But this judgment may be altered by the next few drawings. When we have drawn ten times, if 4, 5, or 6, are white, we have more confidence that the chance is even. When we have drawn a thousand times, if about half have been white, we have great confidence in this result. We now feel pretty sure that, if we were to make a large number of bets upon the color of single beans drawn from the bag, we could approximately insure ourselves in the long run by betting each time upon the white, a confidence which would be entirely wanting if, instead of sampling the bag by 1,000 drawings, we had done so by only two. Now, as the whole utility of probability is to insure us in the long run, and as that assurance depends, not merely on the value of the chance, but also on the accuracy of the evaluation, it follows that we ought not to have the same feeling of belief in reference to all events of which the chance is even. In short, to express the proper state of our belief, not *one* number but *two* are requisite, the first depending on the inferred probability, the second on the amount of knowledge on which that probability is based.<sup>1</sup> It is true that when our knowledge is very precise, when we have made many drawings from the bag, or, as in most of the examples in the books, when the total contents of the bag are absolutely known, the number which expresses the uncertainty of the assumed probability and its liability to be changed by further experience may become insignificant, or utterly vanish. But, when our knowledge is very slight, this number may be even more important than the probability itself; and when we have no knowledge at all this completely overwhelms the other,

1. Strictly we should need an infinite series of numbers each depending on the probable error of the last.

so that there is no sense in saying that the chance of the totally unknown event is even (for what expresses absolutely no fact has absolutely no meaning), and what ought to be said is that the chance is entirely indefinite. We thus perceive that the conceptualistic view, though answering well enough in some cases, is quite inadequate.

Suppose that the first bean which we drew from our bag were black. That would constitute an argument, no matter how slender, that the bean under the thimble was also black. If the second bean were also to turn out black, that would be a second independent argument reënforcing the first. If the whole of the first twenty beans drawn should prove black, our confidence that the hidden bean was black would justly attain considerable strength. But suppose the twenty-first bean were to be white and that we were to go on drawing until we found that we had drawn 1,010 black beans and 990 white ones. We should conclude that our first twenty beans being black was simply an extraordinary accident, and that in fact the proportion of white beans to black was sensibly equal, and that it was an even chance that the hidden bean was black. Yet according to the rule of *balancing reasons*, since all the drawings of black beans are so many independent arguments in favor of the one under the thimble being black, and all the white drawings so many against it, an excess of twenty black beans ought to produce the same degree of belief that the hidden bean was black, whatever the total number drawn.

In the conceptualistic view of probability, complete ignorance, where the judgment ought not to swerve either toward or away from the hypothesis, is represented by the probability  $\frac{1}{2}$ .<sup>2</sup>

But let us suppose that we are totally ignorant what colored hair the inhabitants of Saturn have. Let us, then, take a color-chart in which all possible colors are shown shading into one another by imperceptible degrees. In such a chart the relative areas occupied by different classes of colors are perfectly arbitrary. Let us inclose such an area with a closed line, and ask what is the chance on conceptualistic principles that the color of the hair of the inhabitants of Saturn falls within that area? The answer cannot be indeterminate because we must be in some state of belief; and, indeed, conceptualistic writers do not admit indeterminate probabilities. As there is no certainty in the matter, the answer lies between *zero* and *unity*. As no

2. "Perfect indecision, belief inclining neither way, an even chance."  
—DE MORGAN, p. 182.

numerical value is afforded by the data, the number must be determined by the nature of the scale of probability itself, and not by calculation from the data. The answer can, therefore, only be one-half, since the judgment should neither favor nor oppose the hypothesis. What is true of this area is true of any other one; and it will equally be true of a third area which embraces the other two. But the probability for each of the smaller areas being one-half, that for the larger should be at least unity, which is absurd.

### III

All our reasonings are of two kinds: 1. *Explicative, analytic, or deductive*; 2. *Ampliative, synthetic, or (loosely speaking) inductive*. In explicative reasoning, certain facts are first laid down in the premises. These facts are, in every case, an inexhaustible multitude, but they may often be summed up in one simple proposition by means of some regularity which runs through them all. Thus, take the proposition that Socrates was a man; this implies (to go no further) that during every fraction of a second of his whole life (or, if you please, during the greater part of them) he was a man. He did not at one instant appear as a tree and at another as a dog; he did not flow into water, or appear in two places at once; you could not put your finger through him as if he were an optical image, etc. Now, the facts being thus laid down, some order among some of them, not particularly made use of for the purpose of stating them, may perhaps be discovered; and this will enable us to throw part or all of them into a new statement, the possibility of which might have escaped attention. Such a statement will be the conclusion of an analytic inference. Of this sort are all mathematical demonstrations. But synthetic reasoning is of another kind. In this case the facts summed up in the conclusion are not among those stated in the premises. They are different facts, as when one sees that the tide rises  $m$  times and concludes that it will rise the next time. These are the only inferences which increase our real knowledge, however useful the others may be.

In any problem in probabilities, we have given the relative frequency of certain events, and we perceive that in these facts the relative frequency of another event is given in a hidden way. This being stated makes the solution. This is therefore mere explicative reasoning, and is evidently entirely inadequate to the representation of synthetic reasoning, which goes out beyond the facts given in the

premises. There is, therefore, a manifest impossibility in so tracing out any probability for a synthetic conclusion.

Most treatises on probability contain a very different doctrine. They state, for example, that if one of the ancient denizens of the shores of the Mediterranean, who had never heard of tides, had gone to the bay of Biscay, and had there seen the tide rise, say  $m$  times, he could know that there was a probability equal to

$$\frac{m + 1}{m + 2}$$

that it would rise the next time. In a well-known work by Quetelet, much stress is laid on this, and it is made the foundation of a theory of inductive reasoning.

But this solution betrays its origin if we apply it to the case in which the man has never seen the tide rise at all; that is, if we put  $m = 0$ . In this case, the probability that it will rise the next time comes out  $\frac{1}{2}$ , or, in other words, the solution involves the conceptualistic principle that there is an even chance of a totally unknown event. The manner in which it has been reached has been by considering a number of urns all containing the same number of balls, part white and part black. One urn contains all white balls, another, one black and the rest white, a third, two black and the rest white, and so on, one urn for each proportion, until an urn is reached containing only black balls. But the only possible reason for drawing any analogy between such an arrangement and that of Nature is the principle that alternatives of which we know nothing must be considered as equally probable. But this principle, is absurd. There is an indefinite variety of ways of enumerating the different possibilities, which, on the application of this principle, would give different results. If there be any way of enumerating the possibilities so as to make them all equal, it is not that from which this solution is derived, but is the following: Suppose we had an immense granary filled with black and white balls well mixed up; and suppose each urn were filled by taking a fixed number of balls from this granary quite at random. The relative number of white balls in the granary might be anything, say one in three. Then in one-third of the urns the first ball would be white, and in two-thirds black. In one-third of those urns of which the first ball was white, and also in one-third of those in which the first ball was black, the second ball would be white. In this way, we should have a distribution like that shown in the following table, where  $w$

stands for a white ball and *b* for a black one. The reader can, if he chooses, verify the table for himself.

wwww.

wwwb.	wwbw.	wbww.	bwww.
wwwb.	wwbw.	wbww.	bwww.

wwbb.	wbwb.	bwwb.	wbbb.	bwbw.	bbww.
wwbb.	wbwb.	bwwb.	wbbb.	bwbw.	bbww.
wwbb.	wbwb.	bwwb.	wbbb.	bwbw.	bbww.
wwbb.	wbwb.	bwwb.	wbbb.	bwbw.	bbww.

wbbb.	bwbb.	bbwb.	bbbw.
wbbb.	bwbb.	bbwb.	bbbw.
wbbb.	bwbb.	bbwb.	bbbw.
wbbb.	bwbb.	bbwb.	bbbw.
wbbb.	bwbb.	bbwb.	bbbw.
wbbb.	bwbb.	bbwb.	bbbw.
wbbb.	bwbb.	bbwb.	bbbw.
wbbb.	bwbb.	bbwb.	bbbw.

bbbb. In the second group, where there is one *b*, there are two sets just alike; in the third there are 4, in the fourth 8, and in the fifth 16, doubling every time. This is because we have supposed twice as many black balls in the granary as white ones; had we supposed 10 times as many, instead of

bbbb.

bbbb. 1, 2, 4, 8, 16

bbbb.

bbbb. sets we should have had

bbbb.

bbbb. 1, 10, 100, 1000, 10000

bbbb.

bbbb. sets; on the other hand, had the numbers of black and  
bbbb. white balls in the granary been even, there would have  
bbbb. been but one set in each group. Now suppose two balls  
were drawn from one of these urns and were found to be both white,  
what would be the probability of the next one being white? If the two  
drawn out were the first two put into the urns, and the next to be

drawn out were the third put in, then the probability of this third being white would be the same whatever the colors of the first two, for it has been supposed that just the same proportion of urns has the third ball white among those which have the first two *white-white*, *white-black*, *black-white*, and *black-black*. Thus, in this case, the chance of the third ball being white would be the same whatever the first two were. But, by inspecting the table, the reader can see that in each group all orders of the balls occur with equal frequency, so that it makes no difference whether they are drawn out in the order they were put in or not. Hence the colors of the balls already drawn have no influence on the probability of any other being white or black.

Now, if there be any way of enumerating the possibilities of Nature so as to make them equally probable, it is clearly one which should make one arrangement or combination of the elements of Nature as probable as another, that is, a distribution like that we have supposed, and it, therefore, appears that the assumption that any such thing can be done, leads simply to the conclusion that reasoning from past to future experience is absolutely worthless. In fact, the moment that you assume that the chances in favor of that of which we are totally ignorant are even, the problem about the tides does not differ, in any arithmetical particular, from the case in which a penny (known to be equally likely to come up heads and tails) should turn up heads  $m$  times successively. In short, it would be to assume that Nature is a pure chaos, or chance combination of independent elements, in which reasoning from one fact to another would be impossible; and since, as we shall hereafter see, there is no judgment of pure observation without reasoning, it would be to suppose all human cognition illusory and no real knowledge possible. It would be to suppose that if we have found the order of Nature more or less regular in the past, this has been by a pure run of luck which we may expect is now at an end. Now, it may be we have no scintilla of proof to the contrary, but reason is unnecessary in reference to that belief which is of all the most settled, which nobody doubts or can doubt, and which he who should deny would stultify himself in so doing.

The relative probability of this or that arrangement of Nature is something which we should have a right to talk about if universes were as plenty as blackberries, if we could put a quantity of them in a bag, shake them well up, draw out a sample, and examine them to see what proportion of them had one arrangement and what propor-

tion another. But, even in that case, a higher universe would contain us, in regard to whose arrangements the conception of probability could have no applicability.

## IV

We have examined the problem proposed by the conceptualists, which, translated into clear language, is this: Given a synthetic conclusion; required to know out of all possible states of things how many will accord, to any assigned extent, with this conclusion; and we have found that it is only an absurd attempt to reduce synthetic to analytic reason, and that no definite solution is possible.

But there is another problem in connection with this subject. It is this: Given a certain state of things, required to know what proportion of all synthetic inferences relating to it will be true within a given degree of approximation. Now, there is no difficulty about this problem (except for its mathematical complication); it has been much studied, and the answer is perfectly well known. And is not this, after all, what we want to know much rather than the other? Why should we want to know the probability that the fact will accord with our conclusion? That implies that we are interested in all possible worlds, and not merely the one in which we find ourselves placed. Why is it not much more to the purpose to know the probability that our conclusion will accord with the fact? One of these questions is the first above stated and the other the second, and I ask the reader whether, if people, instead of using the word probability without any clear apprehension of their own meaning, had always spoken of relative frequency, they could have failed to see that what they wanted was not to follow along the synthetic procedure with an analytic one, in order to find the probability of the conclusion; but, on the contrary, to begin with the fact at which the synthetic inference aims, and follow back to the facts it uses for premises in order to see the probability of their being such as will yield the truth.

As we cannot have an urn with an infinite number of balls to represent the inexhaustibleness of Nature, let us suppose one with a finite number, each ball being thrown back into the urn after being drawn out, so that there is no exhaustion of them. Suppose one ball out of three is white and the rest black, and that four balls are drawn. Then the table on page 299 represents the relative frequency of the different ways in which these balls might be drawn. It will be seen

that if we should judge by these four balls of the proportion in the urn, 32 times out of 81 we should find it  $\frac{1}{4}$ , and 24 times out of 81 we should find it  $\frac{1}{2}$ , the truth being  $\frac{1}{3}$ . To extend this table to high numbers would be great labor, but the mathematicians have found some ingenious ways of reckoning what the numbers would be. It is found that, if the true proportion of white balls is  $p$ , and  $s$  balls are drawn, then the error of the proportion obtained by the induction will be—

half the time within	$0.477\sqrt{\frac{2p(1-p)}{s}}$
9 times out of 10 within	$1.163\sqrt{\frac{2p(1-p)}{s}}$
99 times out of 100 within	$1.821\sqrt{\frac{2p(1-p)}{s}}$
999 times out of 1,000 within	$2.328\sqrt{\frac{2p(1-p)}{s}}$
9,999 times out of 10,000 within	$2.751\sqrt{\frac{2p(1-p)}{s}}$
9,999,999,999 times out of 10,000,000,000 within	$4.77\sqrt{\frac{2p(1-p)}{s}}$

The use of this may be illustrated by an example. By the census of 1870, it appears that the proportion of males among native white children under one year old was 0.5082, while among colored children of the same age the proportion was only 0.4977. The difference between these is 0.0105, or about one in a 100. Can this be attributed to chance, or would the difference always exist among a great number of white and colored children under like circumstances? Here  $p$  may be taken at  $\frac{1}{2}$ ; hence  $2p(1-p)$  is also  $\frac{1}{2}$ . The number of white children counted was near 1,000,000; hence the fraction whose square-root is to be taken is about  $\frac{1}{2000000}$ . The root is about  $\frac{1}{400}$ , and this multiplied by 0.477 gives about 0.0003 as the probable error in the ratio of males among the whites as obtained from the induction. The number of black children was about 150,000, which gives 0.0008 for the probable error. We see that the actual discrepancy is ten times the sum of these, and such a result would happen, according to our table, only once out of 10,000,000,000 censuses, in the long run.

It may be remarked that when the real value of the probability

sought inductively is either very large or very small, the reasoning is more secure. Thus, suppose there were in reality one white ball in 100 in a certain urn, and we were to judge of the number by 100 drawings. The probability of drawing no white ball would be  $\frac{366}{1000}$ ; that of drawing one white ball would be  $\frac{370}{1000}$ ; that of drawing two would be  $\frac{185}{1000}$ ; that of drawing three would be  $\frac{61}{1000}$ ; that of drawing four would be  $\frac{15}{1000}$ ; that of drawing five would be only  $\frac{3}{1000}$ , etc. Thus we should be tolerably certain of not being in error by more than one ball in 100.

It appears, then, that in one sense we can, and in another we cannot, determine the probability of synthetic inference. When I reason in this way:

Ninety-nine Cretans in a hundred are liars;

But Epimenides is a Cretan;

Therefore, Epimenides is a liar:—

I know that reasoning similar to that would carry truth 99 times in 100. But when I reason in the opposite direction:

Minos, Sarpedon, Rhadamanthus, Deucalion, and Epimenides, are all the Cretans I can think of;

But these were all atrocious liars,

Therefore, pretty much all Cretans must have been liars; I do not in the least know how often such reasoning would carry me right. On the other hand, what I do know is that some definite proportion of Cretans must have been liars, and that this proportion can be probably approximated to by an induction from five or six instances. Even in the worst case for the probability of such an inference, that in which about half the Cretans are liars, the ratio so obtained would probably not be in error by more than  $\frac{1}{6}$ . So much I know; but, then, in the present case the inference is that pretty much all Cretans are liars, and whether there may not be a special improbability in that I do not know.

## V

Late in the last century, Immanuel Kant asked the question, "How are synthetical judgments *a priori* possible?" By synthetical judgments he meant such as assert positive fact and are not mere affairs of arrangement; in short, judgments of the kind which synthetical reasoning produces, and which analytic reasoning cannot yield. By *a priori* judgments he meant such as that all outward objects are in space, every event has a cause, etc., propositions which

according to him can never be inferred from experience. Not so much by his answer to this question as by the mere asking of it, the current philosophy of that time was shattered and destroyed, and a new epoch in its history was begun. But before asking *that* question he ought to have asked the more general one, "How are any synthetical judgments at all possible?" How is it that a man can observe one fact and straightway pronounce judgment concerning another different fact not involved in the first? Such reasoning, as we have seen, has, at least in the usual sense of the phrase, no definite probability; how, then, can it add to our knowledge? This is a strange paradox; the Abbé Gratry says it is a miracle, and that every true induction is an immediate inspiration from on high.<sup>3</sup> I respect this explanation far more than many a pedantic attempt to solve the question by some juggling with probabilities, with the forms of syllogism, or what not. I respect it because it shows an appreciation of the depth of the problem, because it assigns an adequate cause, and because it is intimately connected—as the true account should be—with a general philosophy of the universe. At the same time, I do not accept this explanation, because an explanation should tell *how* a thing is done, and to assert a perpetual miracle seems to be an abandonment of all hope of doing that, without sufficient justification.

It will be interesting to see how the answer which Kant gave to his question about synthetical judgments *a priori* will appear if extended to the question of synthetical judgments in general. That answer is, that synthetical judgments *a priori* are possible because whatever is universally true is involved in the conditions of experience. Let us apply this to a general synthetical reasoning. I take from a bag a handful of beans; they are all purple, and I infer that all the beans in the bag are purple. How can I do that? Why, upon the principle that whatever is universally true of my experience (which is here the appearance of these different beans) is involved in the condition of experience. The condition of this special experience is that all these beans were taken from that bag. According to Kant's principle, then, whatever is found true of all the beans drawn from the bag must find its explanation in some peculiarity of the contents of the bag. This is a satisfactory statement of the principle of induction.

3. *Logique*. The same is true, according to him, of every performance of a differentiation, but not of integration. He does not tell us whether it is the supernatural assistance which makes the former process so much the easier.

When we draw a deductive or analytic conclusion, our rule of inference is that facts of a certain general character are either invariably or in a certain proportion of cases accompanied by facts of another general character. Then our premise being a fact of the former class, we infer with certainty or with the appropriate degree of probability the existence of a fact of the second class. But the rule for synthetic inference is of a different kind. When we sample a bag of beans we do not in the least assume that the fact of some beans being purple involves the necessity or even the probability of other beans being so. On the contrary, the conceptualistic method of treating probabilities, which really amounts simply to the deductive treatment of them, when rightly carried out leads to the result that a synthetic inference has just an even chance in its favor, or in other words is absolutely worthless. The color of one bean is entirely independent of that of another. But synthetic inference is founded upon a classification of facts, not according to their characters, but according to the manner of obtaining them. Its rule is, that a number of facts obtained in a given way will in general more or less resemble other facts obtained in the same way; or, *experiences whose conditions are the same will have the same general characters.*

In the former case, we know that premises precisely similar in form to those of the given ones will yield true conclusions, just once in a calculable number of times. In the latter case, we only know that premises obtained under circumstances similar to the given ones (though perhaps themselves very different) will yield true conclusions, at least once in a calculable number of times. We may express this by saying that in the case of analytic inference we know the probability of our conclusion (if the premises are true), but in the case of synthetic inferences we only know the degree of trustworthiness of our proceeding. As all knowledge comes from synthetic inference, we must equally infer that all human certainty consists merely in our knowing that the processes by which our knowledge has been derived are such as must generally have led to true conclusions.

Though a synthetic inference cannot by any means be reduced to deduction, yet that the rule of induction will hold good in the long run may be deduced from the principle that reality is only the object of the final opinion to which sufficient investigation would lead. That belief gradually tends to fix itself under the influence of inquiry is, indeed, one of the facts with which logic sets out.

## *The Order of Nature*

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### I

Any proposition whatever concerning the order of Nature must touch more or less upon religion. In our day, belief, even in these matters, depends more and more upon the observation of facts. If a remarkable and universal orderliness be found in the universe, there must be some cause for this regularity, and science has to consider what hypotheses might account for the phenomenon. One way of accounting for it, certainly, would be to suppose that the world is ordered by a superior power. But if there is nothing in the universal subjection of phenomena to laws, nor in the character of those laws themselves (as being benevolent, beautiful, economical, etc.), which goes to prove the existence of a governor of the universe, it is hardly to be anticipated that any other sort of evidence will be found to weigh very much with minds emancipated from the tyranny of tradition.

Nevertheless, it cannot truly be said that even an absolutely negative decision of that question could altogether destroy religion, inasmuch as there are faiths in which, however much they differ from our own, we recognize those essential characters which make them worthy to be called religions, and which, nevertheless, do not postulate an actually existing Deity. That one, for instance, which has had the most numerous and by no means the least intelligent following of any on earth, teaches that the Divinity in his highest perfection is wrapped away from the world in a state of profound and eternal sleep, which really does not differ from non-existence, whether it be called by that name or not. No candid mind who has followed the writings of M. Vacherot can well deny that his religion is as earnest as can be. He worships the Perfect, the Supreme Ideal; but he con-

ceives that the very notion of the Ideal is repugnant to its real existence. In fact, M. Vacherot finds it agreeable to his reason to assert that non-existence is an essential character of the perfect, just as St. Anselm and Descartes found it agreeable to theirs to assert the extreme opposite. I confess that there is one respect in which either of these positions seems to me more congruous with the religious attitude than that of a theology which stands upon evidences; for as soon as the Deity presents himself to either Anselm or Vacherot, and manifests his glorious attributes, whether it be in a vision of the night or day, either of them recognizes his adorable God, and sinks upon his knees at once; whereas the theologian of evidences will first demand that the divine apparition shall identify himself, and only after having scrutinized his credentials and weighed the probabilities of his being found among the totality of existences, will he finally render his circumspect homage, thinking that no characters can be adorable but those which belong to a real thing.

If we could find out any general characteristic of the universe, any mannerism in the ways of Nature, any law everywhere applicable and universally valid, such a discovery would be of such singular assistance to us in all our future reasoning, that it would deserve a place almost at the head of the principles of logic. On the other hand, if it can be shown that there is nothing of the sort to find out, but that every discoverable regularity is of limited range, this again will be of logical importance. What sort of a conception we ought to have of the universe, how to think of the *ensemble* of things, is a fundamental problem in the theory of reasoning.

## II

It is the legitimate endeavor of scientific men now, as it was twenty-three hundred years ago, to account for the formation of the solar system and of the cluster of stars which forms the galaxy, by the fortuitous concourse of atoms. The greatest expounder of this theory, when asked how he could write an immense book on the system of the world without one mention of its author, replied, very logically, "Je n'avais pas besoin de cette hypothèse-là." But, in truth, there is nothing atheistical in the theory, any more than there was in this answer. Matter is supposed to be composed of molecules which obey the laws of mechanics and exert certain attractions upon one another; and it is to these regularities (which there is no attempt to

account for) that general arrangement of the solar system would be due, and not to hazard.

If any one has ever maintained that the universe is a pure throw of the dice, the theologians have abundantly refuted him. "How often," says Archbishop Tillotson, "might a man, after he had jumbled a set of letters in a bag, fling them out upon the ground before they would fall into an exact poem, yea, or so much as make a good discourse in prose! And may not a little book be as easily made by chance as this great volume of the world?" The chance world here shown to be so different from that in which we live would be one in which there were no laws, the characters of different things being entirely independent; so that, should a sample of any kind of objects ever show a prevalent character, it could only be by accident, and no general proposition could ever be established. Whatever further conclusions we may come to in regard to the order of the universe, this much may be regarded as solidly established, that the world is not a mere chance-medley.

But whether the world makes an exact poem or not, is another question. When we look up at the heavens at night, we readily perceive that the stars are not simply splashed on to the celestial vault; but there does not seem to be any precise system in their arrangement either. It will be worth our while, then, to inquire into the degree of orderliness in the universe; and, to begin, let us ask whether the world we live in is any more orderly than a purely chance-world would be.

Any uniformity, or law of Nature, may be stated in the form, "Every A is B"; as, every ray of light is a non-curved line, every body is accelerated toward the earth's centre, etc. This is the same as to say, "There does not exist any A which is not B"; there is no curved ray; there is no body not accelerated toward the earth; so that the uniformity consists in the non-occurrence in Nature of a certain combination of characters (in this case, the combination of being A with being non-B).<sup>1</sup> And, conversely, every case of the non-occurrence of a combination of characters would constitute a uniformity in Nature. Thus, suppose the quality A is never found in combination with the quality C: for example, suppose the quality of idiocy is never

1. For the present purpose, the negative of a character is to be considered as much a character as the positive, for a uniformity may either be affirmative or negative. I do not say that no distinction can be drawn between positive and negative uniformities.

found in combination with that of having a well-developed brain. Then nothing of the sort A is of the sort C, or everything of the sort A is of the sort non-C (or say, every idiot has an ill-developed brain), which, being something universally true of the A's, is a uniformity in the world. Thus we see that, in a world where there were no uniformities, no logically possible combination of characters would be excluded, but every combination would exist in some object. But two objects not identical must differ in some of their characters, though it be only in the character of being in such-and-such a place. Hence, precisely the same combination of characters could not be found in two different objects; and, consequently, in a chance-world every combination involving either the positive or negative of every character would belong to just one thing. Thus, if there were but five simple characters in such a world,<sup>2</sup> we might denote them by A, B, C, D, E, and their negatives by a, b, c, d, e; and then, as there would be  $2^5$  or 32 different combinations of these characters, completely determinate in reference to each of them, that world would have just 32 objects in it, their characters being as in the following table:

TABLE I

ABCDE	AbCDE	aBCDE	abCDE
ABCDe	AbCDe	aBCDe	abCDe
ABCdE	AbCdE	aBCdE	abCdE
ABCde	AbCde	aBCde	abCde
ABcDE	AbcDE	aBcDE	abcDE
ABcDe	AbcDe	aBcDe	abcDe
ABcdE	AbcdE	aBcdE	abcdE
ABcde	Abcde	aBcde	abcde

For example, if the five primary characters were *hard, sweet, fragrant, green, bright*, there would be one object which reunited all these qualities, one which was hard, sweet, fragrant, and green, but not bright; one which was hard, sweet, fragrant, and bright, but not green; one which was hard, sweet, and fragrant, but neither green nor bright; and so on through all the combinations.

This is what a thoroughly chance-world would be like, and cer-

2. There being 5 simple characters, with their negatives, they could be compounded in various ways so as to make 241 characters in all, without counting the characters *existence* and *non-existence*, which make up 243 or  $3^5$ .

tainly nothing could be imagined more systematic. When a quantity of letters are poured out of a bag, the appearance of disorder is due to the circumstance that the phenomena are only partly fortuitous. The laws of space are supposed, in that case, to be rigidly preserved, and there is also a certain amount of regularity in the formation of the letters. The result is that some elements are orderly and some are disorderly, which is precisely what we observe in the actual world. Tillotson, in the passage of which a part has been quoted, goes on to ask, "How long might 20,000 blind men, which should be sent out from the several remote parts of England, wander up and down before they would all meet upon Salisbury Plains, and fall into rank and file in the exact order of an army? And yet this is much more easy to be imagined than how the innumerable blind parts of matter should rendezvous themselves into a world." This is very true, but in the actual world the *blind men* are, as far as we can see, *not* drawn up in any particular order at all. And, in short, while a certain amount of order exists in the world, it would seem that the world is not so orderly as it might be, and, for instance, not so much so as a world of pure chance would be.

But we can never get to the bottom of this question until we take account of a highly-important logical principle<sup>3</sup> which I now proceed to enounce. This principle is that any plurality or lot of objects whatever have some character in common (no matter how insignificant) which is peculiar to them and not shared by anything else. The word "character" here is taken in such a sense as to include negative characters, such as incivility, inequality, etc., as well as their positives, civility, equality, etc. To prove the theorem, I will show what character any two things, A and B, have in common, not shared by anything else. The things, A and B, are each distinguished from all other things by the possession of certain characters which may be named A-ness and B-ness. Corresponding to these positive characters, are the negative characters un-A-ness, which is possessed by everything except A, and un-B-ness, which is possessed by everything except B. These two characters are united in everything except A and B; and this union of the characters un-A-ness and un-B-ness makes a compound character which may be termed A-B-lessness. This is not possessed by either A or B, but it is possessed by everything else. This character, like every other, has its corresponding negative un-A-B-lessness, and this

3. This principle was, I believe, first stated by Mr. De Morgan.

last is the character possessed by both A and B, and by nothing else. It is obvious that what has thus been shown true of two things is, *mutatis mutandis*, true of any number of things. Q.E.D.

In any world whatever, then, there must be a character peculiar to each possible group of objects. If, as a matter of nomenclature, characters peculiar to the same group be regarded as only different aspects of the same character, then we may say that there will be precisely one character for each possible group of objects. Thus, suppose a world to contain five things,  $\alpha, \beta, \gamma, \delta, \epsilon$ . Then it will have a separate character for each of the 31 groups (with *non-existence* making up 32 or  $2^5$ ) shown in the following table:

TABLE II

	$\alpha\beta$	$\alpha\beta\gamma$	$\alpha\beta\gamma\delta$	$\alpha\beta\gamma\delta\epsilon$
$\alpha$	$\alpha\gamma$	$\alpha\beta\delta$	$\alpha\beta\gamma\epsilon$	
$\beta$	$\alpha\delta$	$\alpha\beta\epsilon$	$\alpha\beta\delta\epsilon$	
$\gamma$	$\alpha\epsilon$	$\alpha\gamma\delta$	$\alpha\gamma\delta\epsilon$	
$\delta$	$\beta\gamma$	$\alpha\gamma\epsilon$	$\beta\gamma\delta\epsilon$	
$\epsilon$	$\beta\delta$	$\alpha\delta\epsilon$		
	$\beta\epsilon$	$\beta\gamma\delta$		
	$\gamma\delta$	$\beta\gamma\epsilon$		
	$\gamma\epsilon$	$\beta\delta\epsilon$		
	$\delta\epsilon$	$\gamma\delta\epsilon$		

This shows that a contradiction is involved in the very idea of a chance-world, for in a world of 32 things, instead of there being only  $3^5$  or 243 characters, as we have seen that the notion of a chance-world requires, there would, in fact, be no less than  $2^{32}$ , or 4,294,-967,296 characters, which would not be all independent, but would have all possible relations with one another.

We further see that so long as we regard characters abstractly, without regard to their relative importance, etc., there is no possibility of a more or less degree of orderliness in the world, the whole system of relationship between the different characters being given by mere logic; that is, being implied in those facts which are tacitly admitted as soon as we admit that there is any such thing as reasoning.

In order to descend from this abstract point of view, it is requisite to consider the characters of things as relative to the perceptions and

active powers of living beings. Instead, then, of attempting to imagine a world in which there should be no uniformities, let us suppose one in which none of the uniformities should have reference to characters interesting or important to us. In the first place, there would be nothing to puzzle us in such a world. The small number of qualities which would directly meet the senses would be the ones which would afford the key to everything which could possibly interest us. The whole universe would have such an air of system and perfect regularity that there would be nothing to ask. In the next place, no action of ours, and no event of Nature, would have important consequences in such a world. We should be perfectly free from all responsibility, and there would be nothing to do but to enjoy or suffer whatever happened to come along. Thus there would be nothing to stimulate or develop either the mind or the will, and we consequently should neither act nor think. We should have no memory, because that depends on a law of our organization. Even if we had any senses, we should be situated toward such a world precisely as inanimate objects are toward the present one, provided we suppose that these objects have an absolutely transitory and instantaneous consciousness without memory—a supposition which is a mere mode of speech, for that would be no consciousness at all. We may, therefore, say that a world of chance is simply our actual world viewed from the standpoint of an animal at the very vanishing-point of intelligence. The actual world is almost a chance-medley to the mind of a polyp. The interest which the uniformities of Nature have for an animal measures his place in the scale of intelligence.

Thus, nothing can be made out from the orderliness of Nature in regard to the existence of a God, unless it be maintained that the existence of a finite mind proves the existence of an infinite one.

### III

In the last of these papers we examined the nature of inductive or synthetic reasoning. We found it to be a process of sampling. A number of specimens of a class are taken, not by selection within that class, but at random. These specimens will agree in a great number of respects. If, now, it were likely that a second lot would agree with the first in the majority of these respects, we might base on this consideration an inference in regard to any one of these characters. But such an inference would neither be of the nature of induction,

nor would it (except in special cases) be valid, because the vast majority of points of agreement in the first sample drawn would generally be entirely accidental, as well as insignificant. To illustrate this, I take the ages at death of the first five poets given in Wheeler's *Biographical Dictionary*. They are:

Aagard, 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 divided by three, leaves a remainder of *one*.
3. The sum of the prime factors of each age, including one, is divisible by three.

It is easy to see that the number of accidental agreements of this sort would be quite endless. But suppose that, instead of considering a character because of its prevalence in the sample, we designate a character before taking the sample, selecting it for its importance, obviousness, or other point of interest. Then two considerable samples drawn at random are extremely likely to agree approximately in regard to the proportion of occurrences of a character so chosen. *The inference that a previously designated character has nearly the same frequency of occurrence in the whole of a class that it has in a sample drawn at random out of that class is induction.* If the character be not previously designated, then a sample in which it is found to be prevalent can only serve to suggest that it *may be* prevalent in the whole class. We may consider this surmise as an inference if we please—an inference of possibility; but a second sample must be drawn to test the question of whether the character actually is prevalent. Instead of designating beforehand a single character in reference to which we will examine a sample, we may designate two,

and use the same sample to determine the relative frequencies of both. This will be making two inductive inferences at once; and, of course, we are less certain that both will yield correct conclusions than we should be that either separately would do so. What is true of two characters is true of any limited number. Now, the number of characters which have any considerable interest for us in reference to any class of objects is more moderate than might be supposed. As we shall be sure to examine any sample with reference to these characters, they may be regarded not exactly as predesignated, but as predetermined (which amounts to the same thing); and we may infer that the sample represents the class in all these respects if we please, remembering only that this is not so secure an inference as if the particular quality to be looked for had been fixed upon beforehand.

The demonstration of this theory of induction rests upon principles and follows methods which are accepted by all those who display in other matters the particular knowledge and force of mind which qualify them to judge of this. The theory itself, however, quite unaccountably seems never to have occurred to any of the writers who have undertaken to explain synthetic reasoning. The most widely-spread opinion in the matter is one which was much promoted by Mr. John Stuart Mill—namely, that induction depends for its validity upon the uniformity of Nature—that is, on the principle that what happens once will, under a sufficient degree of similarity of circumstances, happen again as often as the same circumstances recur. The application is this: The fact that different things belong to the same class constitutes the similarity of circumstances, and the induction is good, provided this similarity is “sufficient.” What happens once is, that a number of these things are found to have a certain character; what may be expected, then, to happen again as often as the circumstances recur consists in this, that all things belonging to the same class should have the same character.

This analysis of induction has, I venture to think, various imperfections, to some of which it may be useful to call attention. In the first place, when I put my hand in a bag and draw out a handful of beans, and, finding three-quarters of them black, infer that about three-quarters of all in the bag are black, my inference is obviously of the same kind as if I had found any larger proportion, or the whole, of the sample black, and had assumed that it represented in that respect the rest of the contents of the bag. But the analysis in ques-

tion hardly seems adapted to the explanation of this *proportionate* induction, where the conclusion, instead of being that a certain event uniformly happens under certain circumstances, is precisely that it does not uniformly occur, but only happens in a certain proportion of cases. It is true that the whole sample may be regarded as a single object, and the inference may be brought under the formula proposed by considering the conclusion to be that any similar sample will show a similar proportion among its constituents. But this is to treat the induction as if it rested on a single instance, which gives a very false idea of its probability.

In the second place, if the uniformity of Nature were the sole warrant of induction, we should have no right to draw one in regard to a character whose constancy we knew nothing about. Accordingly, Mr. Mill says that, though none but white swans were known to Europeans for thousands of years, yet the inference that all swans were white was "not a good induction," because it was not known that color was a usual generic character (it, in fact, not being so by any means). But it is mathematically demonstrable that an inductive inference may have as high a degree of probability as you please independent of any antecedent knowledge of the constancy of the character inferred. Before it was known that color is not usually a character of *genera*, there was certainly a considerable probability that all swans were white. But the further study of the *genera* of animals led to the induction of their non-uniformity in regard to color. A deductive application of this general proposition would have gone far to overcome the probability of the universal whiteness of swans before the black species was discovered. When we do know anything in regard to the general constancy or inconstancy of a character, the application of that general knowledge to the particular class to which any induction relates, though it serves to increase or diminish the force of the induction, is, like every application of general knowledge to particular cases, deductive in its nature and not inductive.

In the third place, to say that inductions are true because similar events happen in similar circumstances—or, what is the same thing, because objects similar in some respects are likely to be similar in others—is to overlook those conditions which really are essential to the validity of inductions. When we take all the characters into account, any pair of objects resemble one another in just as many particulars as any other pair. If we limit ourselves to such characters

as have for us any importance, interest, or obviousness, then a synthetic conclusion may be drawn, but only on condition that the specimens by which we judge have been taken at random from the class in regard to which we are to form a judgment, and not selected as belonging to any sub-class. The induction only has its full force when the character concerned has been designated before examining the sample. These are the essentials of induction, and they are not recognized in attributing the validity of induction to the uniformity of Nature. The explanation of induction by the doctrine of probabilities, given in the last of these papers, is not a mere metaphysical formula, but is one from which all the rules of synthetic reasoning can be deduced systematically and with mathematical cogency. But the account of the matter by a principle of Nature, even if it were in other respects satisfactory, presents the fatal disadvantage of leaving us quite as much afloat as before in regard to the proper method of induction. It does not surprise me, therefore, that those who adopt this theory have given erroneous rules for the conduct of reasoning, nor that the greater number of examples put forward by Mr. Mill in his first edition, as models of what inductions should be, proved in the light of further scientific progress so particularly unfortunate that they had to be replaced by others in later editions. One would have supposed that Mr. Mill might have based an induction on *this* circumstance, especially as it is his avowed principle that, if the conclusion of an induction turns out false, it cannot have been a good induction. Nevertheless, neither he nor any of his scholars seem to have been led to suspect, in the least, the perfect solidity of the framework which he devised for securely supporting the mind in its passage from the known to the unknown, although at its first trial it did not answer quite so well as had been expected.

## IV

When we have drawn any statistical induction—such, for instance, as that one-half of all births are of male children—it is always possible to discover, by investigation sufficiently prolonged, a class of which the same predicate may be affirmed universally; to find out, for instance, *what sort of* births are of male children. The truth of this principle follows immediately from the theorem that there is a character peculiar to every possible group of objects. The form in

which the principle is usually stated is, that *every event must have a cause.*

But, though there exists a cause for every event, and that of a kind which is capable of being discovered, yet if there be nothing to guide us to the discovery; if we have to hunt among all the events in the world without any scent; if, for instance, the sex of a child might equally be supposed to depend on the configuration of the planets, on what was going on at the antipodes, or on anything else—then the discovery would have no chance of ever getting made.

That we ever do discover the precise causes of things, that any induction whatever is absolutely without exception, is what we have no right to assume. On the contrary, it is an easy corollary, from the theorem just referred to, that every empirical rule has an exception. But there are certain of our inductions which present an approach to universality so extraordinary that, even if we are to suppose that they are not strictly universal truths, we cannot possibly think that they have been reached merely by accident. The most remarkable laws of this kind are those of *time* and *space*. With reference to space, Bishop Berkeley first showed, in a very conclusive manner, that it was not a thing *seen*, but a thing *inferred*. Berkeley chiefly insists on the impossibility of directly seeing the third dimension of space, since the retina of the eye is a surface. But, in point of fact, the retina is not even a surface; it is a conglomeration of nerve-needles directed toward the light and having only their extreme points sensitive, these points lying at considerable distances from one another compared with their areas. Now, of these points, certainly the excitation of no one singly can produce the perception of a surface, and consequently not the aggregate of all the sensations can amount to this. But certain relations subsist between the excitations of different nerve-points, and these constitute the premises upon which the hypothesis of space is founded, and from which it is inferred. That space is not immediately perceived is now universally admitted; and a mediate cognition is what is called an inference, and is subject to the criticism of logic. But what are we to say to the fact of every chicken as soon as it is hatched solving a problem whose data are of a complexity sufficient to try the greatest mathematical powers? It would be insane to deny that the tendency to light upon the conception of space is inborn in the mind of the chicken and of every animal. The same thing is equally true of time. That time is not directly perceived is evident, since no lapse of time is present, and we only perceive what

is present. That, not having the idea of time, we should ever be able to perceive the flow in our sensations without some particular aptitude for it, will probably also be admitted. The idea of force—at least, in its rudiments—is another conception so early arrived at, and found in animals so low in the scale of intelligence, that it must be supposed innate. But the innateness of an idea admits of degree, for it consists in the tendency of that idea to present itself to the mind. Some ideas, like that of space, do so present themselves irresistibly at the very dawn of intelligence, and take possession of the mind on small provocation, while of other conceptions we are prepossessed, indeed, but not so strongly, down a scale which is greatly extended. The tendency to personify every thing, and to attribute human characters to it, may be said to be innate; but it is a tendency which is very soon overcome by civilized man in regard to the greater part of the objects about him. Take such a conception as that of gravitation varying inversely as the square of the distance. It is a very simple law. But to say that it is simple is merely to say that it is one which the mind is particularly adapted to apprehend with facility. Suppose the idea of a quantity multiplied into another had been no more easy to the mind than that of a quantity raised to the power indicated by itself—should we ever have discovered the law of the solar system?

It seems incontestable, therefore, that the mind of man is strongly adapted to the comprehension of the world; at least, so far as this goes, that certain conceptions, highly important for such a comprehension, naturally arise in his mind; and, without such a tendency, the mind could never have had any development at all.

How are we to explain this adaptation? The great utility and indispensableness of the conceptions of time, space, and force, even to the lowest intelligence, are such as to suggest that they are the results of natural selection. Without something like geometrical, kinetical, and mechanical conceptions, no animal could seize his food or do anything which might be necessary for the preservation of the species. He might, it is true, be provided with an instinct which would generally have the same effect; that is to say, he might have conceptions different from those of time, space, and force, but which coincided with them in regard to the ordinary cases of the animal's experience. But, as that animal would have an immense advantage in the struggle for life whose mechanical conceptions did not break down in a novel situation (such as development must bring about), there would be a constant selection in favor of more and more cor-

rect ideas of these matters. Thus would be attained the knowledge of that fundamental law upon which all science rolls; namely, that forces depend upon relations of time, space, and mass. When this idea was once sufficiently clear, it would require no more than a comprehensible degree of genius to discover the exact nature of these relations. Such an hypothesis naturally suggests itself, but it must be admitted that it does not seem sufficient to account for the extraordinary accuracy with which these conceptions apply to the phenomena of Nature, and it is probable that there is some secret here which remains to be discovered.

## V

Some important questions of logic depend upon whether we are to consider the material universe as of limited extent and finite age, or quite boundless in space and in time. In the former case, it is conceivable that a general plan or design embracing the whole universe should be discovered, and it would be proper to be on the alert for some traces of such a unity. In the latter case, since the proportion of the world of which we can have any experience is less than the smallest assignable fraction, it follows that we never could discover any *pattern* in the universe except a repeating one; any design embracing the whole would be beyond our powers to discern, and beyond the united powers of all intellects during all time. Now, what is absolutely incapable of being known is, as we have seen in a former paper, not real at all. An absolutely incognizable existence is a non-sensical phrase. If, therefore, the universe is infinite, the attempt to find in it any design embracing it as a whole is futile, and involves a false way of looking at the subject. If the universe never had any beginning, and if in space world stretches beyond world without limit, there is no *whole* of material things, and consequently no general character to the universe, and no need or possibility of any governor for it. But if there was a time before which absolutely no matter existed, if there are certain absolute bounds to the region of things outside of which there is a mere void, then we naturally seek for an explanation of it, and, since we cannot look for it among material things, the hypothesis of a great disembodied animal, the creator and governor of the world, is natural enough.

The actual state of the evidence as to the limitation of the universe is as follows: As to time, we find on our earth a constant progress

of development since the planet was a red-hot ball; the solar system seems to have resulted from the condensation of a nebula, and the process appears to be still going on. We sometimes see stars (presumably with systems of worlds) destroyed and apparently resolved back into the nebulous condition, but we have no evidence of any existence of the world previous to the nebulous stage from which it seems to have been evolved. All this rather favors the idea of a beginning than otherwise. As for limits in space, we cannot be sure that we see anything outside of the system of the milky-way. Minds of theological predilections have therefore no need of distorting the facts to reconcile them with their views.

But the only scientific presumption is, that the unknown parts of space and time are like the known parts, occupied; that, as we see cycles of life and death in all development which we can trace out to the end, the same holds good in regard to solar systems; that as enormous distances lie between the different planets of our solar system, relatively to their diameters, and as still more enormous distances lie between our system relatively to its diameter and other systems, so it may be supposed that other galactic clusters exist so remote from ours as not to be recognized as such with certainty. I do not say that these are strong inductions; I only say that they are the presumptions which, in our ignorance of the facts, should be preferred to hypotheses which involve conceptions of things and occurrences totally different in their character from any of which we have had any experience, such as disembodied spirits, the creation of matter, infringements of the laws of mechanics, etc.

The universe ought to be presumed too vast to have any character. When it is claimed that the arrangements of Nature are benevolent, or just, or wise, or of any other peculiar kind, we ought to be prejudiced against such opinions, as being the offspring of an ill-founded notion of the finitude of the world. And examination has hitherto shown that such beneficences, justice, etc., are of a most limited kind—limited in degree and limited in range.

In like manner, if any one claims to have discovered a plan in the structure of organized beings, or a scheme in their classification, or a regular arrangement among natural objects, or a system of proportionality in the human form, or an order of development, or a correspondence between conjunctions of the planets and human events, or a significance in numbers, or a key to dreams, the first thing we have to ask is whether such relations are susceptible of explanation

on mechanical principles, and if not they should be looked upon with disfavor as having already a strong presumption against them; and examination has generally exploded all such theories.

There are minds to whom every prejudice, every presumption, seems unfair. It is easy to say what minds these are. They are those who never have known what it is to draw a well-grounded induction, and who imagine that other people's knowledge is as nebulous as their own. That all science rolls upon presumption (not of a formal but of a real kind) is no argument with them, because they cannot imagine that there is anything solid in human knowledge. These are the people who waste their time and money upon perpetual motions and other such rubbish.

But there are better minds who take up mystical theories (by which I mean all those which have no possibility of being mechanically explained). These are persons who are strongly prejudiced in favor of such theories. We all have natural tendencies to believe in such things; our education often strengthens this tendency; and the result is, that to many minds nothing seems so antecedently probable as a theory of this kind. Such persons find evidence enough in favor of their views, and in the absence of any recognized logic of induction they cannot be driven from their belief.

But to the mind of a physicist there ought to be a strong presumption against every mystical theory; and therefore it seems to me that those scientific men who have sought to make out that science was not hostile to theology have not been so clear-sighted as their opponents.

It would be extravagant to say that science can at present disprove religion; but it does seem to me that the spirit of science is hostile to any religion except such a one as that of M. Vacherot. Our appointed teachers inform us that Buddhism is a miserable and atheistical faith, shorn of the most glorious and needful attributes of a religion; that its priests can be of no use to agriculture by praying for rain, nor to war by commanding the sun to stand still. We also hear the remonstrances of those who warn us that to shake the general belief in the living God would be to shake the general morals, public and private. This, too, must be admitted; such a revolution of thought could no more be accomplished without waste and desolation than a plantation of trees could be transferred to new ground, however wholesome in itself, without all of them languishing for a time, and many of them dying. Nor is it, by-the-way, a thing to be

presumed that a man would have taken part in a movement having a possible atheistical issue without having taken serious and adequate counsel in regard to that responsibility. But, let the consequences of such a belief be as dire as they may, one thing is certain: that the state of the facts, whatever it may be, will surely get found out, and no human prudence can long arrest the triumphal car of truth—no, not if the discovery were such as to drive every individual of our race to suicide!

But it would be folly to suppose that any metaphysical theory in regard to the mode of being of the perfect is to destroy that aspiration toward the perfect which constitutes the essence of religion. It is true that, if the priests of any particular form of religion succeed in making it generally believed that religion cannot exist without the acceptance of certain formulas, or if they succeed in so interweaving certain dogmas with the popular religion that the people can see no essential analogy between a religion which accepts these points of faith and one which rejects them, the result may very well be to render those who cannot believe these things irreligious. Nor can we ever hope that any body of priests should consider themselves more teachers of religion in general than of the particular system of theology advocated by their own party. But no man need be excluded from participation in the common feelings, nor from so much of the public expression of them as is open to all the laity, by the unphilosophical narrowness of those who guard the mysteries of worship. Am I to be prevented from joining in that common joy at the revelation of enlightened principles of religion, which we celebrate at Easter and Christmas, because I think that certain scientific, logical, and metaphysical ideas which have been mixed up with these principles are untenable? No; to do so would be to estimate those errors as of more consequence than the truth—an opinion which few would admit. People who do not believe what are really the fundamental principles of Christianity are rare to find, and all but these few ought to feel at home in the churches.

## *Deduction, Induction, and Hypothesis*

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### I

The chief business of the logician is to classify arguments; for all testing clearly depends on classification. The classes of the logicians are defined by certain typical forms called syllogisms. For example, the syllogism called *Barbara* is as follows:

S is M; M is P;  
Hence, S is P.

Or, to put words for letters—

Enoch and Elijah were men; all men die;  
Hence, Enoch and Elijah must have died.

The “is P” of the logicians stands for any verb, active or neuter. It is capable of strict proof (with which, however, I will not trouble the reader) that all arguments whatever can be put into this form; but only under the condition that the *is* shall mean “*is* for the purposes of the argument” or “*is* represented by.” Thus, an induction will appear in this form something like this:

These beans are two-thirds white;  
But, the beans in this bag are (represented by) these beans;  
∴ The beans in the bag are two-thirds white.

But, because all inference may be reduced in some way to *Barbara*, it does not follow that this is the most appropriate form in

which to represent every kind of inference. On the contrary, to show the distinctive characters of different sorts of inference, they must clearly be exhibited in different forms peculiar to each. *Barbara* particularly typifies deductive reasoning; and so long as the *is* is taken literally, no inductive reasoning can be put into this form. *Barbara* is, in fact, nothing but the application of a rule. The so-called major premise lays down this rule; as, for example, *All men are mortal*. The other or minor premise states a case under the rule; as, *Enoch was a man*. The conclusion applies the rule to the case and states the result: *Enoch is mortal*. All deduction is of this character; it is merely the application of general rules to particular cases. Sometimes this is not very evident, as in the following:

All quadrangles are figures,  
But no triangle is a quadrangle;  
Therefore, some figures are not triangles.

But here the reasoning is really this:

*Rule*.—Every quadrangle is other than a triangle.

*Case*.—Some figures are quadrangles.

*Result*.—Some figures are not triangles.

Inductive or synthetic reasoning, being something more than the mere application of a general rule to a particular case, can never be reduced to this form.

If, from a bag of beans of which we know that  $\frac{2}{3}$  are white, we take one at random, it is a deductive inference that this bean is probably white, the probability being  $\frac{2}{3}$ . We have, in effect, the following syllogism:

*Rule*.—The beans in this bag are  $\frac{2}{3}$  white.

*Case*.—This bean has been drawn in such a way that in the long run the relative number of white beans so drawn would be equal to the relative number in the bag.

*Result*.—This bean has been drawn in such a way that in the long run it would turn out white  $\frac{2}{3}$  of the time.

If instead of drawing one bean we draw a handful at random and conclude that about  $\frac{2}{3}$  of the handful are probably white, the reason-

ing is of the same sort. If, however, not knowing what proportion of white beans there are in the bag, we draw a handful at random and, finding  $\frac{2}{3}$  of the beans in the handful white, conclude that about  $\frac{2}{3}$  of those in the bag are white, we are rowing up the current of deductive sequence, and are concluding a rule from the observation of a result in a certain case. This is particularly clear when all the handful turn out one color. The induction then is:

These beans were in this bag.

These beans are white.

$\therefore$  All the beans in the bag were white.

Which is but an inversion of the deductive syllogism.

*Rule.*—All the beans in the bag were white.

*Case.*—These beans were in the bag.

*Result.*—These beans are white.

So that induction is the inference of the *rule* from the *case* and *result*.

But this is not the only way of inverting a deductive syllogism so as to produce a synthetic inference. Suppose I enter a room and there find a number of bags, containing different kinds of beans. On the table there is a handful of white beans; and, after some searching, I find one of the bags contains white beans only. I at once infer as a probability, or as a fair guess, that this handful was taken out of that bag. This sort of inference is called *making an hypothesis*. It is the inference of a *case* from a *rule* and *result*. We have, then—

### DEDUCTION

*Rule.*—All the beans from this bag are white.

*Case.*—These beans are from this bag.

$\therefore$  *Result.*—These beans are white.

### INDUCTION

*Case.*—These beans are from this bag.

*Result.*—These beans are white.

$\therefore$  *Rule.*—All the beans from this bag are white.

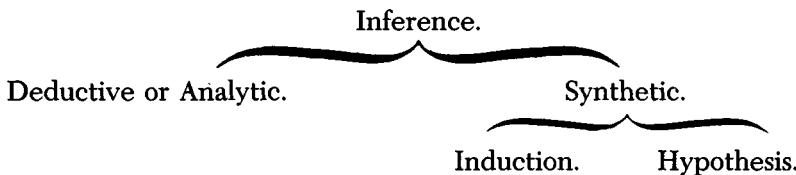
### HYPOTHESIS

*Rule.*—All the beans from this bag are white.

*Result.*—These beans are white.

∴ *Case.*—These beans are from this bag.

We, accordingly, classify all inference as follows:



Induction is where we generalize from a number of cases of which something is true, and infer that the same thing is true of a whole class. Or, where we find a certain thing to be true of a certain proportion of cases and infer that it is true of the same proportion of the whole class. Hypothesis is where we find some very curious circumstance, which would be explained by the supposition that it was a case of a certain general rule, and thereupon adopt that supposition. Or, where we find that in certain respects two objects have a strong resemblance, and infer that they resemble one another strongly in other respects.

I once landed at a seaport in a Turkish province; and, as I was walking up to the house which I was to visit, I met a man upon horseback, surrounded by four horsemen holding a canopy over his head. As the governor of the province was the only personage I could think of who would be so greatly honored, I inferred that this was he. This was an hypothesis.

Fossils are found; say, remains like those of fishes, but far in the interior of the country. To explain the phenomenon, we suppose the sea once washed over this land. This is another hypothesis.

Numberless documents and monuments refer to a conqueror called Napoleon Bonaparte. Though we have not seen the man, yet we cannot explain what we have seen, namely, all these documents and monuments, without supposing that he really existed. Hypothesis again.

As a general rule, hypothesis is a weak kind of argument. It often inclines our judgment so slightly toward its conclusion that we can-

not say that we believe the latter to be true; we only surmise that it may be so. But there is no difference except one of degree between such an inference and that by which we are led to believe that we remember the occurrences of yesterday from our feeling as if we did so.

## II

Besides the way just pointed out of inverting a deductive syllogism to produce an induction or hypothesis, there is another. If from the truth of a certain premise the truth of a certain conclusion would necessarily follow, then from the falsity of the conclusion the falsity of the premise would follow. Thus, take the following syllogism in *Barbara*:

*Rule*.—All men are mortal.

*Case*.—Enoch and Elijah were men.

∴ *Result*.—Enoch and Elijah were mortal.

Now, a person who denies this result may admit the rule, and, in that case, he must deny the case. Thus:

*Denial of Result*.—Enoch and Elijah were not mortal.

*Rule*.—All men are mortal.

∴ *Denial of Case*.—Enoch and Elijah were not men.

This kind of syllogism is called *Baroco*, which is the typical mood of the second figure. On the other hand, the person who denies the result may admit the case, and in that case he must deny the rule. Thus:

*Denial of the Result*.—Enoch and Elijah were not mortal.

*Case*.—Enoch and Elijah were men.

∴ *Denial of the Rule*.—Some men are not mortal.

This kind of syllogism is called *Bocardo*, which is the typical mood of the third figure.

*Baroco* and *Bocardo* are, of course, deductive syllogisms; but of a very peculiar kind. They are called by logicians indirect moods, because they need some transformation to appear as the application

of a rule to a particular case. But if, instead of setting out as we have here done with a necessary deduction in *Barbara*, we take a probable deduction of similar form, the indirect moods which we shall obtain will be—

Corresponding to *Baroco*, an hypothesis;  
and, Corresponding to *Bocardo*, an induction.

For example, let us begin with this probable deduction in *Barbara*:

*Rule*.—Most of the beans in this bag are white.

*Case*.—This handful of beans are from this bag.

∴ *Result*.—Probably, most of this handful of beans are white.

Now, deny the result, but accept the rule:

*Denial of Result*.—Few beans of this handful are white.

*Rule*.—Most beans in this bag are white.

∴ *Denial of Case*.—Probably, these beans were taken from another bag.

This is an hypothetical inference. Next, deny the result, but accept the case:

*Denial of Result*.—Few beans of this handful are white.

*Case*.—These beans came from this bag.

∴ *Denial of Rule*.—Probably, few beans in the bag are white.

This is an induction.

The relation thus exhibited between synthetic and deductive reasoning is not without its importance. When we adopt a certain hypothesis, it is not alone because it will explain the observed facts, but also because the contrary hypothesis would probably lead to results contrary to those observed. So, when we make an induction, it is drawn not only because it explains the distribution of characters in the sample, but also because a different rule would probably have led to the sample being other than it is.

But the advantage of this way of considering the subject might easily be overrated. An induction is really the inference of a rule, and

to consider it as the denial of a rule is an artificial conception, only admissible because, when statistical or proportional propositions are considered as rules, the denial of a rule is itself a rule. So, an hypothesis is really a subsumption of a case under a class and not the denial of it, except for this, that to deny a subsumption under one class is to admit a subsumption under another.

*Bocardo* may be considered as an induction, so timid as to lose its ampliative character entirely. Enoch and Elijah are specimens of a certain kind of men. All that kind of men are shown by these instances to be immortal. But instead of boldly concluding that all very pious men, or all men favorites of the Almighty, etc., are immortal, we refrain from specifying the description of men, and rest in the merely explicative inference that *some* men are immortal. So *Baroco* might be considered as a very timid hypothesis. Enoch and Elijah are not mortal. Now, we might boldly suppose them to be gods or something of that sort, but instead of that we limit ourselves to the inference that they are of *some* nature different from that of man.

But, after all, there is an immense difference between the relation of *Baroco* and *Bocardo* to *Barbara* and that of Induction and Hypothesis to Deduction. *Baroco* and *Bocardo* are based upon the fact that if the truth of a conclusion necessarily follows from the truth of a premise, then the falsity of the premise follows from the falsity of the conclusion. This is always true. It is different when the inference is only probable. It by no means follows that, because the truth of a certain premise would render the truth of a conclusion probable, therefore the falsity of the conclusion renders the falsity of the premise probable. At least, this is only true, as we have seen in a former paper, when the word probable is used in one sense in the antecedent and in another in the consequent.

### III

A certain anonymous writing is upon a torn piece of paper. It is suspected that the author is a certain person. His desk, to which only he has had access, is searched, and in it is found a piece of paper, the torn edge of which exactly fits, in all its irregularities, that of the paper in question. It is a fair hypothetic inference that the suspected man was actually the author. The ground of this inference evidently is that two torn pieces of paper are extremely unlikely to fit together by accident. Therefore, of a great number of inferences of this sort,

but a very small proportion would be deceptive. The analogy of hypothesis with induction is so strong that some logicians have confounded them. Hypothesis has been called an induction of characters. A number of characters belonging to a certain class are found in a certain object; whence it is inferred that all the characters of that class belong to the object in question. This certainly involves the same principle as induction; yet in a modified form. In the first place, characters are not susceptible of simple enumeration like objects; in the next place, characters run in categories. When we make an hypothesis like that about the piece of paper, we only examine a single line of characters, or perhaps two or three, and we take no specimen at all of others. If the hypothesis were nothing but an induction, all that we should be justified in concluding, in the example above, would be that the two pieces of paper which matched in such irregularities as have been examined would be found to match in other, say slighter, irregularities. The inference from the shape of the paper to its ownership is precisely what distinguishes hypothesis from induction, and makes it a bolder and more perilous step.

The same warnings that have been given against imagining that induction rests upon the uniformity of Nature might be repeated in regard to hypothesis. Here, as there, such a theory not only utterly fails to account for the validity of the inference, but it also gives rise to methods of conducting it which are absolutely vicious. There are, no doubt, certain uniformities in Nature, the knowledge of which will fortify an hypothesis very much. For example, we suppose that iron, titanium, and other metals exist in the sun, because we find in the solar spectrum many lines coincident in position with those which these metals would produce; and this hypothesis is greatly strengthened by our knowledge of the remarkable distinctiveness of the particular line of characters observed. But such a fortification of hypothesis is of a deductive kind, and hypothesis may still be probable when such reënforcement is wanting.

There is no greater nor more frequent mistake in practical logic than to suppose that things which resemble one another strongly in some respects are any the more likely for that to be alike in others. That this is absolutely false, admits of rigid demonstration; but, inasmuch as the reasoning is somewhat severe and complicated (requiring, like all such reasoning, the use of A, B, C, etc., to set it forth), the reader would probably find it distasteful, and I omit it. An example, however, may illustrate the proposition: The comparative mythologists occupy themselves with finding points of resemblance between

solar phenomena and the careers of the heroes of all sorts of traditional stories; and upon the basis of such resemblances they infer that these heroes are impersonations of the sun. If there be anything more in their reasonings, it has never been made clear to me. An ingenious logician, to show how futile all that is, wrote a little book, in which he pretended to prove, in the same manner, that Napoleon Bonaparte is only an impersonation of the sun. It was really wonderful to see how many points of resemblance he made out. The truth is, that any two things resemble one another just as strongly as any two others, if recondite resemblances are admitted. But, in order that the process of making an hypothesis should lead to a probable result, the following rules must be followed:

1. The hypothesis should be distinctly put as a question, before making the observations which are to test its truth. In other words, we must try to see what the result of predictions from the hypothesis will be.
2. The respect in regard to which the resemblances are noted must be taken at random. We must not take a particular kind of predictions for which the hypothesis is known to be good.
3. The failures as well as the successes of the predictions must be honestly noted. The whole proceeding must be fair and unbiased.

Some persons fancy that bias and counter-bias are favorable to the extraction of truth—that hot and partisan debate is the way to investigate. This is the theory of our atrocious legal procedure. But Logic puts its heel upon this suggestion. It irrefragably demonstrates that knowledge can only be furthered by the real desire for it, and that the methods of obstinacy, of authority, and every mode of trying to reach a foregone conclusion, are absolutely of no value. These things are proved. The reader is at liberty to think so or not as long as the proof is not set forth, or as long as he refrains from examining it. Just so, he can preserve, if he likes, his freedom of opinion in regard to the propositions of geometry; only, in that case, if he takes a fancy to read Euclid, he will do well to skip whatever he finds with A, B, C, etc., for, if he reads attentively that disagreeable matter, the freedom of his opinion about geometry may unhappily be lost forever.

How many people there are who are incapable of putting to their own consciences this question, “Do I want to know how the fact stands, or not?”

The rules which have thus far been laid down for induction and hypothesis are such as are absolutely essential. There are many other

maxims expressing particular contrivances for making synthetic inferences strong, which are extremely valuable and should not be neglected. Such are, for example, Mr. Mill's four methods. Nevertheless, in the total neglect of these, inductions and hypotheses may and sometimes do attain the greatest force.

#### IV

Classifications in all cases perfectly satisfactory hardly exist. Even in regard to the great distinction between explicative and ampliative inferences, examples could be found which seem to lie upon the border between the two classes, and to partake in some respects of the characters of either. The same thing is true of the distinction between induction and hypothesis. In the main, it is broad and decided. By induction, we conclude that facts, similar to observed facts, are true in cases not examined. By hypothesis, we conclude the existence of a fact quite different from anything observed, from which, according to known laws, something observed would necessarily result. The former, is reasoning from particulars to the general law; the latter, from effect to cause. The former classifies, the latter explains. It is only in some special cases that there can be more than a momentary doubt to which category a given inference belongs. One exception is where we observe, not facts similar under similar circumstances, but facts different under different circumstances—the difference of the former having, however, a definite relation to the difference of the latter. Such inferences, which are really inductions, sometimes present nevertheless some indubitable resemblances to hypotheses.

Knowing that water expands by heat, we make a number of observations of the volume of a constant mass of water at different temperatures. The scrutiny of a few of these suggests a form of algebraical formula which will approximately express the relation of the volume to the temperature. It may be, for instance, that  $v$  being the relative volume, and  $t$  the temperature, the few observations examined indicate a relation of the form—

$$v = 1 + at + bt^2 + ct^3.$$

Upon examining observations at other temperatures taken at random, this idea is confirmed; and we draw the inductive conclusion

that all observations within the limits of temperature from which we have drawn our observations could equally well be satisfied. Having once ascertained that such a formula is possible, it is a mere affair of arithmetic to find the values of  $a$ ,  $b$ , and  $c$ , which will make the formula satisfy the observations best. This is what physicists call an *empirical formula*, because it rests upon mere induction, and is not explained by any hypothesis.

Such formulæ, though very useful as means of describing in general terms the results of observations, do not take any high rank among scientific discoveries. The induction which they embody, that expansion by heat (or whatever other phenomenon is referred to) takes place in a perfectly gradual manner without sudden leaps or innumerable fluctuations, although really important, attracts no attention, because it is what we naturally anticipate. But the defects of such expressions are very serious. In the first place, as long as the observations are subject to error, as all observations are, the formula cannot be expected to satisfy the observations exactly. But the discrepancies cannot be due solely to the errors of the observations, but must be partly owing to the error of the formula which has been deduced from erroneous observations. Moreover, we have no right to suppose that the real facts, if they could be had free from error, could be expressed by such a formula at all. They might, perhaps, be expressed by a similar formula with an infinite number of terms; but of what use would that be to us, since it would require an infinite number of coefficients to be written down? When one quantity varies with another, if the corresponding values are exactly known, it is a mere matter of mathematical ingenuity to find some way of expressing their relation in a simple manner. If one quantity is of one kind—say, a specific gravity—and the other of another kind—say, a temperature—we do not desire to find an expression for their relation which is wholly free from numerical constants, since if it were free from them when, say, specific gravity as compared with water, and temperature as expressed by the centigrade thermometer, were in question, numbers would have to be introduced when the scales of measurement were changed. We may, however, and do desire to find formulas expressing the relations of physical phenomena which shall contain no more arbitrary numbers than changes in the scales of measurement might require.

When a formula of this kind is discovered, it is no longer called an empirical formula, but a law of Nature; and is sooner or later made

the basis of an hypothesis which is to explain it. These simple formulæ are not usually, if ever, exactly true, but they are none the less important for that; and the great triumph of the hypothesis comes when it explains not only the formula, but also the deviations from the formula. In the current language of the physicists, an hypothesis of this importance is called a theory, while the term hypothesis is restricted to suggestions which have little evidence in their favor. There is some justice in the contempt which clings to the word hypothesis. To think that we can strike out of our own minds a true preconception of how Nature acts, is a vain fancy. As Lord Bacon well says: "The subtlety of Nature far exceeds the subtlety of sense and intellect: so that these fine meditations, and speculations, and reasonings of men are a sort of insanity, only there is no one at hand to remark it." The successful theories are not pure guesses, but are guided by reasons.

The kinetical theory of gases is a good example of this. This theory is intended to explain certain simple formulæ, the chief of which is called the law of Boyle. It is, that if air or any other gas be placed in a cylinder with a piston, and if its volume be measured under the pressure of the atmosphere, say fifteen pounds on the square inch, and if then another fifteen pounds per square inch be placed on the piston, the gas will be compressed to one-half its bulk, and in similar inverse ratio for other pressures. The hypothesis which has been adopted to account for this law is that the molecules of a gas are small, solid particles at great distances from each other (relatively to their dimensions), and moving with great velocity, without sensible attractions or repulsions, until they happen to approach one another very closely. Admit this, and it follows that when a gas is under pressure what prevents it from collapsing is not the incompressibility of the separate molecules, which are under no pressure at all, since they do not touch, but the pounding of the molecules against the piston. The more the piston falls, and the more the gas is compressed, the nearer together the molecules will be; the greater number there will be at any moment within a given distance of the piston, the shorter the distance which any one will go before its course is changed by the influence of another, the greater number of new courses of each in a given time, and the oftener each, within a given distance of the piston, will strike it. This explains Boyle's law. The law is not exact; but the hypothesis does not lead us to it exactly. For, in the first place, if the molecules are large, they will strike each other oftener when

their mean distances are diminished, and will consequently strike the piston oftener, and will produce more pressure upon it. On the other hand, if the molecules have an attraction for one another, they will remain for a sensible time within one another's influence, and consequently they will not strike the wall so often as they otherwise would, and the pressure will be less increased by compression.

When the kinetical theory of gases was first proposed by Daniel Bernoulli, in 1738, it rested only on the law of Boyle, and was therefore pure hypothesis. It was accordingly quite naturally and deservedly neglected. But, at present, the theory presents quite another aspect; for, not to speak of the considerable number of observed facts of different kinds with which it has been brought into relation, it is supported by the mechanical theory of heat. That bringing together bodies which attract one another, or separating bodies which repel one another, when sensible motion is not produced nor destroyed, is always accompanied by the evolution of heat, is little more than an induction. Now, it has been shown by experiment that, when a gas is allowed to expand without doing work, a very small amount of heat disappears. This proves that the particles of the gas attract one another slightly, and but very slightly. It follows that, when a gas is under pressure, what prevents it from collapsing is not any repulsion between the particles, since there is none. Now, there are only two modes of force known to us, force of position or attractions and repulsions, and force of motion. Since, therefore, it is not the force of position which gives a gas its expansive force, it must be the force of motion. In this point of view, the kinetical theory of gases appears as a deduction from the mechanical theory of heat. It is to be observed, however, that it supposes the same law of mechanics (that there are only those two modes of force) which holds in regard to bodies such as we can see and examine, to hold also for what are very different, the molecules of bodies. Such a supposition has but a slender support from induction. Our belief in it is greatly strengthened by its connection with the law of Boyle, and it is, therefore, to be considered as an hypothetical inference. Yet it must be admitted that the kinetical theory of gases would deserve little credence if it had not been connected with the principles of mechanics.

The great difference between induction and hypothesis is, that the former infers the existence of phenomena such as we have observed in cases which are similar, while hypothesis supposes something of a different kind from what we have directly observed, and

frequently something which it would be impossible for us to observe directly. Accordingly, when we stretch an induction quite beyond the limits of our observation, the inference partakes of the nature of hypothesis. It would be absurd to say that we have no inductive warrant for a generalization extending a little beyond the limits of experience, and there is no line to be drawn beyond which we cannot push our inference; only it becomes weaker the further it is pushed. Yet, if an induction be pushed very far, we cannot give it much credence unless we find that such an extension explains some fact which we can and do observe. Here, then, we have a kind of mixture of induction and hypothesis supporting one another; and of this kind are most of the theories of physics.

## V

That synthetic inferences may be divided into induction and hypothesis in the manner here proposed,<sup>1</sup> admits of no question. The utility and value of the distinction are to be tested by their applications.

Induction is, plainly, a much stronger kind of inference than hypothesis; and this is the first reason for distinguishing between them. Hypotheses are sometimes regarded as provisional resorts, which in the progress of science are to be replaced by inductions. But this is a false view of the subject. Hypothetic reasoning infers very frequently a fact not capable of direct observation. It is an hypothesis that Napoleon Bonaparte once existed. How is that hypothesis ever to be replaced by an induction? It may be said that from the premise that such facts as we have observed are as they would be if Napoleon existed, we are to infer by induction that *all* facts that are hereafter to be observed will be of the same character. There is no doubt that every hypothetic inference may be distorted into the appearance of an induction in this way. But the essence of an induction is that it infers from one set of facts another set of similar facts, whereas hypothesis infers from facts of one kind to facts of another. Now, the facts which serve as grounds for our belief in the historic reality of Napoleon are not by any means necessarily the only kind of facts which are explained by his existence. It may be that, at the time of

1. This division was first made in a course of lectures by the author before the Lowell Institute, Boston, in 1866, and was printed in the *Proceedings of the American Academy of Arts and Sciences*, for April 9, 1867.

his career, events were being recorded in some way not now dreamed of, that some ingenious creature on a neighboring planet was photographing the earth, and that these pictures on a sufficiently large scale may some time come into our possession, or that some mirror upon a distant star will, when the light reaches it, reflect the whole story back to earth. Never mind how improbable these suppositions are; everything which happens is infinitely unprobable. I am not saying that *these* things are likely to occur, but that *some* effect of Napoleon's existence which now seems impossible is certain nevertheless to be brought about. The hypothesis asserts that such facts, when they do occur, will be of a nature to confirm, and not to refute, the existence of the man. We have, in the impossibility of inductively inferring hypothetical conclusions, a second reason for distinguishing between the two kinds of inference.

A third merit of the distinction is, that it is associated with an important psychological or rather physiological difference in the mode of apprehending facts. Induction infers a rule. Now, the belief of a rule is a habit. That a habit is a rule active in us, is evident. That every belief is of the nature of a habit, in so far as it is of a general character, has been shown in the earlier papers of this series. Induction, therefore, is the logical formula which expresses the physiological process of formation of a habit. Hypothesis substitutes, for a complicated tangle of predicates attached to one subject, a single conception. Now, there is a peculiar sensation belonging to the act of thinking that each of these predicates inheres in the subject. In hypothetic inference this complicated feeling so produced is replaced by a single feeling of greater intensity, that belonging to the act of thinking the hypothetic conclusion. Now, when our nervous system is excited in a complicated way, there being a relation between the elements of the excitation, the result is a single harmonious disturbance which I call an emotion. Thus, the various sounds made by the instruments of an orchestra strike upon the ear, and the result is a peculiar musical emotion, quite distinct from the sounds themselves. This emotion is essentially the same thing as an hypothetic inference, and every hypothetic inference involves the formation of such an emotion. We may say, therefore, that hypothesis produces the *sensuous* element of thought, and induction the *habitual* element. As for deduction, which adds nothing to the premises, but only out of the various facts represented in the premises selects one and brings the attention down to it, this may be considered as

the logical formula for paying attention, which is the *volitional* element of thought, and corresponds to nervous discharge in the sphere of physiology.

Another merit of the distinction between induction and hypothesis is, that it leads to a very natural classification of the sciences and of the minds which prosecute them. What must separate different kinds of scientific men more than anything else are the differences of their *techniques*. We cannot expect men who work with books chiefly to have much in common with men whose lives are passed in laboratories. But, after differences of this kind, the next most important are differences in the modes of reasoning. Of the natural sciences, we have, first, the classificatory sciences, which are purely inductive—systematic botany and zoölogy, mineralogy, and chemistry. Then, we have the sciences of theory, as above explained—astronomy, pure physics, etc. Then, we have sciences of hypothesis—geology, biology, etc.

There are many other advantages of the distinction in question which I shall leave the reader to find out by experience. If he will only take the custom of considering whether a given inference belongs to one or other of the two forms of synthetic inference given on pages 325-26, I can promise him that he will find his advantage in it, in various ways.

## *Comment se fixe la croyance*

P 129: Revue Philosophique de la France  
et de L'Étranger 6 (December 1878): 553-69

### I

On se soucie peu généralement d'étudier la logique, car chacun se considère comme suffisamment versé déjà dans l'art de raisonner. Mais il est à remarquer qu'on n'applique cette satisfaction qu'à son propre raisonnement sans l'étendre à celui des autres.

Le pouvoir de tirer des conséquences des prémisses est de toutes nos facultés celle à la pleine possession de laquelle nous atteignons

en dernier lieu, car c'est moins un don naturel qu'un art long et difficile. L'histoire du raisonnement fournirait le sujet d'un grand ouvrage. Au moyen âge, les scolastiques, suivant l'exemple des Romains, firent de la logique, après la grammaire, le premier sujet des études d'un enfant, comme étant très-facile. Elle l'était de la façon qu'ils la comprenaient. Le principe fondamental était, selon eux, que toute connaissance a pour base l'autorité ou la raison. Mais tout ce qui est déduit par la raison repose en fin de compte sur des prémisses émanant de l'autorité. Par conséquent, dès qu'un jeune homme était rompu aux procédés du syllogisme, son arsenal intellectuel passait pour complet.

Roger Bacon, ce remarquable génie qui, au milieu du XIII<sup>e</sup> siècle, eut presque l'esprit scientifique, n'apercevait dans la conception scolastique du raisonnement qu'un obstacle à la vérité. Il voyait que seule l'expérience apprend quelque chose. Pour nous, c'est là une proposition qui semble facilement intelligible, parce que les générations passées nous ont légué une notion exacte de l'expérience. A Bacon, elle paraissait aussi parfaitement claire, parce que ses difficultés ne s'étaient pas encore dévoilées. De tous les genres d'expériences, le meilleur, pensait-il, était une intuition, une lumière intime qui apprend sur la nature bien des choses que les sens ne pourraient jamais découvrir: par exemple, la transmutation des espèces.

Quatre siècles plus tard, l'autre Bacon, le plus célèbre, dans le premier livre du *Novum Organum*, donnait sa définition si claire de l'expérience, comme d'un procédé qui doit rester ouvert à la vérification et au contrôle. Toutefois, si supérieure aux idées plus anciennes que soit la définition de lord Bacon, le lecteur moderne, qui ne s'extasie pas devant sa hautaine éloquence, est surtout frappé de l'insuffisance de ses vues sur la méthode scientifique. Il suffirait de faire quelques grosses expériences, d'en résumer les résultats suivant certaines formes déterminées, de les effectuer selon la règle en écartant tout ce qui est prouvé faux et acceptant l'hypothèse qui subsiste seule après cela; de cette façon, la science de la nature serait complète au bout de peu d'années. Quelle doctrine! «Il a écrit sur la science en grand chancelier,» a-t-on dit. Cette remarque est vraie.

Les premiers savants, Kopernik, Tycho Brahé, Képler, Galilée et Gilbert, eurent des méthodes plus semblables à celles des modernes. Képler entreprit de tracer la courbe des positions de Mars.<sup>1</sup> Le plus

1. Cela n'est pas tout à fait exact, mais l'est autant qu'il se peut faire en peu de mots.

grand service qu'il ait rendu à la science a été de prémunir l'esprit humain de cette idée: que c'était ainsi qu'il fallait agir si l'on voulait faire avancer l'astronomie; qu'on ne devait pas se contenter de rechercher si tel système d'épicycles était meilleur que tel autre, mais qu'il fallait s'appuyer sur des chiffres et trouver ce que la courbe cherchée était en réalité. Il y parvint en déployant une énergie et un courage incomparables, s'attardant longuement, et d'une manière, pour nous, inconcevable, d'hypothèses en hypothèses irrationnelles, jusqu'à ce qu'après en avoir épuisé vingt et une, et simplement parce qu'il était à bout d'invention, il tomba sur l'orbite qu'un esprit bien pourvu des armes de la logique moderne aurait essayé presque tout d'abord.

C'est ainsi que tout ouvrage scientifique assez important pour vivre dans la mémoire de quelques générations témoigne de ce qu'il y avait de défectueux dans l'art de raisonner, à l'époque où il fut écrit, et chaque pas en avant fait dans la science a été un enseignement dans la logique. C'est ce qui eut lieu quand Lavoisier et ses contemporains entreprirent l'étude de la chimie. La vieille maxime des chimistes avait été: «*Lege, lege, lege, labora, ora, et relege.*» La méthode de Lavoisier ne fut pas de lire et de prier, ni de rêver que quelque opération chimique longue et compliquée aurait un certain effet; de l'exécuter avec une patience désespérante; puis, après un insuccès inévitable, de rêver qu'avec quelque modification on obtiendrait un autre résultat; puis de publier le dernier rêve comme réalité. Sa méthode était de transporter son esprit dans son laboratoire et de faire de ses alambics et de ses cornues des instruments de travail intellectuel. Il faisait concevoir d'une façon nouvelle le raisonnement comme une opération qui devait se faire les yeux ouverts, en maniant des objets réels au lieu de mots et de chimères.

La controverse sur le darwinisme est de même en grande partie une question de logique. Darwin a proposé d'appliquer la méthode statistique à la biologie. C'est ce qu'on a fait dans une science fort différente pour la théorie des gaz. Sans pouvoir dire ce que serait le mouvement de telle molécule particulière d'un gaz, dans une certaine hypothèse sur la constitution de cette classe de corps, Clausius et Maxwell ont cependant pu, par l'application de la théorie des probabilités, prédire qu'en moyenne telle ou telle proportion de molécules acquerrait dans des circonstances données telles ou telles vitesses, que dans chaque seconde se produirait tel et tel nombre de

collisions, etc. De ces données, ils ont pu déduire certaines propriétés des gaz, spécialement en ce qui touche à leurs relations caloriques. C'est ainsi que Darwin, sans pouvoir dire quels seraient sur un individu quelconque les effets de la variation et de la sélection naturelle, démontre qu'à la longue ces lois adapteront les animaux à leur milieu. Les formes animales existantes sont-elles ou non dues à l'action de ces lois? quelle place doit-on donner à cette théorie? Tout cela forme le sujet d'une controverse dans laquelle les questions de fait et les questions de logique s'entremêlent d'une singulière façon.

## II

Le but du raisonnement est de découvrir par l'examen de ce qu'on sait déjà quelque autre chose qu'on ne sait pas encore. Par conséquent, le raisonnement est bon s'il est tel qu'il puisse donner une conclusion vraie tirée de prémisses vraies; autrement, il ne vaut rien. Sa validité est donc ainsi purement une question de fait et non d'idée. A étant les prémisses, et B la conclusion, la question consiste à savoir si ces faits sont réellement dans un rapport tel, que si A est, B est. Si oui, l'inférence est juste; si non, non. La question n'est pas du tout de savoir si, les prémisses étant acceptées par l'esprit, nous avons une propension à accepter aussi la conclusion. Il est vrai qu'en général nous raisonnons juste naturellement. Mais ceci n'est logiquement qu'un accident. Une conclusion vraie resterait vraie si nous n'avions aucune propension à l'accepter, et la fausse resterait fausse, bien que nous ne pussions résister à la tendance d'y croire.

Certainement, l'homme est, somme toute, un être logique; mais il ne l'est pas complètement. Par exemple, nous sommes pour la plupart portés à la confiance et à l'espoir, plus que la logique ne nous y autoriserait. Nous semblons faits de telle sorte que, en l'absence de tout fait sur lequel nous appuyer, nous sommes heureux et satisfaits de nous-mêmes; en sorte que l'expérience a pour effet de contredire sans cesse nos espérances et nos aspirations. Cependant l'application de ce correctif durant toute une vie ne déracine pas ordinairement cette disposition à la confiance. Quand l'espoir n'est entamé par aucune expérience, il est vraisemblable que cet optimisme est extravagant. L'esprit de logique dans les choses pratiques est une des plus utiles qualités que puisse posséder un être vivant, et peut, par conséquent, être un résultat de l'action de la sélection naturelle.

Mais, les choses pratiques mises à part, il est probablement plus avantageux à l'être vivant d'avoir l'esprit plein de visions agréables et encourageantes, quelle qu'en soit d'ailleurs la vérité. Donc, sur les sujets non pratiques, la sélection naturelle peut produire une tendance d'esprit décevante.

Ce qui nous détermine à tirer de prémisses données une conséquence plutôt qu'une autre est une certaine habitude d'esprit, soit constitutionnelle, soit acquise. Cette habitude d'esprit est bonne ou ne l'est pas, suivant qu'elle porte ou non à tirer des conclusions vraies de prémisses vraies. Une inférence est considérée comme bonne ou mauvaise, non point d'après la vérité ou la fausseté de ses conclusions dans un cas spécial, mais suivant que l'habitude d'esprit qui la détermine est ou non de nature à donner en général des conclusions vraies. L'habitude particulière d'esprit qui conduit à telle ou telle inférence peut se formuler en une proposition dont la vérité dépend de la validité des inférences déterminées par cette habitude d'esprit. Une semblable formule est appelée *principe directeur* d'inférence. Supposons, par exemple, qu'on observe qu'un disque de cuivre en rotation vient promptement à s'arrêter quand on le place entre les deux pôles d'un aimant et que nous inférions que ceci arrivera à tous les disques de cuivre. Le principe directeur est ici que ce qui est vrai d'un morceau de cuivre est vrai d'un autre. Ce principe directeur serait plus valide appliqué au cuivre qu'à toute autre substance, le bronze par exemple.

On pourrait faire un livre pour relever les plus importants de ces principes directeurs du raisonnement. Ce livre, nous l'avouons, ne serait peut-être d'aucune utilité aux personnes dont toutes les pensées sont tournées vers les choses pratiques et dont l'activité se déploie dans des sentiers tout à fait battus. Les problèmes qui s'offrent à de tels esprits sont affaires de routine qu'on a, une fois pour toutes, appris à traiter en apprenant sa profession. Mais qu'un homme s'aventure sur un terrain qui ne lui est pas familier ou sur lequel les résultats de ses raisonnements ne sont pas sans cesse corrigés par l'expérience, l'histoire tout entière montre que la plus virile intelligence sera souvent désorientée et gaspillera ses efforts dans des directions qui ne la rapprocheront pas du but, qui même l'entraîneront d'un côté tout opposé. C'est comme un navire en pleine mer et à bord duquel personne ne connaît les règles de la navigation. En pareil cas, une étude sommaire des principes directeurs du raisonnement serait certainement utile.

Le sujet, toutefois, pourrait à peine se traiter s'il n'était d'abord limité, car presque tout fait peut servir de principe directeur. Mais les faits se trouveront être divisés en deux classes: l'une comprend tous ceux qui sont absolument essentiels comme principes directeurs; l'autre renferme tous ceux qui offrent un autre genre quelconque d'intérêt comme objet d'investigation. Cette distinction existe entre les faits qui sont nécessairement considérés comme admis lorsqu'on demande si certaine conclusion découle de certaines prémisses, et ceux dont l'existence n'est pas impliquée par cette question. Un instant de réflexion fera voir qu'une certaine catégorie de faits est admise dès qu'on pose cette question logique. Il est implicitement entendu, par exemple, qu'il existe des états d'esprit tels que le doute et la croyance; que le passage est possible de l'un à l'autre, l'objet de la pensée restant le même, et que cette transition est soumise à des règles qui gouvernent toutes les intelligences. Comme ce sont là des faits que nous devons déjà connaître, avant de pouvoir posséder la moindre conception claire du raisonnement, on ne peut supposer qu'il puisse y avoir encore grand intérêt à en rechercher l'exactitude et la fausseté. D'autre part, il est facile de croire que les règles les plus essentielles du raisonnement sont celles qui se déduisent de la notion même de ce procédé, et, tant que le raisonnement s'y conformera, il est certain que, pour le moins, il ne tirera point des conclusions fausses de prémisses vraies. En réalité, l'importance des faits qu'on peut déduire des postulats impliqués dans une question logique se trouve être plus grande qu'on ne l'eût supposé, et cela pour des raisons qu'il est difficile de faire voir au début de notre étude. La seule que je me bornerai à mentionner est que des concepts qui sont en réalité des produits d'une opération de logique, sans qu'ils paraissent tels au premier abord, se mêlent à nos pensées ordinaires et causent fréquemment de grandes confusions. C'est ce qui a lieu, par exemple, avec le concept de qualité. Une qualité prise en elle-même n'est jamais connue par l'observation. On peut voir qu'un objet est bleu ou vert, mais la qualité *bleu* ou la qualité *vert* ne sont point choses qu'on voit, ce sont les produits d'une opération de logique. La vérité est que le sens commun, c'est-à-dire la pensée quand elle commence à s'élever au-dessus du niveau de la pratique étroite, est profondément imprégné de cette fâcheuse qualité logique à laquelle on applique communément le nom d'esprit métaphysique. Rien ne peut l'en débarrasser, sinon une bonne discipline logique.

## III

On reconnaît en général la différence entre faire une question et prononcer un jugement, car il y a dissemblance entre le sentiment de douter et celui de croire.

Mais ce n'est pas là seulement ce qui distingue le doute de la croyance. Il existe une différence pratique. Nos croyances guident nos désirs et règlent nos actes. Les Assassins (Hatchichins) ou sectateurs du Vieux de la Montagne couraient à la mort au moindre commandement, car ils croyaient que l'obéissance à leur chef leur assurerait l'éternelle félicité. S'ils en avaient douté, ils n'eussent pas agi comme ils le faisaient. Il en est ainsi de toute croyance, en proportion de son intensité. Le sentiment de croyance est une indication plus ou moins sûre, que s'est enracinée en nous une habitude d'esprit qui déterminera nos actions. Le doute n'a jamais un tel effet.

Il ne faut pas non plus négliger un troisième point de différence. Le doute est un état de malaise et de mécontentement dont on s'efforce de sortir pour atteindre l'état de croyance. Celui-ci est un état de calme et de satisfaction qu'on ne veut pas abandonner ni changer pour adopter une autre croyance.<sup>2</sup> Au contraire, on s'attache avec ténacité non-seulement à croire, mais à croire précisément ce qu'on croit.

Ainsi, le doute et la croyance produisent tous deux sur nous des effets positifs, quoique fort différents. La croyance ne nous fait pas agir de suite, mais produit en nous des dispositions telles que nous agirons de certaine façon lorsque l'occasion se présentera. Le doute n'a pas le moindre effet de ce genre, mais il nous excite à agir jusqu'à ce qu'il ait été détruit. Cela rappelle l'irritation d'un nerf et l'action réflexe qui en est le résultat. Pour trouver dans le fonctionnement du système nerveux quelque chose d'analogue à l'effet de la croyance, il faut prendre ce qu'on appelle les associations nerveuses: par exemple, l'habitude nerveuse par suite de laquelle l'odeur d'une pêche fait venir l'eau à la bouche.

## IV

L'irritation produite par le doute nous pousse à faire des efforts pour atteindre l'état de croyance. Je nommerai cette série d'efforts *recherche*, tout en reconnaissant que parfois ce nom n'est pas absolument convenable pour ce qu'il veut désigner.

2. Je ne parle point des effets secondaires produits dans certaines circonstances par l'intervention d'autres mobiles.

L'irritation du doute est le seul mobile qui nous fasse lutter pour arriver à la croyance. Il vaut certainement mieux pour nous que nos croyances soient telles, qu'elles puissent vraiment diriger nos actions de façon à satisfaire nos désirs. Cette réflexion nous fera rejeter toute croyance qui ne nous semblera pas de nature à assurer ce résultat. La lutte commence avec le doute et finit avec lui. Donc, le seul but de la *recherche* est d'établir une opinion. On peut croire que ce n'est pas assez pour nous, et que nous cherchons non pas seulement une opinion, mais une opinion vraie. Qu'on soumette cette illusion à l'examen, on verra qu'elle est sans fondement. Sitôt qu'on atteint une ferme croyance, qu'elle soit vraie ou fausse, on est entièrement satisfait. Il est clair que rien hors de la sphère de nos connaissances ne peut être l'objet de nos investigations, car ce que n'atteint pas notre esprit ne peut être un motif d'effort intellectuel. Ce qu'on peut tout au plus soutenir, c'est que nous cherchons une croyance *que nous pensons vraie*. Mais nous pensons que chacune de nos croyances est vraie, et le dire est réellement une pure tautologie.

Il est fort important d'établir que le seul but de la *recherche* est de fixer son opinion. Cela fait d'un seul coup disparaître quelques conceptions de la preuve, vagues et erronées. On peut noter ici quelques-unes de ces conceptions:

1° Quelques philosophes ont imaginé que, pour entamer une *recherche*, il suffisait de formuler une question ou de la coucher par écrit. Ils ont même recommandé de commencer l'étude en mettant tout en question. Mais le seul fait de donner à une proposition la forme interrogative n'excite pas l'esprit à la lutte pour la croyance. Il doit y avoir doute réel et vivant; sans quoi toute discussion est oiseuse.

2° C'est une idée commune qu'une démonstration doit se poser sur des propositions irréductibles et absolument indubitables. Ces propositions sont, pour une certaine école des principes premiers universels, pour une autre des sensations premières. En réalité, une *recherche*, pour avoir ce résultat complètement satisfaisant appelé démonstration, n'a qu'à partir de propositions à l'abri de tout doute actuel. Si les prémisses n'inspirent bien réellement aucun doute, elles ne sauraient être plus satisfaisantes.

3° Il est des gens qui aiment à discuter un point dont tout le monde est convaincu. Mais cela ne peut mener plus loin. Le doute cessant, l'activité intellectuelle au sujet de la question examinée prend fin. Si elle continuait, elle serait sans but.

Si l'unique objet de la recherche est de fixer une opinion, et si la croyance est une espèce d'habitude, pourquoi n'atteindrait-on pas le but désiré, en acceptant comme réponse à une question tout ce qu'il nous plaira d'imaginer, en se le répétant, en insistant sur tout ce qui peut conduire à la croyance, et en s'exerçant à écarter avec haine et dédain tout ce qui pourrait la troubler? Cette méthode simple et sans détours est en réalité celle de bien des gens. Je me souviens qu'on me pressait un jour de ne pas lire certain journal, de crainte que mes opinions sur le libre échange n'en fussent modifiées; ou, comme on s'exprimait, <>de crainte que je ne me laisse abuser par ses sophismes et ses inexactitudes.>>— «Vous n'êtes pas, me disait-on, spécialement versé dans l'économie politique; vous pouvez donc, sur ce sujet, être aisément déçu par des arguments fallacieux. Vous pouvez, en lisant cette feuille, vous laisser entraîner aux doctrines protectionnistes. Vous admettez que la doctrine du libre échange est la vraie, et vous ne voudriez pas croire ce qui n'est pas vrai.» J'ai vu souvent adopter ce système de propos délibéré; plus souvent encore, une aversion instinctive contre l'état d'indécision, s'accroissant jusqu'à devenir une vague terreur du doute, fait qu'on s'attache convulsivement aux idées qui sont présentes dans le moment. On sent que, si l'on peut seulement se maintenir sans broncher dans sa croyance, on aura tout lieu d'être satisfait, car on ne peut nier qu'une foi robuste et inébranlable ne procure une grande paix d'esprit. Cela peut, il est vrai, produire de pernicieux effets, comme si par exemple on persistait à croire avec quelques fous que le feu ne vous brûlera pas ou qu'on sera damné pour l'éternité, si l'on ingurgite les aliments autrement qu'à l'aide d'une sonde œsophagienne. Mais alors, l'homme qui suit cette méthode n'admettra pas que les inconvénients en surpassent les avantages. «Je reste fermement attaché à la vérité, dira-t-il, et la vérité est toujours salutaire.»

Dans beaucoup de cas, il peut très-bien se faire que le plaisir puisé dans le calme de la foi contrebalance, et au delà, tous les inconvénients qui résultent de son caractère décevant. Ainsi, quand il serait vrai que la mort est l'anéantissement, si l'on croit qu'on ira certainement droit au ciel, pourvu qu'on ait accompli certaines pratiques simples, on a un plaisir peu coûteux, que ne suivra pas le moindre désappointement. Des considérations de ce genre semblent avoir une grande influence sur beaucoup de personnes dans les matières

religieuses, car souvent on entend dire: Oh! je ne puis croire telle et telle chose, car je serais damné si je la croyais. L'autruche, lorsqu'elle enfonce la tête dans le sable à l'approche du danger, tient vraisemblablement la conduite qui la rend la plus heureuse. Elle ne voit plus le danger et se dit tranquillement qu'il n'y en a pas, et, si elle est parfaitement sûre qu'il n'y a pas de danger, pourquoi leverait-elle la tête pour voir? Un homme peut parcourir la vie en détournant systématiquement ses regards de tout ce qui pourrait amener un changement dans ses opinions, et pourvu seulement qu'il réussisse,—prenant, comme il le fait, pour base de sa méthode deux lois psychologiques fondamentales,—je ne vois pas ce qu'on peut dire contre sa façon d'agir. Ce serait une présomption impertinente d'objecter que son procédé est irrationnel, car cela revient simplement à dire que sa méthode pour fixer la croyance n'est pas la nôtre. Il ne se propose pas d'être rationnel, et, de fait, il parlera souvent avec dédain de la faiblesse et des erreurs de la raison humaine. Laissez-le donc penser comme il lui convient.

Cette méthode pour fixer la croyance, qu'on peut appeler *méthode de ténacité*, ne pourra s'appliquer constamment dans la pratique; elle a contre elle les instincts sociaux. L'homme qui l'aura adoptée s'apercevra que d'autres hommes pensent autrement que lui, et, dans un moment de bon sens, il lui viendra à l'esprit que les opinions d'autrui sont aussi valables que les siennes; et cela ébranlera sa confiance en ce qu'il croit.

La conception que la pensée ou le sentiment d'un autre peuvent valoir la nôtre est certainement un progrès nouveau et très-important. Elle naît d'un instinct trop fort pour être étouffée chez l'homme, sans danger de destruction pour l'espèce. A moins de vivre en ermite, on influera nécessairement sur les opinions les uns des autres. De cette façon, le problème se ramène à savoir comment se fixe la croyance, non pas seulement chez l'individu, mais dans la société.

Qu'on substitue la volonté de l'Etat à celle de l'individu; qu'on crée des institutions ayant pour objet de maintenir les doctrines orthodoxes présentes à l'esprit des peuples, de les rappeler continuellement et de les enseigner à la jeunesse; que la loi ait en même temps le pouvoir d'empêcher l'enseignement, l'apologie ou l'expression des doctrines contraires; qu'on écarte toutes les causes qui puissent faire appréhender un changement d'idées; qu'on maintienne les hommes dans l'ignorance, de peur qu'ils n'apprennent d'une façon

quelconque à penser autrement; qu'on enrôle leurs passions de manière à leur faire considérer avec haine et avec horreur toute opinion personnelle ou sortant de l'ornière commune; qu'on réduise au silence par la terreur ceux qui rejettent la croyance d'Etat; que le peuple les chasse et les conspue, ou qu'une inquisition scrute la façon de penser des suspects, et, lorsqu'ils sont trouvés infectés de croyances interdites, qu'ils subissent un châtiment signalé. Si l'on ne pouvait arriver autrement à une complète uniformité, un massacre général de tous ceux qui pensent d'une certaine façon serait, et a été, un moyen fort efficace d'enraciner une opinion dans un pays. Si le pouvoir manque pour agir ainsi, qu'on dresse une liste d'opinions auxquelles ne puisse adhérer aucun homme ayant la moindre indépendance d'esprit, et qu'on mette les fidèles en demeure d'accepter toutes ces propositions, afin de les soustraire autant que possible à l'influence du reste du monde.

Cette méthode a depuis les temps les plus reculés fourni l'un des principaux moyens de maintenir l'orthodoxie des doctrines théologiques et politiques et de leur conserver un caractère catholique ou universel. A Rome en particulier, on l'a pratiquée du temps de Numa Pompilius à celui de Léon XIII. C'est le plus complet exemple qu'en offre l'histoire; mais, partout où il y a eu un sacerdoce, cette méthode a été plus ou moins appliquée. Partout où il existe une aristocratie ou une association quelconque d'une classe dont les intérêts ont ou sont supposés avoir pour base certaines maximes, on rencontrera nécessairement des traces de cette politique, produit naturel d'un instinct social.

Ce système est toujours accompagné de cruautés, qui, lorsqu'on l'applique avec persistance, deviennent des atrocités de la plus horrible sorte aux yeux de tout homme raisonnable. Cette conséquence ne doit pas surprendre, car le ministre d'une société ne se sent pas le droit de sacrifier à la pitié les intérêts de cette société, comme il pourrait sacrifier ses intérêts particuliers. La sympathie et l'instinct de société peuvent ainsi naturellement produire un pouvoir absolument impitoyable.

Quand on juge cette méthode de fixer la croyance, qu'on peut appeler la *méthode d'autorité*, il faut tout d'abord lui reconnaître une immense supériorité intellectuelle et morale sur la méthode de ténacité. Le succès en est proportionnellement plus grand, et de fait elle a mainte et mainte fois produit les plus majestueux résultats. Même les amoncellements de pierres qu'elle a fait entasser à Siam, en

Egypte, en Europe ont souvent une sublimité que surpassent à peine les plus grandes œuvres de la nature. A part les époques géologiques, il n'est point de périodes de temps aussi vastes que celles qu'ont parcourues plusieurs de ces croyances organisées. En y regardant de près, on verra qu'il n'en est pas dont les dogmes soient toujours demeurés les mêmes. Mais le changement y est si lent et si imperceptible, pendant la durée d'une vie humaine, que la croyance individuelle reste presque absolument fixe. Pour la grande masse des hommes, il n'y a peut-être pas de méthode meilleure. Si leur plus haute capacité est de vivre dans l'esclavage intellectuel, qu'ils restent esclaves.

Toutefois, nul système ne peut embrasser la réglementation des opinions sur tout sujet. On ne peut s'occuper que des plus importants; sur les autres, il faut abandonner l'esprit humain à l'action des causes naturelles. Cette imperfection du système ne sera pas une cause de faiblesse aussi longtemps que les opinions ne réagiront pas les unes sur les autres, c'est-à-dire aussi longtemps qu'on ne saura point additionner deux et deux. Mais, dans les Etats les plus soumis au joug sacerdotal, se rencontrent des individus qui ont dépassé ce niveau. Ces hommes ont une sorte d'instinct social plus large; ils voient que les hommes en d'autres pays et dans d'autres temps ont professé des doctrines fort différentes de celles qu'ils ont eux-mêmes été élevés à croire. Ils ne peuvent s'empêcher de remarquer que c'est par hasard qu'ils ont été instruits comme ils le sont et qu'ils ont vécu au milieu des institutions et des sociétés qui les entourent, ce qui les a fait croire comme ils croient et non pas fort différemment. Leur bonne foi ne peut échapper à cette réflexion qu'il n'y a pas de raison pour estimer leur manière de voir à plus haut prix que celle d'autres nations et d'autres siècles; et ceci fait naître des doutes dans leur esprit.

Ils apercevront ensuite qu'ils doivent nourrir des doutes semblables sur toute croyance qui semble déterminée soit par leur fantaisie propre, soit par la fantaisie de ceux qui furent les créateurs des opinions populaires. Adhérer obstinément à une croyance et l'imposer arbitrairement aux autres sont donc deux procédés qu'il faut abandonner, et pour fixer les croyances on doit adopter une nouvelle méthode qui non-seulement fasse naître une tendance à croire, mais qui détermine aussi quelles propositions il faut croire. Qu'on laisse agir sans obstacle les préférences naturelles; sous leur influence, les hommes, échangeant leurs pensées et considérant les choses de

points de vue divers, développeront graduellement des croyances en harmonie avec les causes naturelles. Cette méthode ressemble à celle qui a conduit à maturité les conceptions du domaine de l'art.

L'histoire de la philosophie métaphysique en offre un exemple parfait. Les systèmes de cet ordre ne se sont pas d'ordinaire appuyés sur des faits observés, ou du moins ne l'ont fait qu'à un assez faible degré. On les a adoptés surtout parce que les propositions fondamentales en paraissaient *agréables à la raison*. Cette expression est fort juste, elle désigne non pas les théories qui s'accordent avec l'expérience, mais celles que de nous-mêmes nous inclinons à croire. Platon, par exemple, trouve agréable à la raison que les distances des sphères célestes entre elles soient proportionnelles aux longueurs des cordes qui produisent les harmonies musicales. Ce sont des considérations de ce genre qui ont conduit bien des philosophes à leurs conclusions les plus importantes. Mais c'est là la forme la plus inférieure et la plus rudimentaire de la méthode, car il est évident qu'un autre homme peut trouver plus agréable à sa raison à lui la théorie de Kepler, que les sphères célestes sont proportionnelles aux sphères inscrites et circonscrites aux différents solides réguliers. Le choc des opinions conduira bientôt à s'appuyer sur des préférences d'un caractère plus universel. Soit par exemple la doctrine que l'homme seul agit par égoïsme, c'est-à-dire par la considération que telle façon d'agir lui procurera plus de plaisir que telle autre. Cette idée ne repose absolument sur aucun fait, mais elle a été fort généralement acceptée, comme étant la seule théorie raisonnable.

Cette méthode est bien plus intelligente et bien plus respectable aux yeux de la raison qu'aucune de celles mentionnées précédemment. Mais l'insuccès en a été plus manifeste. Elle fait de l'investigation quelque chose de semblable au goût développé: mais malheureusement le goût est toujours plus ou moins une affaire de mode; c'est pourquoi les métaphysiciens n'ont jamais pu arriver à aucun accord solide. Leurs doctrines philosophiques, depuis les temps les plus reculés jusqu'à nos jours, ont oscillé du matérialisme au spiritualisme. Aussi de cette méthode, dite *a priori*, sommes-nous amenés nécessairement à la véritable induction. Nous avons considéré cette méthode *a priori* comme un procédé qui promettait de débarrasser nos opinions des éléments accidentels et arbitraires; mais l'évolution, si elle tend à éliminer les effets de quelques circonstances fortuites, ne fait qu'augmenter ceux de certaines autres. Cette méthode ne diffère donc point d'une manière très-essentielle de la méthode d'au-

torité. Le gouvernement peut n'avoir pas levé le doigt pour influencer mes convictions; je puis avoir été laissé extérieurement complètement libre de choisir par exemple entre la monogamie et la polygamie, et, ne consultant que ma conscience, je puis avoir conclu que la polygamie était une pratique licencieuse en soi. Mais, lorsque je considère que le principal obstacle à l'expansion du christianisme chez un peuple aussi cultivé que les Hindous a été la conviction que notre manière de traiter les femmes est immorale, je ne puis m'empêcher de voir que, bien que les gouvernements n'interviennent pas ici, les sentiments seront en grande partie déterminés par des causes accidentelles. Or il existe des personnes, au nombre desquelles, je dois le croire, se trouve le lecteur, qui, dès qu'elles verront que l'une de leurs croyances est déterminée par quelque circonstance en dehors de la réalité, admettront à l'instant même et non pas seulement des lèvres que cette croyance est douteuse, mais en douteront réellement, de sorte qu'elle cessera d'être une croyance.

Pour mettre fin à nos doutes, il faut donc trouver une méthode grâce à laquelle nos croyances ne soient produites par rien d'humain, mais par quelque chose d'extérieur à nous et d'immuatable, quelque chose sur quoi notre pensée n'ait point d'effet. Quelques mystiques s'imaginent trouver une méthode de ce genre dans une inspiration personnelle d'en haut. Ce n'est là qu'une forme de la *méthode de ténacité*, avant que se soit développée la conception de la vérité comme bien commun à tous. Ce quelque chose d'extérieur et d'immuatable dont nous parlons ne serait pas extérieur, à notre sens, si l'influence en était restreinte à un individu. Ce doit être quelque chose qui agisse ou puisse agir sur tous les hommes. Bien que ces actions soient nécessairement aussi variables que la condition des individus, la méthode doit pourtant être telle que chaque homme arrive à la même conclusion finale. Telle est la *méthode scientifique*.

Son postulatum fondamental traduit en langage ordinaire est celui-ci: Il existe des réalités dont les caractères sont absolument indépendants des idées que nous pouvons en avoir. Ces réalisés affectent nos sens suivant certaines lois, et bien que nos sensations soient aussi variées que nos relations avec les choses, en nous appuyant sur les lois de la perception, nous pouvons connaître avec certitude, en nous aidant du raisonnement, comment les choses sont réellement; et tous les hommes, pourvu qu'ils aient une expérience suffisante et qu'ils raisonnent suffisamment sur ses données, seront conduits à une seule et véritable conclusion.

Ceci implique une conception nouvelle, celle de la réalité. On peut demander d'où nous savons qu'il existe des réalités. Si cette hypothèse est la base unique de notre méthode d'investigation, notre méthode d'investigation ne peut servir à confirmer cette hypothèse. Voici ce que je répondrai:

1° Si l'investigation ne peut être considérée comme prouvant qu'il existe des choses réelles, du moins elle ne conduit pas à une conclusion contraire; mais la méthode reste toujours en harmonie avec la conception qui en forme la base. Sa pratique ne fait donc pas naître des doutes sur notre méthode, comme cela arrive pour toutes les autres.

2° Le sentiment d'où naissent toutes les méthodes de fixer la croyance est une sorte de mécontentement de ne pouvoir faire accorder deux propositions. Mais alors on admet déjà vaguement qu'il existe un quelque chose à quoi puisse être conforme une proposition. Par conséquent, nul ne peut douter qu'il existe des réalités, ou, si l'on en doutait, le doute ne serait pas une cause de malaise. C'est donc là une hypothèse qu'admet toute intelligence.

3° Tout le monde emploie la méthode scientifique, dans un grand nombre de circonstances, et l'on n'y renonce que lorsqu'on ne voit plus comment l'appliquer.

4° L'usage de la méthode ne m'a pas conduit à douter d'elle; au contraire, l'investigation scientifique a obtenu les plus merveilleux succès, quand il s'est agi de fixer les opinions.

Voilà pourquoi je ne doute ni de la méthode, ni de l'hypothèse qu'elle présuppose. N'ayant aucun doute, et ne croyant pas qu'une autre personne que je peux influencer en ait plus que moi, je crois qu'en dire plus long sur ce sujet ne serait qu'un verbiage inutile. Si quelqu'un a sur ce sujet un doute réel, qu'il l'examine.

Le but de ce travail est de décrire l'investigation scientifique. Je vais pour l'instant me borner à relever quelques contrastes entre elle et les autres méthodes.

Des quatre méthodes, elle est la seule qui fasse reconnaître quelque différence entre une bonne et une fausse voie. Si l'on adopte la méthode de ténacité et qu'on se cloître à l'abri de toute influence extérieure, tout ce qu'on croit nécessaire pour parvenir à ce but est nécessaire selon l'essence même de cette méthode.

Il en est de même avec la méthode d'autorité. L'Etat peut essayer d'écraser les hérésies par des moyens qui, au point de vue scientifique, semblent très-mal calculés pour atteindre ce but. Mais le seul

critérium de cette méthode est ce que pense l'Etat, de sorte qu'il ne peut l'appliquer à faux.

Ainsi pour la méthode *a priori*. Son principe même consiste à penser comme on est enclin à le faire. Tous les métaphysiciens seront sûrs de faire cela, si enclins soient-ils à juger que leurs confrères se trompent abominablement. Le système d'Hegel admet que tout courant naturel d'idées est logique, bien qu'il soit certain qu'il sera annulé par les contre-courants. Hegel pense que ces courants se succèdent d'une façon régulière, de sorte que, après s'être longtemps égarée dans une voie et dans une autre, l'opinion finit par prendre la bonne direction. Il est en effet vrai que les métaphysiciens atteignent à la fin des idées justes. Le système hégelien de la nature reflète assez bien l'état de la science de son époque, et, l'on peut en être certain, tout ce que l'investigation scientifique aura mis hors de doute sera gratifié par les métaphysiciens d'une démonstration *a priori*.

Avec la méthode scientifique, les choses se passent autrement. Je puis partir des faits connus et observés pour aller à l'inconnu, sans que cependant les règles que je suivrai en agissant ainsi soient telles que les exige l'investigation. Mon critérium, pour savoir si je suis vraiment la méthode, n'est pas un appel direct à mes sentiments et à mes intentions, mais au contraire il implique en lui-même l'application de la méthode; de là vient que le mauvais raisonnement est aussi bien possible que le bon. Ce fait est le fondement de la partie pratique de la logique.

Il ne faut pas supposer que les trois autres méthodes de fixer la croyance n'aient aucune espèce de supériorité sur la méthode scientifique. Au contraire, chacune offre des avantages qui lui sont propres. La méthode *a priori* se distingue par le caractère agréable de ses conclusions. L'essence de ce procédé est d'adopter toute croyance que nous avons de la propension à admettre. Il y a certaines choses flatteuses pour la vanité de l'homme et que tous nous croyons naturellement, jusqu'à ce que nous soyons réveillés de notre songe par quelque fait brutal.—La méthode d'autorité régira toujours la grande masse des hommes, et ceux qui détiennent dans l'Etat la force organisée sous diverses formes ne seront jamais convaincus que les doctrines dangereuses ne doivent pas être supprimées de façon ou d'autre. Si la liberté de parler reste à l'abri des formes grossières de contrainte, on assurera l'uniformité d'opinion par une terreur morale que sanctionnera sans restriction la pruderie sociale. Appliquer la

méthode d'autorité, c'est avoir la paix. Certains dissensments sont permis; d'autres (jugés dangereux) sont interdits. Cela varie suivant les lieux et les temps; mais, n'importe où vous êtes, laissez voir que vous êtes sérieusement partisan de quelque croyance à l'*index*, et vous pouvez être certain qu'on vous traitera avec une cruauté moins brutale, mais plus raffinée que si l'on vous traquait comme un loup. Aussi les plus grands bienfaiteurs de l'intelligence humaine n'ont jamais osé, et n'osent pas encore, dire leur pensée tout entière. Cela fait qu'un nuage de doute plane de prime abord sur toute proposition, considérée comme essentielle au salut de la société. Et, chose assez singulière, la persécution ne vient pas toujours de l'extérieur: l'homme se tourmente lui même et souvent est plongé dans le désespoir, en découvrant qu'il croit à des doctrines que par éducation il considère avec horreur. Aussi l'homme paisible et doux résistera-t-il avec peine à la tentation de soumettre ses opinions à l'autorité.

Mais, par-dessus tout, j'admire la méthode de ténacité pour sa force, sa simplicité, sa droite ligne. Ceux qui en font usage sont remarquables par leur caractère décidé, la décision devenant très-facile avec une pareille règle intellectuelle. Ils ne perdent pas leur temps à examiner ce qu'il leur faut; mais saisissant, prompts comme l'éclair, l'alternative quelconque qui s'offre la première, ils s'y attachent jusqu'au bout, quoi qu'il advienne sans un instant d'irrésolution. Un tel caractère est un de ces dons splendides qui accompagnent généralement des succès brillants et éphémères. Impossible de ne pas envier l'homme qui peut mettre de côté la raison, bien qu'on sache ce qui doit à la fin en résulter.

Tels sont les avantages des autres méthodes sur celle de l'investigation scientifique. On doit bien en tenir compte. Puis on considère qu'après tout on désire que ses opinions soient conformes à la réalité, et qu'il n'y a pas de raison pour que tel soit le résultat de ces trois méthodes. Un tel résultat n'est dû qu'à la méthode scientifique. D'après ces considérations, il faut choisir, et ce choix est bien plus que l'adoption pour l'esprit d'une opinion quelconque: c'est une de ces résolutions qui règleront l'existence et à laquelle, une fois prise, on est obligé de se tenir. Par la force de l'habitude, on reste quelquefois attaché à ses vieilles croyances après qu'on est en état de voir qu'elles n'ont aucun fondement. Mais, en réfléchissant sur l'état de la question, on triomphera de ces habitudes; on doit laisser à la réflexion tout son effet. Il répugne à certaines gens d'agir ainsi, parce qu'ils ont l'idée que les croyances sont choses salutaires, même quand ils ne

peuvent s'empêcher de voir qu'elles ne reposent sur rien. Mais supposons un cas analogue à celui de ces personnes, bien que fort différent. Que diraient-elles d'un musulman converti à la religion réformée qui hésiterait à abandonner ses anciennes idées sur les relations entre les sexes. Ne diraient-elles pas que cet homme doit examiner les choses à fond, de façon à comprendre clairement sa nouvelle doctrine et à l'embrasser en totalité. Par-dessus tout, il faut considérer qu'il y a quelque chose de plus salutaire que toute croyance particulière: c'est l'intégrité de la croyance, et qu'éviter de scruter les bases d'une croyance, par crainte de les trouver vermoulues, est immoral tout autant que désavantageux. Avouer qu'il existe une chose telle que le vrai, distinguée du faux simplement par ce caractère que, si l'on s'appuie sur elle, elle conduira au but que l'on cherche sans nous égarer, avouer cela et, bien qu'en étant convaincu, ne pas oser connaître la vérité, chercher au contraire à l'éviter, c'est là, certes, une triste situation d'esprit.

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## *Comment rendre nos idées claires*

*P 162: Revue Philosophique de la France et de L'Étranger 7 (January 1879): 39–57*

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### I

Pour peu qu'on ait ouvert un traité moderne sur la logique telle qu'on l'enseigne d'ordinaire, on se rappellera sans doute qu'on y divise les conceptions en claires et en obscures, en distinctes et en confuses. Ces divisions se rencontrent dans les livres depuis près de deux siècles, sans progrès et sans changement, et les logiciens les mettent généralement au nombre des perles de la science.

On définit idée claire une idée saisie de telle sorte qu'elle sera reconnue partout où on la rencontrera, de sorte que nulle ne sera prise pour elle. A défaut de cette clarté, l'idée est dite obscure.

Voici là un assez joli morceau de terminologie philosophique.

Pourtant, puisque c'était la clarté que définissaient les logiciens, on souhaiterait qu'ils eussent fait leur définition un peu plus claire. Ne jamais manquer de reconnaître une idée sous quelque forme qu'elle se dérobe et dans aucune circonstance, n'en prendre aucune autre pour elle, impliquerait à coup sûr une puissance et une clarté d'esprit si prodigieuses, qu'elles ne se rencontrent que rarement. D'autre part, le simple fait de connaître une idée assez pour s'être familiarisé avec elle, au point de ne pas hésiter à la reconnaître dans les circonstances ordinaires, semble mériter à peine d'être nommé une claire compréhension. Ce n'est après tout qu'un sentiment subjectif de possession qui peut être entièrement erroné. Toutefois, je tiens qu'en parlant de clarté les logiciens n'entendent rien de plus qu'une familiarité de ce genre avec une idée, puisqu'ils n'accordent pas une bien grande valeur à cette qualité prise en elle-même, car elle doit être complétée par une autre, celle d'être distincte.

Une idée est dite distincte quand elle ne comprend rien qui ne soit clair: ce sont là des termes techniques. La compréhension d'une idée dépend pour les logiciens de ce que contient sa définition. Ainsi, suivant eux, une idée est comprise *distinctement* lorsqu'on peut en donner une définition précise en termes abstraits. Les logiciens de profession en restent là, et je n'aurais point fatigué le lecteur de ce qu'ils ont à dire, si ce n'était un exemple frappant de la façon dont ils ont sommeillé dans des siècles d'activité intellectuelle, insoucieux des ressources de la pensée moderne, et ne songeant jamais à en appliquer les enseignements à l'avancement de la logique. Il est aisément de montrer que cette doctrine, suivant laquelle la compréhension parfaite consiste dans l'usage familier d'une idée et dans sa distinction abstraite, a sa place marquée parmi les philosophies depuis longtemps éteintes. Il faut maintenant formuler la méthode qui fait atteindre une clarté de pensée plus parfaite, telle qu'on la voit et qu'on l'admirer chez les penseurs de notre temps.

Lorsque Descartes entreprit de reconstruire la philosophie, son premier acte fut de commencer en théorie par le scepticisme et d'écartier la tradition scolastique, qui était de considérer l'autorité comme base première de la vérité. Cela fait, il chercha une source plus naturelle de principes vrais et déclara la trouver dans l'esprit humain. Il passa pour ainsi dire de la méthode d'autorité à la méthode *à priori*, telle qu'elle est décrite dans notre première partie. La perception intérieure devait nous fournir les vérités fondamentales et décider ce qui agréait à la raison. Mais, comme évidemment

toutes les idées ne sont pas vraies, il fut conduit à remarquer comme premier caractère de certitude qu'elles devaient être claires. Il n'a jamais songé à distinguer une idée qui paraît claire d'une idée qui est réellement telle. S'en rapportant, comme il le faisait, à l'observation intérieure, même pour connaître les objets extérieurs, pourquoi aurait-il mis en doute le témoignage de sa conscience sur ce qui se passait dans son esprit lui-même? Mais alors il faut supposer que, voyant des hommes qui lui semblaient avoir l'esprit parfaitement clair et positif appuyer sur des principes fondamentaux des opinions opposées, il fut amené à faire un pas de plus et à dire que la clarté des idées ne suffisait pas, mais qu'elles devaient encore être distinctes, c'est-à-dire ne contenir rien qui ne fût clair. Par ces mots, il entendait sans doute, car il ne s'est pas expliqué avec précision, qu'elles doivent être soumises à l'épreuve de la critique dialectique, qu'elles doivent non-seulement sembler claires au premier abord, mais que la discussion ne doit jamais pouvoir découvrir d'obscurités dans ce qui s'y rattache.

Telle était la distinction faite par Descartes, et l'on voit que cela est en harmonie avec son système philosophique. Sa théorie fut un peu développée par Leibniz. Ce grand et singulier génie est aussi remarquable par ce qui lui a échappé que par ce qu'il a vu. Qu'un mécanisme ne pût fonctionner perpétuellement sans que la force en fût alimentée de quelque façon, c'était là une chose évidente pour lui; cependant il n'a pas compris que le mécanisme de l'intelligence peut transformer la connaissance, mais non pas la produire, à moins qu'il ne soit alimenté de faits par l'observation. Il oubliait ainsi l'axiome le plus essentiel de la philosophie cartésienne: qu'il est impossible de ne pas accepter les propositions évidentes, qu'elles soient ou non conformes à la logique. Au lieu de considérer le problème de cette façon, il chercha à réduire les premiers principes en formules qu'il est contradictoire de nier, et sembla ne pas apercevoir combien grande était la différence qui le séparait de Descartes. Il revient ainsi au vieux formalisme logique; les définitions abstraites jouent un grand rôle dans son système. Observant que la méthode de Descartes offrait cet inconvénient qu'il peut nous sembler que nous saisissions clairement des idées en réalité fort confuses, il ne vit naturellement pas d'autre remède que d'exiger une définition abstraite de tout terme important. C'est pourquoi, en discernant entre les idées claires et les idées distinctes, il décrivit ces dernières comme des idées dont la définition ne contient rien qu'on ne saisisse clairement.

Tous les ouvrages de logique ont copié ses paroles. Il n'est pas à craindre qu'on se remette jamais à faire trop grand cas de son chimérique projet. Rien de nouveau ne peut s'apprendre par l'analyse des définitions. Néanmoins, ce procédé peut mettre de l'ordre dans nos croyances actuelles, et l'ordre est un élément essentiel dans l'économie de l'intelligence, comme en toute autre chose. Reconnaissions donc que les livres ont eu raison de présenter la familiarité de l'esprit avec une notion comme un premier pas, et sa définition comme un second pas vers sa claire compréhension. Mais, en omettant toute mention d'une perspicacité intellectuelle plus haute, ils ne font que refléter une philosophie rejetée depuis cent ans. La théorie tant admirée des idées claires et des idées distinctes, ce joyau de la logique, est peut-être assez jolie, mais il est grand temps de reléguer au musée des curiosités cet antique bijou et de prendre quelque chose de plus assorti aux moeurs modernes.

La première chose qu'on est en droit de demander à la logique est de nous enseigner à rendre nos idées claires; c'est un enseignement fort important, dédaigné par ceux-là seuls qui en ont besoin. Connaître ses idées, savoir bien ce qu'on veut dire, c'est là un solide point de départ pour penser avec largeur et gravité. C'est un art qu'apprennent très-facilement les esprits à conceptions sèches et restreintes, bien plus heureux que ceux qui se débattent désespérément dans un chaos touffu d'idées. Un peuple peut, il est vrai, dans une longue suite de générations, remédier aux inconvénients d'une excessive richesse de langue et à son accompagnement naturel, une vaste et insondable profondeur d'idées. On peut le voir dans l'histoire perfectionner lentement ses formes littéraires, débrouillant à la longue sa métaphysique, et grâce à une infatigable patience, qu'il a souvent comme dédommagement, atteignant un haut degré dans tous les genres de culture intellectuelle. L'histoire n'a pas encore déroulé les pages qui nous diront si, dans la suite des temps, un tel peuple l'emportera sur un autre peuple ayant les idées en aussi petit nombre que les mots de sa langue, mais exerçant une maîtrise parfaite sur les idées qu'il a. Toutefois, on ne peut douter que pour l'individu quelques idées claires vaillent mieux qu'un grand nombre d'idées confuses. On persuaderait difficilement à un jeune homme de sacrifier la plus grande partie de ses idées pour savoir le reste, et une tête encombrée est moins apte que toute autre à sentir la nécessité de ce sacrifice. Le plus souvent, un esprit de cette trempe est à plaindre, comme l'est une personne affligée d'un défaut constitution-

nel. Le temps viendra à son secours; mais, sous le rapport de la clarté des idées, il ne sera mûr qu'assez tard. C'est une fâcheuse loi de la nature, car la clarté des idées est moins utile à l'homme avancé dans la vie et dont les erreurs ont en grande partie produit leur effet, qu'elle ne le serait à l'homme au début de sa carrière. C'est chose terrible à voir, comment une seule idée confuse, une simple formule sans signification, couvant dans une jeune tête, peut quelquefois, comme une substance inerte obstruant une artère, arrêter l'alimentation cérébrale et condamner la victime à dépérisir dans la plénitude de son intelligence, au sein de l'abondance intellectuelle. Plus d'un a durant des années caressé avec tendresse quelque vague semblant d'idée, trop dépourvue de sens pour être fausse. Malgré cela, il l'a passionnément aimée et en a fait la compagne de ses jours et de ses nuits; il lui a consacré ses forces et sa vie, il a pour elle mis de côté toute autre préoccupation, il a en un mot vécu pour elle et par elle, tant qu'enfin elle devienne l'os de ses os et la chair de sa chair. Puis, un beau matin, il s'est réveillé et ne l'a plus trouvée, elle s'était évanouie dans l'air comme Mélusine, la belle fée, et toute sa vie s'était envolée avec elle. J'ai connu moi-même un de ces hommes. Qui pourrait compter tous les quadrateurs de cercle, métaphysiciens, astrologues, que sais-je encore, dont les annales de la vieille Allemagne pourraient nous redire l'histoire?

## II

Les principes exposés dans notre première partie conduisent immédiatement à une méthode qui fait atteindre une clarté d'idées bien supérieure à «l'idée distincte» des logiciens. Nous avons reconnu que la pensée est excitée à l'action par l'irritation du doute, et cesse quand on atteint la croyance: produire la croyance est donc la seule fonction de la pensée. Ce sont là toutefois de bien grands mots pour ce que je veux dire; il semble que je décrive ces phénomènes comme s'ils étaient vus à l'aide d'un microscope moral. Les mots doute et croyance, comme on les emploie d'ordinaire, sont usités quand il est question de religion ou d'autres matières importantes. Je les emploie ici pour désigner la position de toute question grande ou petite et sa solution. Lorsqu'on voit dans sa bourse une pièce d'argent et son équivalent en billon, on décide, du temps que la main s'y porte, en quelle monnaie on payera son emplette. Appeler une telle alternative doute, et la décision croyance, c'est à coup

sûr employer des mots hors de proportion avec les choses; et parler d'un tel doute comme produisant une irritation qu'il faille faire cesser, c'est suggérer l'idée d'une sensibilité impressionnable presque jusqu'à la folie. Cependant, à considérer scrupuleusement les faits, il faut admettre que si l'on éprouve la moindre hésitation à payer en argent ou en billet, ce qui aura lieu infailliblement à moins qu'on agisse en pareil cas par suite d'une habitude contractée d'avance, il faut, dis-je, admettre que si le mot irritation dépasse la mesure, on est néanmoins excité à la minime activité intellectuelle, qui peut être nécessaire pour décider l'acte en question. La plupart du temps, les doutes naissent d'une indécision, même passagère, dans nos actions. Quelquefois il n'en est pas ainsi. Par exemple, on attend à une station de chemin de fer. Pour tuer le temps, on lit les affiches sur le mur. On compare les avantages de différents trains et de différentes routes qu'on ne s'attend pas à prendre jamais: on fait seulement semblant de balancer parce qu'on est las de n'avoir à s'inquiéter de rien. L'hésitation feinte dans un but de simple amusement ou dans un but de haute spéculation joue un grand rôle dans l'engendrement de l'investigation scientifique. Quelle que soit son origine, le doute stimule l'esprit à une activité faible ou énergique, calme ou violente. La conscience voit passer rapidement des idées qui se fondent incessamment l'une dans l'autre,—cela peut durer une fraction de seconde, une heure ou des années,—jusqu'à ce qu'enfin, tout étant terminé, nous avons décidé comment nous agirons en des circonstances semblables à celles qui ont causé chez nous l'hésitation, le doute. En d'autres termes, nous avons atteint l'état de croyance.

Observons ici deux sortes d'éléments de perception intérieure, dont quelques exemples feront mieux saisir la différence. Dans un morceau de musique, il y a des notes séparées et il y a l'air. Un simple son peut être prolongé une heure ou une journée; il existe aussi parfaitement dans chaque seconde que durant tout cet espace de temps. De cette façon, aussi longtemps qu'il résonne, il est présent à un esprit auquel le passé échapperait aussi complètement que l'avenir lui-même. Mais il en est autrement de l'air. Son exécution occupe un certain temps, et dans les parties de ce temps ne sont jouées que des parties de l'air. L'air consiste en une succession ordonnée de sons qui frappent l'oreille à différents moments. Pour percevoir l'air, il faut qu'il existe dans la conscience une continuité qui rende présents pour nous les faits accomplis dans un certain laps de

temps. Evidemment nous ne percevons l'air qu'en entendant séparément les notes; on ne peut donc pas dire que nous l'entendons directement, car nous n'entendons que ce qui se passe à l'instant présent, et une succession de faits ordonnés ne peut exister en un seul instant. Ces deux sortes d'éléments que la conscience perçoit, les uns immédiatement, les autres *médialement*, se retrouvent dans toute perception intérieure. Certains éléments, les sensations, sont complètement présentes à chaque instant aussi longtemps qu'elles durent; les autres, comme les pensées, sont des actes ayant un commencement, un milieu et une fin, et consistent dans un accord de sensations qui se succèdent et traversent l'esprit. Elles ne peuvent être présentes pour nous d'une façon immédiate, mais elles doivent s'étendre quelque peu dans le passé et dans l'avenir. La pensée est comme le fil d'une mélodie qui parcourt la suite de nos sensations.

On peut ajouter que, comme un morceau de musique peut être écrit en parties ayant chacune son air, ainsi les mêmes sensations peuvent appartenir à différents systèmes de successions ordonnées. Ces divers systèmes se distinguent comme comprenant des mobiles, des idées et des fonctions différentes. La pensée n'est qu'un de ces systèmes; car ses seuls mobiles, idées et fonctions, sont de produire la croyance, et tout ce qui ne tend pas à ce but appartient à d'autres systèmes d'associations. L'acte de penser peut quelquefois avoir d'autres résultats; il peut servir à nous amuser. Par exemple, il n'est pas rare de trouver parmi les *dilettanti* des hommes qui ont tellement perverti leur pensée dans un but de plaisir, qu'ils paraissent fâchés en songeant que les questions sur lesquelles ils aiment à exercer la finesse de leur esprit, peuvent finir par être résolues. Une découverte positive qui met hors des débats littéraires un de leurs sujets favoris de discussion, rencontre chez eux un mauvais vouloir mal déguisé. Une pareille tendance est une véritable débauche d'esprit. Mais la pensée, dans son essence et dans son but, abstraction faite de ses autres éléments, même lorsqu'elle est volontairement faussée, ne peut jamais tendre vers autre chose que la production de la croyance. La pensée en activité ne poursuit d'autre but que le repos de la pensée; tout ce qui ne touche point à la croyance ne fait point partie de la pensée proprement dite.

Qu'est-ce donc que la croyance? C'est la *demi-cadence* qui clôt une phrase musicale dans la symphonie de notre vie intellectuelle. Nous avons vu qu'elle a juste trois propriétés. D'abord elle est quelque chose dont nous avons connaissance; puis elle apaise l'irrita-

tion causée par le doute; enfin elle implique l'établissement dans notre esprit d'une règle de conduite, ou, pour parler plus brièvement, d'une *habitude*.

Puisqu'elle apaise l'irritation du doute qui excite à l'action, elle détend l'esprit qui se repose pour un moment lorsqu'il a atteint la croyance. Mais la croyance étant une règle d'action, dont l'application implique un nouveau doute et une réflexion nouvelle, en même temps qu'elle est un point de repos, elle est aussi un nouveau point de départ. C'est pourquoi j'ai cru pouvoir appeler l'état de croyance la pensée au repos, bien que la pensée soit essentiellement une action. Le résultat final de la pensée est l'exercice de la volonté, fait auquel n'appartient plus la pensée. La croyance n'est qu'un moment d'arrêt dans notre activité intellectuelle, un effet produit sur notre être par la pensée et qui influe sur la pensée future.

La marque essentielle de la croyance est l'établissement d'une habitude, et les différentes espèces de croyance se distinguent par les divers modes d'action qu'elles produisent. Si les croyances ne diffèrent point sous ce rapport, si elles mettent fin au même doute en créant la même règle d'action, de simples différences dans la façon de les percevoir ne suffisent pas pour en faire des croyances différentes, pas plus que jouer un air avec différentes clefs n'est jouer des airs différents. On établit souvent des distinctions imaginaires entre des croyances qui ne diffèrent que par la façon dont elles sont exprimées. Les dissensions qui naissent de là sont toutefois fort réelles. Croire que des objets sont disposés comme dans la figure 1, et croire qu'ils le sont comme dans la figure 2, c'est croire une seule et même chose. Cependant on peut concevoir que cela n'apparaisse pas au premier abord, et qu'un homme, de deux propositions présentées d'une façon analogue, puisse accepter l'une et rejeter l'autre.

Ces fausses distinctions sont aussi nuisibles que la confusion de croyances réellement différentes et sont au nombre des pièges dont nous devons constamment nous préoccuper, surtout sur le terrain métaphysique. Une erreur de ce genre, et qui se produit fréquemment, consiste à prendre l'effet même de l'obscurité de notre pensée pour une propriété de l'objet auquel nous pensons. Au lieu d'apercevoir que cette obscurité est purement subjective, nous nous imaginons considérer une qualité essentiellement mystérieuse de l'objet, et si la même acception se présente ensuite à nous sous une forme claire, nous ne la reconnaissons plus par suite de la disparition de cet élément inintelligible. Aussi longtemps que dure cette méprise, elle

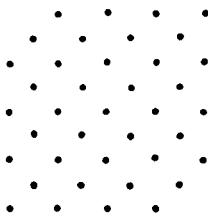


Fig. 1.

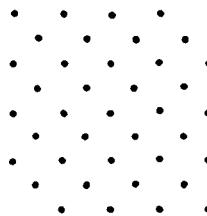


Fig. 2.

est un infranchissable obstacle à la clarté de la pensée. Perpétuer cette confusion est donc aussi important pour les adversaires de la raison qu'il est important pour ses partisans de se mettre en garde de ce côté.

Une autre méprise consiste à prendre une simple différence grammaticale entre deux mots pour une différence entre les idées qu'ils expriment. Dans un siècle pédantesque où la grande masse des écrivains s'occupent bien plus des mots que des choses, cette erreur est assez commune. Quand je disais tout à l'heure que la pensée est une *action* et qu'elle consiste en une relation, bien qu'une personne accomplisse une action et non une relation qui ne peut être que le résultat d'une action, cependant il n'y avait point là contradiction, mais seulement un certain vague grammatical.

On sera complètement à l'abri de tous ces sophismes tant qu'on réfléchira que toute la fonction de la pensée est de créer des habitudes d'action et que tout ce qui se rattache à la pensée sans concourir à son but en est un accessoire, mais n'en fait pas partie. S'il existe quelque ensemble de sensations qui n'ait aucun rapport avec la manière dont nous agirons dans une circonstance donnée,—comme par exemple quand on écoute un morceau de musique,—nous n'appelons point cela penser.

Pour développer le sens d'une pensée, il faut donc simplement déterminer quelles habitudes elle produit, car le sens d'une chose consiste simplement dans les habitudes qu'elle implique. Le caractère d'une habitude dépend de la façon dont elle peut nous faire agir non pas seulement dans telle circonstance probable, mais dans toute circonstance possible, si improbable qu'elle puisse être. Ce qu'est une habitude dépend de ces deux points: quand et comment elle fait agir. Pour le premier point: quand? tout stimulant à l'action dérive d'une perception; pour le second point: comment? le but de toute

action est d'amener au résultat sensible. Nous atteignons ainsi le tangible et le pratique comme base de toute différence de pensée, si subtile qu'elle puisse être. Il n'y a pas de nuance de signification assez fine pour ne pouvoir produire une différence dans la pratique.

Considérons, à la lumière de ce principe, où nous sommes conduits, dans une question comme la transsubstantiation. Les Eglises protestantes admettent en général que les éléments du sacrement ne sont de la chair et du sang que dans un sens symbolique: ils nourrissent les âmes, comme la viande et son suc nourrissent les corps. Les catholiques au contraire soutiennent que ce sont bien de la chair et du sang en réalité, bien qu'ils aient toutes les propriétés sensibles du pain sans levain, et du vin étendu d'eau. Mais nous pouvons avoir du vin une autre conception que celle qui peut entrer dans une croyance. De deux choses l'une:

Ou une telle chose est du vin;  
Ou le vin a certaines propriétés.

Ces croyances sont seulement des assurances que nous nous donnons à nous-mêmes qu'à l'occasion nous agirons vis-à-vis de ce que nous croyons être du vin, selon les propriétés que nous croyons appartenir au vin. L'occasion d'un tel acte serait la perception d'un fait sensible, et son but la production de quelque effet sensible. Ainsi nos actions ont exclusivement pour objet ce qui affecte les sens; notre habitude a le même caractère que nos actions; notre croyance que notre habitude et notre conception que notre croyance. Donc, par vin nous n'entendons rien autre chose que ce qui produit sur les sens divers effets directs ou indirects, et parler d'un objet doué de toutes les propriétés matérielles du vin comme étant en réalité du sang n'est qu'un jargon dépourvu de sens.

Mais mon but n'est pas d'examiner cette question théologique, et, après m'en être servi comme d'un exemple en logique, je l'abandonne sans vouloir préjuger la réponse du théologien. Je désire seulement montrer combien il est impossible qu'il y ait dans nos intelligences une idée qui ait un autre objet que des conceptions de faits sensibles. L'idée d'une chose quelconque est l'idée de ses effets sensibles. S'imaginer qu'on en a d'autres, c'est s'abaisser et prendre une simple sensation accompagnant la pensée pour une partie de la pensée elle-même. Il est absurde de dire que la pensée contient quelque élément qui soit sans rapport avec son unique fonction. C'est folie de la part des catholiques et des protestants de se croire en désaccord sur les éléments du sacrement s'ils sont d'accord sur tous leurs effets sensibles, présents et à venir.

Il semble donc que la règle pour atteindre le troisième degré de clarté dans la compréhension peut se formuler de la manière suivante: Considérer quels sont les effets pratiques que nous pensons pouvoir être produits par l'objet de notre conception. La conception de tous ces effets est la conception complète de l'objet.

### III

Quelques exemples pour faire comprendre cette règle. Commençons par le plus simple possible, et demandons-nous ce que nous entendons en disant qu'une chose est *dure*. Evidemment nous voulons dire qu'un grand nombre d'autres substances ne la rayeront pas. La conception de cette propriété comme de toute autre, est la somme de ses effets conçus par nous. Il n'y a pour nous absolument aucune différence entre une chose dure et une chose molle tant que nous n'avons pas fait l'épreuve de leurs effets. Supposons donc qu'un diamant soit cristallisé au milieu d'un moelleux coussin de coton, et qu'il y reste jusqu'à ce qu'il soit entièrement brûlé. Serait-il faux de dire que ce diamant était mou? Cette proposition semble insensée et serait telle en effet, sauf dans le domaine de la logique. Là, de pareilles questions sont souvent fort utiles pour mettre en relief les principes logiques, mieux que ne pourraient jamais le faire des discussions d'un caractère pratique. Quand on étudie la logique, on ne doit pas les écarter par des réponses précipitées, mais les examiner avec un soin minutieux pour en extraire les principes qu'elles contiennent. Dans le cas actuel, il faut modifier notre question et demander ce qui nous empêche de dire que tous les corps durs restent parfaitement mous jusqu'à ce qu'on les touche, qu'alors la pression augmente leur dureté jusqu'au moment où ils sont rayés. La réflexion montre que la réponse est qu'il n'y aurait pas de fausseté dans cette façon de parler. Elle implique soit une modification dans l'emploi actuel des mots *dur* et *mou* dans la langue, mais non de leur signification. En effet, ces expressions ne représenteraient aucun fait comme différent de ce qu'il est: elles impliqueraient seulement des arrangements d'idées qui seraient excessivement incommodes.

Ceci conduit à remarquer que la question de ce qui arriverait en des circonstances qui n'existent pas actuellement n'est pas une question de fait, mais seulement d'un plus clair arrangement de faits. Par exemple, la question du libre arbitre et du destin, dépouillée de tout verbiage, se réduit à peu près à ceci. J'ai fait une action dont j'ai honte; aurais-je pu, par un effort de volonté, résister à la tentation et

agir d'autre façon? La réponse philosophique est que ce n'est point là une question de fait, mais seulement une question d'arrangement de faits. Disposons-les de façon à mettre en lumière ce qui touche plus particulièrement à ma question, c'est-à-dire si je dois me reprocher d'avoir mal agi.—Il est parfaitement exact de dire que, si j'avais voulu agir autrement que je n'ai fait, j'aurais agi autrement. Mais disposons maintenant les faits de façon à mettre en relief une autre considération importante: il est également vrai que si on laisse agir une tentation et si elle a une certaine force, elle produira son effet: à moi de résister comme je le puis. Que le résultat d'une hypothèse fausse soit contradictoire, cela n'est pas une objection. La réduction à l'absurde consiste à montrer que les conséquences d'une certaine hypothèse seraient contradictoires, et cela fait naturellement juger fausse cette hypothèse. Les discussions sur le libre arbitre touchent à un grand nombre de questions, et je suis loin de vouloir dire que les deux façons de résoudre le problème soient également justes. Je suis d'avis au contraire que l'une des solutions est en contradiction avec certains faits importants, et que l'autre ne l'est pas. Ce que je prétends, c'est que la question formulée plus haut est la source de tout le doute, que sans cette question aucune controverse ne se serait jamais élevée, enfin que cette question se résout complètement de la manière que j'ai indiquée.

Cherchons maintenant une idée claire de la pesanteur; c'est là un autre exemple bien facile à saisir. Dire qu'un corps est pesant signifie simplement qu'en l'absence de toute force opposante il tombera. C'est là évidemment toute la conception de la pesanteur,—en mettant de côté certains détails spéciaux sur les lois de la chute des corps, et présents à l'esprit du physicien qui emploie le mot pesanteur. C'est une question importante de savoir si certains faits particuliers *n'expliquent pas* la pesanteur; mais ce que nous entendons par cette force elle-même consiste entièrement dans la somme de ses effets.

Tout nous conduit à entreprendre l'analyse de l'idée de *force* en général. C'est là la grande conception qui, dès le commencement du XVII<sup>e</sup> siècle, se dégageant de la notion rudimentaire de cause, et se développant sans cesse, nous a donné le moyen d'expliquer les modifications de mouvement éprouvées par les corps et enseigné la manière d'envisager tous les phénomènes physiques. C'est d'elle qu'est née la science moderne, c'est elle qui a changé la face du globe, elle qui, en dehors des applications spéciales, a joué un rôle prépondérant dans la direction de la pensée moderne, et reculé les

limites du développement social; et elle mérite donc qu'on se donne quelque peine pour la bien saisir. Conformément à notre règle, il faut commencer par se demander quelle utilité immédiate il y a pour nous à méditer sur la force. La réponse est qu'ainsi nous essayons d'expliquer les modifications du mouvement. Si les corps étaient abandonnés à eux-mêmes sans qu'aucune force intervint, tout mouvement se continuerait sans changement de vitesse ni de direction. En outre, les changements qui se produisent ne sont jamais brusques. Si la direction change, c'est toujours suivant une courbe sans angles; si la rapidité varie, c'est par degrés. Ces changements graduels, qui se produisent sans cesse, sont conçus par les géomètres comme les résultantes formées suivant les lois du parallélogramme des forces. Si le lecteur n'est pas encore familier avec ce dont je parle, il trouvera du profit, je l'espère, à suivre les quelques explications que je vais donner.

Un trajet est une ligne dont on distingue le commencement et la fin. Deux trajets sont dits équivalents quand, partant du même point, ils aboutissent au même point. Ainsi, les deux trajets (fig. 3) ABCDE, AFGHE sont équivalents. Des trajets qui ne commencent pas au même point sont considérés comme équivalents, lorsque déplaçant l'un quelconque d'entre eux sans le tourner, mais en le maintenant toujours parallèle à sa position primitive, son point de départ coïncide avec celui de l'autre trajet et que les points d'arrivée coïncident également. Les trajets sont dits ajoutés géométriquement quand l'un commence où l'autre finit. Ainsi, le trajet AE est considéré comme la somme de AB + BC + CD + DE.

Dans le parallélogramme de la figure 4, la diagonale AC est considérée comme un trajet égal à la somme de AB + BC. Or, BC étant égal à AD, AC est la somme géométrique des deux trajets AB + AD.

Tout ceci est purement conventionnel et équivaut à dire qu'il nous plaît d'appeler équivalents ou additionnés les trajets qui sont dans les rapports sus-indiqués. La règle d'addition géométrique peut s'appliquer non-seulement à des trajets, mais à toute autre quantité

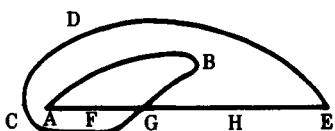


Fig. 3.

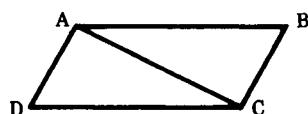


Fig. 4.

pouvant être représentée par des trajets. Or, comme un trajet est déterminé par la direction que prend, et par la distance que franchit un point qui le parcourt depuis son origine, il s'ensuit que tout ce qui, du commencement à la fin, est déterminé par des variations de direction et de grandeur, peut être représenté par une ligne. Par conséquent, les vitesses peuvent être représentées par des lignes, car elles n'ont que des directions et des degrés. Cela est encore vrai des accélérations ou changements de vitesse. Pour ce qui est des vitesses, cela est assez évident et le devient pour les accélérations, si l'on considère que les accélérations sont aux vitesses précisément ce que les vitesses sont aux positions, c'est-à-dire des états de changement de ces positions.

Ce qu'on nomme le parallélogramme des forces est simplement une règle pour composer des accélérations. La règle est de représenter les accélérations par des trajets. Toutefois les géomètres emploient le parallélogramme des forces non-seulement à composer différentes accélérations, mais à résoudre aussi une accélération en une somme de plusieurs. Soit AB (fig. 5) le trajet représentant une certaine accélération, c'est-à-dire une modification dans le mouvement d'un corps telle que, sous l'influence de ce changement, ce corps occuperait, au bout d'une seconde, une position séparée par la distance AB de la position qu'il eût occupée, si le mouvement se fût continué sans modification. On peut considérer cette accélération comme la somme des accélérations représentées par AC et CB. On peut aussi la considérer comme la somme des accélérations fort différentes représentées par AD et DB, alors que AD est presque l'opposé de AC. Il est clair qu'on peut, d'une infinité de manières, résoudre AB en une somme d'accélérations.

Après cette explication, qui, vu l'importance extraordinaire de la conception de force, n'aura point, je l'espère, épousé la patience du lecteur, nous sommes en état d'énoncer le grand fait qui résume

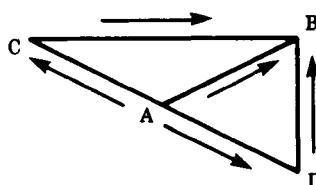


Fig. 5.

cette conception. Ce fait est que si l'on résout de la façon qui convient chacune des modifications actuelles de mouvement subies par les différentes particules d'un corps, chaque accélération composante est précisément telle que l'ordonne une loi naturelle d'après laquelle les corps, placés dans les positions relatives qu'occupe, au moment actuel, le corps en question, éprouvent toujours certaines accélérations de mouvement qui, composées par addition géométrique, forment l'accélération actuellement éprouvée par ce corps.

Tel est le fait certain contenu dans l'idée de force, et quiconque voudra prendre la peine de comprendre clairement ce qu'est ce fait comprendra parfaitement ce que c'est que la force. Doit-on dire que la force *est* une accélération de mouvement ou qu'elle *cause* l'accélération? C'est pure question de propriété de termes et qui ne touche pas plus au vrai sens de la pensée que ne le fait la différence entre l'expression française: Il fait froid, et l'expression anglaise correspondante: *It is cold.*<sup>1</sup> Il est cependant surprenant de voir quel désordre cette simple différence de mots a porté dans les esprits. Combien d'ouvrages sérieux parlent de la force comme d'une entité mystérieuse, ce qui semble seulement dénoter que l'auteur désespère d'acquérir jamais une claire notion de ce que le mot signifie.

Un livre récent et admiré sur la «*Mécanique analytique*» déclare qu'on saisit avec précision l'effet d'une force, mais que ce qu'est la force en elle-même, on ne le comprend pas. Ceci est simplement contradictoire; l'idée que le mot force fait naître dans l'esprit ne peut faire autre chose que d'affecter nos actions, et ces actions ne peuvent avoir de rapports avec la force que par l'intermédiaire de ses effets. Par conséquent, connaissant les effets de la force, on connaît tous les faits impliqués dans l'affirmation de l'existence d'une force, et il n'y a rien de plus à savoir. La vérité est qu'il circule la notion vague qu'une question peut renfermer quelque chose que l'esprit ne peut concevoir. Lorsqu'on a mis en face de l'absurdité d'une telle vue certains philosophes, ils ont imaginé une vaine distinction entre des conceptions positives et des conceptions négatives, dans un effort pour donner à leur idée vide une forme moins manifestement saugrenue. Le néant de cette tentative ressort suffisamment des considérations exposées quelques pages plus haut. A défaut de cela, le caractère captieux de la distinction imaginée doit avoir frappé tout esprit accoutumé à examiner des réalités.

1. Mot à mot: Il est froid.

## IV

Abordons maintenant l'objet de la logique, et examinons une conception d'une importance particulière pour cette science, la *notion de réalité*. Si par une idée claire on entendait une idée familière, aucune ne serait plus claire que celle-là. L'enfant s'en sert avec la plus entière confiance, et jamais il n'imagine qu'il ne la comprend pas. Toutefois, s'il s'agit de la clarté à son second degré, bien des hommes, même parmi ceux qui sont habitués à réfléchir, seraient embarrassés de donner une définition abstraite du réel. Cependant on peut arriver à formuler cette définition en considérant les différences entre le réel et son opposé le fictif. Une fiction est le produit d'une imagination humaine; elle a les caractères que lui impose la pensée qui la crée. Ce qui a des caractères indépendants de la pensée de tel ou tel homme est une réalité extérieure. Il y a cependant des phénomènes ayant pour théâtre l'esprit de l'homme et sa pensée pour élément, et qui sont en même temps réels, en ce sens qu'on les pense réellement. Mais si leurs caractères résultent de notre façon de penser, ils ne résultent pas de la façon dont on pense qu'ils sont. Ainsi, un rêve existe réellement comme phénomène intellectuel, pourvu qu'on l'ait rêvé. Qu'on ait rêvé de telle ou telle façon, cela ne dépend pas de ce qu'en peut penser qui que ce soit, mais est entièrement indépendant de toute opinion sur ce sujet. D'autre part, si l'on considère, non point le fait de rêver, mais la chose rêvée, le rêve ne possède certains caractères que parce que nous avons rêvé qu'il les possédait. Ainsi, le réel peut se définir: *ce dont les caractères ne dépendent pas de l'idée qu'on peut en avoir*.

Si satisfaisante, toutefois, qu'on puisse trouver cette définition, ce serait une grosse erreur de supposer qu'elle rend parfaitement claire l'idée de réalité. Appliquons donc ici les règles de notre méthode. Conformément à ces règles, la réalité, comme toute autre qualité, consiste dans les effets perceptibles particuliers produits par les choses qui la possèdent. Le seul effet des choses réelles est de produire la croyance, car toutes les sensations qu'elle excite apparaissent dans la conscience sous forme de croyance. La question se ramène donc à savoir ce qui distingue la croyance vraie ou croyance au réel, de la croyance fausse ou croyance à la fiction. Or, comme on l'a vu, les idées de vrai ou de faux, complètement développées, sont exclusivement du domaine de la méthode scientifique de fixer la croyance. Quiconque choisit arbitrairement les propositions qu'il adoptera

pour vraies ne saurait employer le mot *vérités* que pour proclamer sa détermination de s'en tenir à celles qu'il a choisies. Sans doute, la méthode de ténacité n'a jamais régné d'une manière exclusive, la raison est trop naturelle à l'homme. Mais la littérature des âges sombres nous offre quelques beaux spécimens dans ce genre.

Commentant un passage de quelque poète où il était dit que l'ellébore avait fait mourir Socrate, Scott Erigène apprend sans hésiter au lecteur qu'Elleborus et Socrates étaient deux illustres philosophes grecs et que Socrates, ayant été vaincu par l'autre en argumentation, avait pris la chose si à cœur, qu'il en était mort. Quelle idée de la vérité pouvait avoir un homme capable d'accepter et d'enseigner une opinion sans fondement même probable et adoptée absolument au hasard! Le véritable esprit socratique—car Socrate eût été, je crois, ravi d'être vaincu en arguments, parce qu'il eût ainsi appris quelque chose—contraste singulièrement avec l'idée naïve que s'en fait le commentateur pour qui la discussion ne semblerait avoir été qu'un tournoi.

Quand la philosophie commença à se réveiller de son long sommeil et avant qu'elle fût complètement dominée par la théologie, chaque maître semble avoir eu pour méthode de s'emparer de toute position philosophique qu'il trouvait inoccupée et qui lui semblait forte, de s'y retrancher et d'en sortir de temps en temps pour livrer bataille à ses rivaux. Aussi, des minces comptes rendus que nous avons de ces disputes, nous pouvons dégager une douzaine ou plus d'opinions professées en même temps par différents maîtres sur la question du réalisme et du nominalisme. Qu'on lise le début de l'*Historia calamitatum* d'Abélard, qui certes était philosophe autant que pas un de ses contemporains, on verra quel esprit batailleur y souffle. Pour lui, la vérité n'est qu'un château fort qui lui appartient en propre.

Quand prévalut la méthode d'autorité, vérité ne signifia guère que foi catholique. Tous les efforts des docteurs scolastiques tendent à concilier leur foi en Aristote avec leur foi en l'Eglise, et l'on peut lire leurs pesants in-folio, sans trouver un argument qui vise au delà de ce but. Fait remarquable, là où différents *credos* s'épanouissent côte à côte, les transfuges sont méprisés même du parti dont ils embrassent la foi, tant l'idée de loyauté féodale a remplacé l'ardeur pour la vérité.

Depuis le temps de Descartes, l'imperfection de la notion de vérité a été moins apparente. Cependant les esprits scientifiques sont

parfois frappés du fait que les philosophes ont moins travaillé à découvrir ce que sont les faits, qu'à chercher quelle croyance était mieux en harmonie avec leur système. Il est difficile de convaincre un adepte de la méthode *à priori* en produisant des faits. Mais montrez-lui que l'opinion qu'il soutient ne s'accorde pas avec ce qu'il a avancé autre part, et il se prêtera de bonne grâce à une rétractation. Les esprits de cette sorte ne semblent pas croire qu'une controverse doive jamais cesser. Ils semblent penser que l'opinion qui convient à une nature d'homme ne convient pas à une autre, et que par conséquent la croyance ne sera jamais fixée. En se contentant de fixer leurs opinions par une méthode qui peut conduire un autre homme à un résultat différent, ils trahissent la faiblesse de leur conception de la vérité.

Tous les adeptes de la science, au contraire, sont pleinement convaincus que les procédés de l'investigation, pourvu seulement qu'on la pousse assez loin, fourniront une solution certaine de toutes les questions auxquelles on les appliquera. Un savant peut chercher quelle est la vitesse de la lumière en étudiant les passages de Vénus et les aberrations des étoiles; un autre, en observant les oppositions de Mars et les éclipses des satellites de Jupiter; un troisième emploiera la méthode de Fizeau, un autre celle de Foucault; un autre encore fera usage des mouvements des courbes de Lissajous; d'autres enfin suivront diverses méthodes pour comparer les mesures obtenues au moyen de l'électricité statique et de l'électricité dynamique. Ils pourront d'abord obtenir des résultats différents; mais chacun d'eux perfectionnant sa méthode et ses procédés, les résultats convergeront constamment vers un point central prédestiné. Ainsi pour toutes les recherches scientifiques. Des esprits très-divers peuvent se lancer dans les recherches avec des vues tout opposées; mais, à mesure qu'avance l'investigation, une force extérieure à eux-mêmes les entraîne vers une seule et même conclusion. Cette activité de la pensée qui nous emporte, non pas où nous voulons, mais à un but fixé d'avance, semble être l'effet d'un arrêt du destin. Modification des points de vue, choix d'autres faits comme sujets d'étude, inclination naturelle de l'esprit même, rien ne permet d'échapper à l'opinion fatale.<sup>2</sup> Cette grande loi est contenue dans la notion de vérité et de

2. Par fatal, nous entendons simplement ce qui doit inévitablement arriver. C'est une superstition que de croire que certains événements peuvent être fatals, et c'est une erreur de supposer que le mot fatal ne puisse jamais être exempt d'une teinte de superstition. Nous mourrons tous, cela est fatal.

réalité. L'opinion prédestinée à réunir finalement tous les chercheurs est ce que nous appelons le vrai, et l'objet de cette opinion est le réel. C'est ainsi que j'expliquerais la réalité.

On peut objecter que cela est absolument contraire à la définition abstraite qui a été donnée de la réalité, puisqu'on fait ainsi dépendre les caractères du réel de ce qu'on en pense finalement. La réponse est d'abord que la réalité est indépendante, non pas de la pensée en général, mais seulement de ce que peut en penser un nombre limité d'hommes; ensuite, bien que l'objet de l'opinion définitive dépende de ce qu'est cette opinion, cependant la nature de cette opinion ne dépend pas de ce que pense tel ou tel homme. L'aberration des hommes peut retarder indéfiniment la fixation d'une opinion; on peut même concevoir que, grâce à elle, une proposition arbitraire soit universellement acceptée aussi longtemps que durera l'espèce humaine; cependant cela même ne changerait point la notion de la croyance qui pourrait résulter seulement d'une investigation poussée assez loin. Si, après l'extinction de notre race, il en apparaissait une autre douée de facultés et de tendances investigatrices, l'opinion vraie serait précisément celle qu'elle atteindrait finalement. «La vérité abattue se relèverait,» et l'opinion définitive qui résulterait de l'investigation ne dépend pas de ce que peut actuellement penser un être quelconque. Mais la réalité du réel dépend de ce fait que l'investigation, poursuivie assez longtemps, doit enfin conduire à y croire.

On peut demander ce que j'ai à dire de tous les menus faits de l'histoire oubliés pour jamais, des livres antiques perdus, des secrets ensevelis dans l'oubli:

Bien des perles rayonnant du plus pur éclat  
Reposent dans les abîmes sombres, inexplorés de l'Océan;  
Bien des fleurs naissent pour briller inaperçues  
Et jeter leur arôme au vent solitaire.

Tout cela n'existe-t-il point, pour être inévitablement hors de l'atteinte de notre science? L'univers mort—suivant la prédiction de quelques savants aventureux—and toute vie ayant cessé pour toujours, le choc des atomes ne continuerait-il plus parce qu'il n'y aurait plus d'intelligence pour le connaître? Bien que, quel que soit l'état de la science, aucun nombre ne puisse jamais être assez grand pour exprimer le rapport entre le total des faits connus et celui des faits inconnus, je crois cependant antiphilosophique de supposer que, étant donnée une grande question quelconque, offrant un sens clair,

l'investigation n'en donnerait pas la solution si on la poussait assez avant. Qui eût dit, il y a quelques années, que nous saurions un jour la composition de ces étoiles dont la lumière a mis, pour arriver jusqu'à nous, plus longtemps que n'a encore duré l'espèce humaine. Peut-on dire avec certitude ce que l'humanité ignorera dans quelques centaines d'années? Peut-on deviner les résultats de recherches scientifiques poursuivies pendant dix mille ans avec la même activité que depuis les cent dernières années. Et si on les continuait pendant un million, un milliard d'années, pendant nombre de siècles aussi grand qu'on voudra, est-il possible de dire qu'il y ait une question qu'on ne résoudrait pas à la fin.

Mais, peut-on objecter, pourquoi attacher tant d'importance à ces aperçus lointains, surtout quand vous avez pour principe que, seules, les distinctions pratiques signifient quelque chose? Soit, j'avoue que cela fait peu de différence de dire ou non qu'une pierre au fond de l'océan, dans une complète obscurité, est brillante. Encore vaut-il mieux dire qu'il est probable que cela ne fait pas de différence, car il faut toujours se rappeler que cette pierre peut être péchée demain. Mais affirmer qu'il y a des perles au fond des mers, des fleurs dans les solitudes vierges, etc., ce sont là des propositions qui, comme ce que nous disions d'un diamant pouvant être dur alors qu'il n'est pas serré, touchent beaucoup plus aux formes du langage qu'au sens des idées.

Il me semble qu'en faisant application de notre règle, nous sommes arrivés à saisir si clairement ce que nous entendons par réalité, et le fait qui est la base de cette idée, que ce serait peut-être, de notre part, une prétention moins présomptueuse que singulière d'offrir une théorie métaphysique de l'existence, acceptable pour tous ceux qui pratiquent la méthode scientifique de fixer la croyance. Toutefois, la métaphysique étant chose plus curieuse qu'utile, et dont la connaissance, comme celle d'un récif submergé, sert surtout à nous mettre en état de l'éviter, je n'imposerai plus d'ontologie au lecteur.

## [*Ferrero's Esposizione del metodo dei minimi quadrati*]

P 111: American Journal of Mathematics I  
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Recent discussions in this country, of the literature of the method of Least Squares, have passed by without mention the views of the accomplished chief of the geodetical division of the Italian Survey, as set forth in the work above cited, which was first published, in part, in 1871. The subject is here, for the first time, in my opinion, set upon its true and simple basis; at all events the view here taken is far more worthy of attention than most of the proposed proofs of the method.

Lieut. Col. Ferrero begins by considering the principles of the arithmetical mean. A quantity having been directly observed, a number of times, independently, and under like circumstances, the value which might be inferred from the observations is, in the first place, a symmetrical function of the observed quantities; for, if the observations are independent, the order of their occurrences is of no consequence, and the circumstances under which they are taken, differ in no assignable respect, except that of being taken at different times. In the second place, the value inferred must be such a function of the values observed, that when the latter are all equal, the former reduces to this common value. The author calls functions having these two properties, (1st, that of being symmetrical with respect to all the variables, and 2d, that of reducing to the common value of the variables when these are all equal,) *means*. There is a whole class of functions of this sort, such as the arithmetic mean, the geometrical mean, the arithmetic-geometrical mean of Gauss, the quadratic mean,<sup>1</sup> and many others instanced in the text. It is shown, without

1. This seems the appropriate name for  $\sqrt{\frac{[x^2]}{n}}$ .

difficulty, that these means are continuous functions, and that their value is intermediate between the extreme values of the different variables, when the latter do not differ greatly.

Let  $o', o'', o''',$  etc. denote the values given by the observations. Let  $n$  denote the number of the observations; let  $p$  denote the arithmetical mean; and let  $x', x'', x''',$  etc. denote the excess of the observed values over the arithmetical mean. Then write

$$V = f(o', o'', o''', \text{etc.})$$

for any mean of the observations. Develop this function according to powers of  $x', x'', x''',$  etc. We have

$$\begin{aligned} V &= f(p + x', p + x'', p + x''', \text{etc.}) \\ &= f(p, p, p, \text{etc.}) + \frac{dV}{dp} (x' + x'' + x''' + \text{etc.}) + \Delta; \end{aligned}$$

where  $\Delta$  denotes the terms of higher orders.

Since  $x' + x'' + x''' + \text{etc.} = 0,$

and  $f(p, p, p, \text{etc.}) = p,$

this reduces to

$$V = p + \Delta.$$

In considering the value of  $\Delta$ , we may limit ourselves to terms of the second order. As the partial differentials of any species and order, relatively to  $o', o'', o''',$  etc. all become equal when  $x', x'', x''',$  etc. vanish, we may write

$$\frac{d^2V}{do'^2} = \frac{d^2V}{do''^2} = \frac{d^2V}{do'''^2} = \text{etc.} = \beta$$

$$\frac{d^2V}{do'. do''} = \frac{d^2V}{do''. do'''} = \text{etc.} = \gamma$$

then

$$\begin{aligned} \Delta &= \frac{1}{2} \beta (x'^2 + x''^2 + x'''^2 + \text{etc.}) \\ &\quad + \gamma (x'x'' + x'x''' + \text{etc.}). \end{aligned}$$

But the square of  $[x] = 0$ , gives

$$\Sigma xx' = -\frac{1}{2}[x^2],$$

so that

$$\Delta = \frac{\beta - \gamma}{2}[x^2] = k \frac{[x^2]}{n},$$

where  $k$  is a quantity which does not increase indefinitely with  $n$ .

Now, when the observations are good,  $\frac{[x^2]}{n}$  is not large, and, there-

fore, in such a case no mean will differ very much from the arithmetical mean. The latter, being the simplest to deal with, may therefore be used without great disadvantage. Such is, according to Colonel Ferrero, the utmost defence of the principle which can be made to cover all the cases in which it is usual to employ the method; and all further defence of it is more or less limited in its application.

In very many cases, however, it is easy to see that either in regard to the quantity directly observed, or in regard to some function of it, the zero of the scale of measurement, and the unit of the same scale, are both arbitrary. For instance, in photometric observations, this is true of the logarithm of the light. In such cases, considering such function to be the observed quantity, we have there two principles, first proposed, in connection with a really superfluous third one, by Schiaparelli.

1st. The mean to be adopted must be such that if each observed value is multiplied by any constant, the result is increased in the same ratio.

2d. The mean to be adopted must be one which is increased by a constant  $o$ , when each observed value is increased by the same constant.

Our author's treatment of these principles is exceedingly neat. Using the same notation as above, write

$$V = p + A_2 + A_3 \dots + A_n \dots$$

where  $A_n$  is the sum of the terms of the order  $n$  in  $x'$ ,  $x''$ ,  $x'''$ , etc. The general term  $A_n$  is, therefore, of the form  $A_n = \alpha \Sigma x^n$

$+ \beta \Sigma x'^{n-1}x'' + \gamma \Sigma x'^{n-2}x''^2 \dots + \zeta \Sigma x'^r x''^s x'''^t \dots$  where  $\Sigma$  expresses the symmetrical sum of similar terms. In the general term  $r+s+t+\text{etc.} = n$ . Since  $\zeta$  is evidently a function of  $p$ , we may put  $\zeta = \varphi(p)$ , and it remains to find the form of this function. Multiplying every  $o$  by  $c$ ,  $p$  is changed to  $cp$ ,  $x$  to  $cx$ , and the general term  $\zeta \Sigma x'^r x''^s x'''^t \text{ etc.} = \varphi(p) \Sigma x'^r x''^s x'''^t \text{ etc.}$  is changed to  $\varphi(cp)c^n \Sigma x'^r x''^s x'''^t \text{ etc.}$  Since, therefore,  $V$  is changed to  $cV$ , we have  $\varphi(cp)c^n = \varphi(p)c$ . Putting  $p = 1$ ,  $\varphi(c) = \frac{\varphi(1)}{c^{n-1}}$ . Denoting this numerator by  $\xi_1$ , the general term becomes

$$A_n = \frac{1}{p^{n-1}} [\alpha_1 \Sigma x^n + \dots + \xi_1 \Sigma x'^r x''^s x'''^t \dots + \dots],$$

where  $\alpha$ ,  $\xi$ , etc., are numerical coëfficients independent of  $p$ . From this circumstance it follows that the quantity in square brackets, which may be called  $A'_n$ , does not change when the same constant quantity  $k$  is added to all the observed quantities  $o'$ ,  $o''$ ,  $o'''$ , etc.; for such an addition only increases  $p$  by this same constant, and leaves  $x'$ ,  $x''$ ,  $x'''$ , etc., unchanged. Thus the mean in question, which may now be written

$$V = p + \frac{A'_2}{p} + \frac{A'_3}{p^2} + \text{etc.},$$

becomes, in consequence of such an addition,

$$V_k = p + k + \frac{A'_2}{p+k} + \frac{A'_3}{(p+k)^2} + \text{etc.}$$

But by principle No. 2, it becomes,

$$V_k = p + k + \frac{A'_2}{p} + \frac{A'_3}{p^2} = \text{etc.}$$

So that,  $A'_2 = A'_3 = \text{etc.} = 0$ , and we have

$$V = p,$$

or the arithmetical mean is the only one which conforms to the given conditions.

Another still more special case, is that contemplated by the demonstrations of Laplace, Poisson, Hagen, Crofton, etc. It is treated by our author, but need not be considered in this notice.

It may be of interest to see how Colonel Ferrero is able, without basing least squares expressly upon the theory of probabilities, to derive the formula for finding mean error. Using always the same notation, he terms

$$m = \sqrt{\frac{[x^2]}{n}}$$

the *mean residual* of the observations.

Suppose, then, that there be an indefinitely great *series of series* of observations of the same quantity, each lesser series consisting of  $n$  observations, and each having the same mean residual. Then, there being an infinite number of such series, the mean of their mean results may be taken as the true value, by definition. For the ultimate result of indefinitely continued observation is all that we aim at in sciences of observation. Then the number of the lesser series being  $q$ , the result will be

$$V = \frac{[p]}{q}.$$

Adopt the notation

$$\delta = p - V \quad \delta_1 = p_1 - V \quad \delta_2 = p_2 - V, \text{ etc.,}$$

then  $\delta, \delta_1, \delta_2$ , etc., are the true errors of  $p, p_1, p_2$ , etc. Let  $y'_0, y''_0, y'''_0$ , etc. be the true errors of the first series of observations,  $y'_1, y''_1, y'''_1$ , etc. those of the second series, and so for the others. We have, then,  $y = o - V = o - p + \delta = x + \delta$ .

Squaring and summing for the  $nq$  values of  $y$ , we have

$$\Sigma y^2 = \Sigma x^2 + \Sigma \delta^2 + 2\Sigma x \Sigma \delta$$

or, since

$$\Sigma x = 0, \text{ and } \Sigma \delta = 0,$$

$$\Sigma y^2 = \Sigma x^2 + \Sigma \delta^2.$$

Now if  $\eta$  be the quadratic mean of the error of  $p$ , we have  $\Sigma\delta^2 = nq\eta^2$ , and

$$\Sigma y^2 = nqm^2 + nq\eta^2,$$

or the mean error  $\mu$  of an observation is given by

$$\mu^2 = \frac{\Sigma y^2}{nq} = m^2 + \eta^2.$$

But it is easily shown (from the equality of positive and negative errors) that

$$\eta^2 = \frac{\mu^2}{n}$$

whence

$$\mu = \sqrt{\frac{[x^2]}{n - 1}}.$$

With regard to the mode of passing from the principle of the arithmetical mean to the general method of least squares, the best way seems to be first to prove that the solution of the equations

$$a_1x = n_1$$

$$a_2x = n_2$$

etc.,

is  $x = \frac{[an]}{[a^2]}$ . This is easy, after the rule for the error of a mean is established. Then, having given the equations

$$a_1x + b_1y + c_1z + \text{etc.} = n_1$$

$$a_2x + b_2y + c_2z + \text{etc.} = n_2;$$

first, consider these as similar to the equations just given; thus,

$$a_1x = n_1 - b_1y - c_1z - \text{etc.},$$

$$a_2x = n_2 - b_2y - c_2z - \text{etc.},$$

etc.,

whence we obtain the first normal equation,

$$x = \frac{[an_1] - [ab]y - [ac]z - \text{etc.}}{[a^2]}$$

and the others in a similar way.

The treatise of Colonel Ferrero may be recommended to those desirous of having a thorough practical acquaintance with the method, as decidedly the best and clearest on the subject.

## *Photometric Researches: Made in the Years 1872-1875*

*P 118: Leipzig;  
Wilhelm Engelmann, 1878*

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### CHAPTER I. THE SENSATION OF LIGHT

When a point is emitting unpolarized and homogeneous undulations equally in all directions, its state may be defined by two numbers; as, for instance by the wave-length of the undulations and their amplitude at a certain distance from the point. If the light is not homogeneous, indefinitely more numbers will be required to define it. But when a point upon the retina is illuminated, just three numbers are in every case requisite to define the sensation produced. In other words, light is a triple sensation.

Since we have not yet succeeded in getting a clear general conception of any relation between different sensations, except that of more or less, it follows that when we have said that the sensation of light has three elements (arbitrarily taken as primary) we have gone as far toward describing that sensation as the present state of our ideas enables us to do.

When two lights which present precisely the same appearance separately, fall at once on the same point of the retina, without interference, the light is said to be doubled. From this convention we obviously deduce the principle that the brightness of two lights which in every other respect are alike are proportional to their energies. But this does not necessarily follow with regard to light of different wave-lengths.

Suppose that there are three sorts of measurement which we can apply to light, by the first of which we ascertain a quantity called its

$X$ , by the second a quantity called its  $Y$ , and by the third a quantity called its  $Z$ , so that if we know that

$$X = a \quad Y = b \quad Z = c,$$

we have completely determined the sensation. Then it will not be necessary to measure  $X$ ,  $Y$ , and  $Z$  directly, but we may measure any three independent functions of these three quantities and we shall thus have three independent equations which will equally serve to define the sensation. There are therefore an infinite variety of sets of three quantities which will serve to define the sensation of light.

Light considered purely as something in the external world may be called *noumenal light*. Light considered as an appearance, and as a function of the sensation, such that it is measured by the convention just mentioned, may be termed *phenomenal light*. Photometry generally concerns phenomenal light; and in these researches, I shall nowhere touch upon the question of how the noumenal light of stars is constituted, (as to the difference of their spectra, for example), but shall confine myself to considering how it appears.

If the light  $A$  precisely matches the light  $A'$  in appearance, and the light  $B$  precisely matches the light  $B'$ , then the mixture of  $A$  and  $B$  will precisely match the mixture  $A'$  and  $B'$ . This is by no means a self-evident proposition, for as two lights may have precisely the same appearance and yet a totally different noumenal constitution, it might very well be that the effect of mixing should depend on something not represented in the phenomenal light.

This fact, that the appearance of a mixture of lights is determined entirely by the appearance of its constituents, renders it convenient to denote an appearance of light by an expression of the form

$$xX + yY + zZ$$

where  $x$ ,  $y$ , and  $z$  are variable numbers, and  $X$ ,  $Y$ ,  $Z$ , are three different fixed lights arbitrarily chosen, subject to the condition that no one shall be a linear function of the other two. It is found possible to choose  $X$ ,  $Y$ , and  $Z$  so that  $x$ ,  $y$ , and  $z$  need never be negative, and so that nearly every positive system of values shall represent some sensation which may actually be experienced. When these conditions

are satisfied,  $X$ ,  $Y$ , and  $Z$  are a crimson red, an emerald green, and a blue or violet. We may denote these lights by  $R$ ,  $G$ , and  $B$ , respectively.

Since phenomenal light is a triple quantity, the points of a solid are just adequate to represent it. Suppose a system of Cartesian coördinates, and let  $R$ ,  $G$ , and  $B$  be measured along the three axes. Then we shall have a triangular pyramid having darkness for its apex and every point within it representing a light. Then to find the point which represents the light resulting from the mixture of two lights, draw lines from the points representing these lights to the apex, complete the parallelogram and the fourth angle will be the point sought. If the pyramid of light be cut by a plane, the points upon the triangular section so obtained will be adequate to represent the colour without the intensity of every kind of light. We may then imagine each point to be weighted proportionally to the brightness of the light; and in this case the centre of gravity of two points represents the colour of the mixture of the two lights represented by the two weighted points, while the brightness of the mixture is equal to the sum of the two weights. Since the inclinations of the three axes of our system of Cartesian coördinates as well as the units of length are entirely arbitrary, we have precisely the degree of indeterminacy which is requisite to enable us to represent upon the plane section made as above, any three lights by a triangle of any size and shape. We may therefore begin by assuming any three points upon a plane to represent any three lights of different colours and then fill in the other colours according to the rule of centres of gravity. This is called Newton's Diagram.

If the colours of the spectrum be laid down on such a diagram, then, as Maxwell has shown,<sup>1</sup> that whole portion of the spectrum between  $C$  and a point considerably beyond  $F$ , appears as two straight lines. These lines meet near  $E$ ; and there the corner is a little rounded off, but not more I suspect than may be accounted for by the impurity of the spectrum. Towards the violet the colours are all crowded together in an irregular manner, but this is to be accounted for, no doubt, by the known fluorescence of the retina. At the extreme red end there is also a departure from the straight line, which is such as might be produced by stray light which is difficult to keep out at this end of the spectrum. It seems, therefore, that light is

1. *Philosophical Transactions* 150 (1861): 57-84.

composed of three elementary sensations of red, green, and blue. Upon examining Maxwell's experiments upon color I was led to conclude that there was a relation of a very singular nature between the wave-length and the apparent color of any part of the spectrum. Let  $\lambda_x$ ,  $\lambda_y$ ,  $\lambda_z$ , be the wave-lengths of three points of the spectrum which are all on the same side of the elementary green. If, then,  $C_x$ ,  $C_y$ ,  $C_z$ , are the apparent lights of these three parts and if

$$C_y = XC_x + ZC_z$$

I find from Maxwell's experiments that

$$\frac{X}{Z} = \frac{\lambda_y - \lambda_z}{\lambda_x - \lambda_y}.$$

I announced this fact to the American Academy of Arts and Sciences in April 1872, and afterwards to the Philosophical Society of Washington. The same thing was afterwards independently announced by W. von Bezold<sup>2</sup> and proved (in a less satisfactory manner, as it seems to me) by Helmholtz's experiments.<sup>3</sup> The following is the comparison of the law with Maxwell's Experiments. The quantities compared are the percentages of certain arbitrarily taken colors in the various colors of the spectrum.

#### OBSERVER K

#### RED TO GREEN

Point of spectrum on Maxwell's Scale	Per cent of blue		Per cent of green		Per cent of red	
	Obs.	Calc.	Obs.	Calc.	Obs.	Calc.
24	00	00	00	-01	100	101
28	-01	00	24	+25	77	75
32	-00	-01	51	51	49	50
36	-02	-01	73	72	29	29
40	-01	-01	91	92	10	09

2. "Ueber das Gesetz der Farbenmischung," *Annalen der Physik und Chemie* 150 (1873): 71-93 and 221-47.

3. Helmholtz, *Archiv für Anatomie und Physiologie*, 1852, p. 461.

## GREEN TO BLUE

Point of spectrum on Maxwell's Scale	Per cent of blue	Per cent of green	Per cent of red			
	Obs.	Calc.	Obs.	Calc.	Obs.	Calc.
48	11	12	97	96	-08	-08
52	38	37	69	70	-07	-07
56	63	62	43	44	-06	-06
60	84	84	21	20	-05	-04
64	100	100	04	04	-04	-04

## OBSERVER J

## RED TO GREEN

24	00	-01	00	+01	100	100
28	-03	-02	26	27	77	75
32	-02	-02	56	55	43	47
36	-01	-01	78	77	23	24
40	-02	-02	95	97	07	05

## GREEN TO BLUE

48	29	31	75	74	-04	-05
52	60	59	44	45	-04	-04
56	93	84	12	20	-05	-04
60	101	107	02	-03	-03	-04

The agreement is all that could be desired. Without regarding the wave-lengths but simply the fact that the loci of the two parts of the spectrum are straight lines on Newton's diagram, we may calculate the constitution of elementary green and we find it to be, in terms of the colors of (24) and (44) of Maxwell's Scale

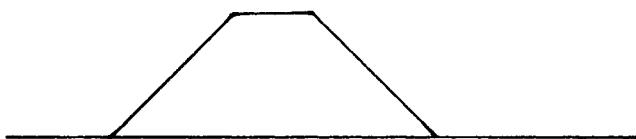
$$\begin{array}{ll} \text{Observer K.} & -0.09(24) + 1.09(44) \\ \text{Observer J.} & -0.04(24) + 1.04(44) \end{array}$$

Having thus obtained the constituents of pure green we may by the law of wave-lengths get *two* values of the wave-length of it, one derived from the less refrangible and the other from the more refrangible part of the spectrum. Thus I find

*Wave-length of pure green*

	From red end	From blue end
K	530	521
J	534	514

It appears from these calculations that the law of the variation of the green constituent of the sensation is such that if we represent the wave-length by the abscissas of rectangular coördinates, the ratio of the intensity of the green sensation to the whole light of any part of the spectrum being the ordinate, we have a curve like this:



Now we cannot suppose the real curve to consist of broken lines and we are therefore led to ask what curve resembles this. The curve which exhibits the intensity of resonance of a vibrating body for sounds which differ more or less from its natural pitch has a strong likeness to this; and the likeness suggests an explanation of the phænomena which falls in very well with current opinions about colour. In the present state of our knowledge of the action of the nerves, it would perhaps be useless to push our speculations on this subject any further.

So far we have adopted an arbitrary definition of the intensity of light which has no applicability except to lights differing in no respect except in intensity. We have now to consider another mode of measuring the intensity of all sensations which has much higher pretensions to real truth.

If a certain force  $x$  applied to irritate a nerve produces a certain sensation, there is perhaps no addition to it  $\delta x$  so slight that the sensation produced by  $x + \delta x$  will not in some slight majority of trials be pronounced more intense than that produced by  $x$ . Nevertheless, a value can be assigned such that if  $\delta x$  is much larger than that, the sensation produced by  $x + \delta x$  will be decidedly more intense than that produced by  $x$ , while if  $\delta x$  is much smaller than that, we shall feel uncertain which is the more intense sensation. Supposing  $\delta x$  to

have that value, we find that  $\frac{\delta x}{x}$  is a constant. Assuming that the addition to the sensation is the same and denoting this by  $\delta y$ , we have

$$\delta y = \frac{\delta x}{x} \text{ or } y = A + B \log x.$$

The reasoning involved in this procedure would be open to criticism if the result were not fully confirmed in various ways. But it is so confirmed; and the (at least, approximate) truth of *Fechner's psychophysical law* is now fully admitted, that as the *vis viva* of the exciting force increases in geometrical ratio the sensation increases in arithmetical ratio.

Various circumstances interfere with the exactitude of this formula in the case of light; but still it is approximately true and this is why we do well to fix our scale of magnitudes of stars so that equal increments of the numerical magnitude correspond to equal increments in the logarithm of the light.

Our sensibility to the three elementary colours is different. That is to say, the brighter a light is the more red and the less blue it appears. Consequently, observations on the colors of stars present very little agreement. I have given at the end of this paper a comparative catalogue showing the colors of stars according to Secchi at Rome, Schmidt at Athens, Sestini at Milan, and myself at different places. Secchi observed spectroscopically and he divides the stars into three classes, which I have indicated by I, II, III. I signifies that the star is a bluish one of the type of  $\alpha$  Lyrae; II signifies that the star is a yellow one of the type of Capella; and III that it is a reddish one of the type of  $\alpha$  Orionis. Schmidt has a numerical scale, the higher the number the warmer being the tone of the light. My own observations were partly made with the colorimeter of Zöllner's astrophotometer and these observations are italicized. The others are mere estimates. The former colors are all reduced to my day-time judgment of the colors. That is to say having matched the real star with the photometer star, I observed in the day time what the color of the photometer star was. This is why I always make stars more blue and less red than other observers.

The different sensibility of the eye for the three primary colors also causes discrepancies in the observations of the relative brightness of different colored stars, made by different observers or under different atmospheric circumstances or with telescopes of different

power. For if a red and a blue light which appear equally bright are both doubled in brightness according to the definition on page 382, they will no longer appear equally bright, but the red will appear the brighter.

Abbreviations of the Names of Astronomers etc.,  
*used in this Book*

A.	Argelander.	P.	Piazzi; Peirce.
B.	Bode.	II.	Ptolemy.
ঃ.	Behrmann.	Σ.	F. G. W. Struve.
DM.	Durchmusterung.	σ.	Otto Struve.
F.	Flamsteed.	S.	Sūfi.
H.	Hevelius.	ং.	Seidel.
ং.	Sir Wm. Herschel.	Sj.	Schjellerup.
h.	Sir John Herschel.	Se.	Secchi.
ঃ.	Heis.	T.	Tycho Brahe.
LC.	Lacaille (Henderson's Edition).	U.	Ulugh Beg.
LL.	Lalande (Baily's Edition).	Z.	Zöllner.
M.	Messier.		

CHAPTER III. ORIGINAL OBSERVATIONS

My observations have been made with a Zöllner's Astrophotometer (see Plate II). Though the principle of this instrument is well-known, I may remind the reader that an artificial star is thrown into the field of a telescope, and that its brightness is reduced by the rotation of a Nicol prism, until it matches, in brightness, any real star which is in the field at the same time. The Nicol prism being furnished with a graduated circle, the ratio of the reduction of the light is calculated from the reading. A third Nicol with an interposed quarz plate, cut perpendicular to its axis, makes it possible to alter the color of the artificial star, and this third Nicol is also furnished with a graduated circle. I suppose it was Zöllner's intention that the observer should match the real star in color as well as in brightness, and so determine two of the three constants which are necessary to define any appearance of light. My experience, however, soon led me to attach a clamp to the "color-circle," and to keep it fixed in position, except when I desired to make a special observation upon the color

of a star. For the color of a Kerosene lamp varies very much, even during one evening, so that the change of brightness of the artificial star produced by turning the color-circle was by no means constant. Moreover, the color-circle was only graduated to every  $5^{\circ}$  and an alteration of its position equal to a tenth of this interval would make a difference of a twentieth of a magnitude, so that an error of reading might have a decided effect. Another important reason for not using the color-circle in measures of brightness was that, owing to the lens which serves as objective to the artificial star not being achromatic, it had to be focussed every time the color-circle was moved, and this focussing was not only difficult, but also, as I shall explain presently, tended to introduce another sort of error.

There is not, as is generally supposed, any great difficulty in comparing two lights of different colors and deciding which is the brighter, and the impossibility of accurately comparing the brightness of two lamps or two stars does not lie in the difficulty of the immediate observation. It lies chiefly in the fact that a change in the objective light produces less change in the sensation of blue than in those of red and green, so that the warmer colored stars appear relatively brighter on fine, clear nights. If we could keep our artificial star constant in color and could easily modify the color in a known way, all difficulty in comparing stars of different color could be overcome. But since this cannot be done, and since the error in the immediate comparison of light of different colors is not great, I should prefer, in constructing a photometer, to leave the artificial star fixed in brightness and only alter the light of the real star. In that way, we should compare the stars of different colors at a fixed relation to one another. Further on, I shall mention another reason for preferring such an arrangement.

I have specially observed the color of about 150 stars. In the comparative color-catalogue which is appended to this memoir, it will be observed that my italicized observations, which are those made with the instrument, never make a star red, or even orange,—never more than decidedly yellow,—while I frequently call stars blue which other observers consider to be yellow. This is because I have attached names to the colors by lighting the photometer lamp in the daytime, setting the color-circle to the various readings and then assigning such names to the colors as seemed appropriate. Owing to the relative faintness of the light in the daytime, all the colors appear much bluer.

The quartz plate which produced the color of the photometer star was measured with an accurate plate gauge (tested in the office of weights and measures), and was found to have thickness of from 0.1960 to 0.1961 inches. The thickness was also determined by the amount of deviation of the plane of polarization of the light formed by impregnating the wick of an alcohol lamp with salt. The thickness calculated from this experiment was 0.1953 inches. The following shows the comparative thickness of the quartz plates used by three observers.

mm.	mm.	mm.
Zöllner 5.150	Rosén 4.966	Peirce 4.976

I observed the relative brightness of the photometer star in different positions of the color-circle and found it well represented by the empirical formula

$$m = -4.374 + .095288 \psi - .0004526 \psi^2$$

where  $\psi$  is the reading of the color-circle and  $m$  is the magnitude on a scale for which  $p = 2.25$ . The following table shows the comparison of the formula with the observations.

#### MAGNITUDE

Color-circle	Color	Observed	Calculated Obs. – Calc.		
			m.	m.	m.
15°	Grey	−3.01	−3.04	+.04	
30	Intense blue green	−1.87	−1.92	+.05	
45	Decidedly greenish white	−0.91	−1.10	+.09	
60	Straw	−0.33	−0.29	−.04	
75	Decided yellow	+0.12	+0.22	−.10	
90	Intense yellow	+0.51	+0.53	−.02	
105	Yellow orange	+0.66	+0.65	+.01	
120	Very red orange	+0.57	+0.55	+.02	
135	Scarlet	+0.30	+0.24	+.06	
150	Cherry	−0.19	−0.27	+.08	
165	Crimson	−1.07	−0.97	−.10	
180	Very red purple	−1.94	−1.89	−.05	
195	Grey	−3.01	−3.00	−.01	

These numbers were obtained by the comparison of one artificial star of fixed color with another whose color was varied. They suffice of themselves to dispel the idea, if anybody still has it, that the observation of two lights of different colors, to say which is the brighter, is devoid of all certainty, because they exhibit so much regularity and concordance among themselves. My color equation differs perceptibly but not greatly from those of Zöllner and Rosén.

The flame which produces the artificial star is that of a kerosene lamp. It was necessary to take the greatest pains to keep this clean and well filled, and to have the wick trimmed with perfect neatness and parallelism to the top of the wick-tube, except at the corners, which were cut off, to prevent the expansion of the flame to each side. This lamp ought to have been made with great nicety, but it was, in fact, a poor affair, not nearly so well constructed as those we use in our houses. The height of the flame could be kept constant by means of a little sight attached, but when the lamp worked well this did not vary during the evening. The lamp was prevented from being blown by the wind by a shield which would have shut off the draught, if it had not been for some tubes by which the air was conveyed to the lamp. As this came from the maker, it was badly constructed, the tubes not supplying enough air nor at the right points. I soon had it altered, after which it gave no trouble, except when there was a considerable breeze, or when the weather was hot. In the summer of 1872 in Washington when the temperature was generally about 90° F. in the observatory, the lamp did not behave well.<sup>4</sup> The accompanying table shows the fluctuations of the light on the adopted scale of magnitudes.

Table showing the magnitude of the Photometer Star, with no cap, and pinhole 5, and with the circle at 90°, diminished by 4<sup>m</sup>.10

No. of set	Date	Value m.	Remarks
1	1872 March 15	1.04	<i>Cambridge.</i>
2	" " 16	.93	Obs. thinks lamp may have altered during set.
3	" " 18	.92	Bright moon. Thin, light clouds.

4. The observations were also made difficult, that season, by the swarms of insects.

No. of set	Date	Value m.	Remarks
4	1872 March 21	1.00	Moon very near. Very windy. Seeing very bad.
5	" " "	1.14	
6	" " 22	0.74	Very bright moonlight, windy and hazy.
7	May 4	-0.03	
8	" " "	+.08	
9	" " 5	.37	Sky slightly hazy.
10	" " 21	.11	Full moon; sky very bright and clear.
11	" " 26	-.54	Very fine night.
12	" " 28	.74	Clear still night; no moon. Interrupted by clouds.
13	" " 30	.47	Night clear, no moon. Stars twinkling, somewhat windy, clouds.
14	" " 31	.37	Night clear, no moon, and still. Later, windy.
15	June 2	.74	Still, cloudless, no moon. Stopped by haze.
16	" " 10	.38	Moon about 3 days old. Gentle breeze, light clouds.
17	" " 11	.38	
18	" " 12	.10	Clear. Moon 6 days old. Gentle breeze.
19	" " "	.29	
20	" " 13	.01	Moon quartered. Sky moderately clear. Windy.
21	" " "	.37	Windy; light clouds illuminated by moon. Stopped by haze.
22	" " 18	.26	Moon nearly full, pretty windy. Light clouds repeatedly interrupted.
23	" " 19	.19	Pretty calm, moon full. Light clouds.
24	" " 29	.37	
25	" " 30	.13	Clear, calm, moonless.
26	" " "	.16	Superb night.
27	July 1	.11	Pretty clear, gentle wind, no moon. Later, clouds.
28	" " 2	.16	Nearly clear, breeze, no moon.
29	" " 8	-.12	Nearly clear, moon 2 days old.
30	" " 19	.18	Gentle wind, full moon. Haze near horizon.
31	" " 20	.10	Very clear, full moon, moderate breeze.
32	" " 28	.24	Strong wind blowing lamp, no moon.
33	" " 30	.04	Moonless, gentle wind, stopped by clouds.
34	" " 31	.22	
35	Aug. 6	.18	Lamp in some way out of order. Clear, pretty strong wind. No moon. Affected by irregularities of lamp or of atmosphere.
36	" " "	.06	
37	" " 7	.14	Clear; moon about 2 days old; pretty strong wind.
38	" " "	-.15	

No. of set	Date		Value m.	Remarks
39	1872	Aug. 8	.29	Clear; moderately bright aurora; pretty strong wind; moon about 3 days old.
40	"	" 7	.14	
41	"	" 9	-.08	Clear, moon about 5 days old; brisk wind blowing lamp badly.
42	"	" 17	-.01	Clear, nearly calm, moon nearly full. Interrupted by clouds.
43	"	" 18	-.15	Clear, slight haze, very calm, v. slight breeze.
44	"	Sep. 7	-.42	Moderately clear, v. hazy, no moon.
45	"	" 14	-.25	Clear and moderate wind. Moon about 11 days old.
46	"	" 16	-.30	Clear, calm, and full moon.
47	"	" 19	-.40	Clear, moon about 17 days old, strong wind.
90	"	Oct. 15	.18	Full moon, very windy.
48	1873	Feb. 28	-.37	Clear, hazy and windy.
49	"	March 6	-.02	Clear, gentle wind. Moon about 8 days old.
50	"	" 7	.32	Rather hazy, light wind, moon about 9 d. old. Haze increased, then decreased.
51	"	" 9	.26	Clear. Moon-light. Stopped by increasing wind.
52	"	" 10	-.08	Bright moonlight, clear, pretty windy; lamp went out once; light clouds close to place of observation.
53	"	" "	.27	
54	"	" 17	.22	Clear, moonless, moon rose 3d round; suspicion of clouds.
55	"	" 22	.31	Clear, moonless, moderate wind; seeing bad, stars varying.
56	"	May 26	.22	Night still, moonless, clear.
57	"	" 31	.18	Clear, light west wind, moon about 3 d. old.
58	"	June 1	.26	Night clear, pretty strong wind, moon about 5 d. old.
59	"	" 7	.14	Clear, calm, moonlight night. Clouds came up.
60	"	" 8	.19	Moderate wind, moonlight. Images bad. Clouds in the neighborhood.
61	"	" 13	.45	Clear, moonless night, rather strong wind.
62	"	" 18	.74	
63	"	" 20	.45	Pretty hard northwind.
64	"	" 21	1.17	Light haze.
65	"	" 25	r	Cool, clear, moderate wind, stars are steady.
66	"	"	1.03	
67	"	July 1	1.20	Calm, very clear fine night.
68	"	Sep. 6	.40	<i>Hoosac mountain.</i> Remarkably clear and calm: moon nearly full.

No. of set	Date		Value m.	Remarks
69	1873	Sep. 8	.31	
70	"	"	.35	
71	"	9	.40	Interruption by clouds. Lamp blown by wind.
72	"	12	.35	Cap wind blows lamp.
73	"	"	.61	Comp <sup>n</sup> . vitiated by clouds.
74	"	16	.06	Strong wind; lamp badly blown; once blown out.
75	"	17	.0	Superb night.
76	"	21	.69	
77	"	24	.34	Seeing excessively bad.
78	"	25	.63	Wind interfered.
79	"	26	.66	Seeing bad.
80	"	26	.64	
81	"	27	.65	
82	"	"	.56	
83	"	28	.26	
84	"	"	.41	Lamp blown a little; light wind.
85	"	30	.80	
86	"	"	.42	
87	"	"	.62	Observ. stopped by clouds.
88	"	Oct. 1	—	
89	"	8	.59	
91	"	"	.54	
92	"	9	.66	
93	"	"	.58	
94	"	"	.61	
95	"	11	.47	
96	"	"	.11	
97	"	13	.16	Very hazy, seeing not very good.
98	"	15	— .17	
99	"	"	— .22	
100	1874	Feb. 4	.62	Washington.
101	"	"	.36	
102	"	"	r	
103	"	10	.66	
104	"	"	.80	
105	"	"	.90	
106	"	11	1.12	Seeing very bad.
107	"	12	1.47	Hazy, perhaps cloudy, no moon.
108	"	18	.52	
109	"	March 24	.58	
110	"	"	.50	
111	"	26	.29	Bright moonlight.
112	"	"	.46	
113	"	"	.82	
114	"	29	.86	
115	"	Apr. 13	.17	Cambridge.
116	"	30	.51	
117	"	May 2	.26	Very clear.
118	"	"	.46	

No. of set	Date		Value m.	Remarks
119	1874	May 2	.22	
120	"	3	.43	
121	"	"	.55	
122	"	28	.25	<i>Hoosac mountain.</i> Very clear, bright moonlight.
123	"	June 13	.08	
124	"	21	.11	
125	"	"	-.02	
126	"	27	.06	Lamp considerably blown.
127	"	"	.11	
128	"	"	.12	
129	"	"	-.42	
130	"	28	.02	
131	"	"	.11	
132	"	July 14	r	Night very hazy.
133	"	"	.81	Seeing especially bad.
134	"	15	.23	
135	"	"	.45	
136	"	17	.30	
137	"	"	.40	
138	"	22	.45	
139	"	"	.74	
140	"	"	.69	
141	"	30	.90	
142	"	"	.73	
143	"	"	.70	
144	"	31	.95	Bright moonlight and light clouds continually passing over the field.
145	"	Aug. 2	.54	
146	"	"	.66	
147	"	"	.66	
148	"	3	.88	
149	"	"	.95	Very light passing clouds.
150	"	"	.98	
151	"	"	1.62	Growing light rapidly.
152	"	4	.91	Thin flying clouds during all these observ.
153	"	"	.91	
154	"	"	.93	
155	"	22	.85	
156	"	"	1.08	
157	"	"	1.33	
158	"	"	.99	
159	"	Oct. 21	.88	<i>Northampton.</i>
160	"	"	.93	
161	"	"	.91	After 2 <sup>nd</sup> round lamp filled again.
162	"	22	.94	Bright moonlight.
163	"	Nov. 26	.82	<i>Cambridge.</i>
164	"	"	.82	

No. of set	Date	Value m.	Remarks
165	1874 Nov. 26	.94	
166	" " "	.86	
167	" " "	1.01	
168	" " "	.94	
169	" " "	.84	
170	" " "	.88	
171	" " "	.85	
172	" " 30	.74	
173	" " "	.63	
174	" " "	.89	
175	" " "	.99	
176	" " "	.99	
177	" " "	1.04	Presence of clouds suspected.
178	" " "	.87	
179	" " "	.88	
180	Dec. 4	.75	Wind blows lamp a little.
181	" 21	.80	
182	" " "	.91	
183	" " "	.98	
184	" " "	.98	
185	" " "	.95	
186	1875 Jan. 10	.78	
187	" " "	.86	
188	" " "	1.04	Extreme cold interferes.
189	" 14	.78	Lamp very much blown by wind.

The light shone through a small glass window in the metallic chimney of the lamp, on to a disk, perforated with minute holes which I have called the pinholes. There were six pinholes, either of which could be so placed that the light would pass through it. I measured the longest and shortest diameter of each pinhole and found them to be as follows. The measures are expressed in turns of a micrometer screw each of which is equal to about  $\frac{1}{40}$  of an inch.

Pinhole	Longest Diameter	Shortest Diameter
1	1.039	1.038
2	.817	.805
3	.599	.597
4	.396	.315
5	.315	.301
6	.219	.217

Pinhole 4 is very irregular, (thus ). The measure of its short diameter was accommodated, so as to make what it was estimated

would be an ellipse of equal area. I made experiments to determine the effect of the different pinholes upon the magnitude of the photometer star, and found it to be such as would be obtained by multiplying the theoretical logarithms of the light by 0.894. The following tables exhibit the results of these experiments. They are expressed in a scale of magnitudes for which  $\rho = 2.25$ , this being the scale used in the reduction of my observations.

1872 March 8

Pinhole	Diff. from mean	
	Obs.	Theory $\times .894$
3	+ .31	+ .25
2	+ .94	+ .92
5	-1.25	-1.19

1872 March 18

Pinhole	Diff. from mean	
	Obs.	Theory $\times .894$
1	+1.68	+1.71
2	+1.14	+1.16
3	+0.51	+0.49
4	-0.61	-0.67
5	-1.03	-0.97
6	-1.67	-1.73

In the reduction of my observations, I have used the theoretical values multiplied by .894. The pinhole was not often changed.

From the pinhole, the light passes through a small double concave lens of short focus, then through a diaphragm with an aperture of about a millimeter, then through the first Nicol, then through the quartz plate, then through the other two Nicols. It will be observed that, unless these Nicols are placed with their axes in coincidence with the optical axis, the law of the diminution of light

$$l = a (\sin \varphi)^2,$$

where  $\varphi$  is the photometer reading, does not hold. The Nicols which came with the instrument were replaced by others in the winter of

1872-73, and these later ones were, I think, successfully put into position. They seem absolutely to extinguish the brightest light I have been able to throw in. To avoid, in some degree, the error due to malposition of the prisms, it was my habit to make four observations in the four quadrants of the photometer circle.

From the last Nicol the light passes through an unachromatic double convex lens. This lens is capable of being moved so as to bring its focus into coincidence with that of the objective of the telescope, and the screw by means of which this is effected, is furnished with a divided scale. But this motion is excessively bad and wobbling; and as the last Nicol is moved with this lens, the parallelism of the Nicols cannot be kept perfect. The motion of this lens by means of the screw is so slow that I found it excessively difficult to find the focus. For if I attempted to do this by means of the appearance of distinctness of the image, the involuntary accommodation of the eye greatly troubled me; while owing to the low magnifying power of the telescope (about 11) and its want of fixity, it was quite impossible to do so by the parallax between the photometer-star and the real star. In fact, I found the only way was to get it as near as I could and then alter it as I got the impression of one or the other image appearing more distinct.

From this lens, the light passes through a diaphragm with an aperture of about a millimeter, and then strikes upon a plate of glass in the middle of the telescope, from which it is reflected to the eye-piece (an ordinary negative ocular), and thence passes through a totally reflecting prism, which turns it at right angles, and thence through the diaphragm of the eye-piece.

The light of the lamp therefore strikes the following surfaces.

Part of the Instrument	No. of surfaces
Window of chimney	2
Concave lens	2
First Nicol	4
Quartz plate	2
Two last Nicols	8
Convex lens	2
Glass plate	1
Eye-piece	4
Reflecting prism	3
Total	28

The plate of glass which reflects the photometer star down to the eye-piece has its two surfaces inclined to one another. The effect of the interposition of this plate is to make the image of the real star excessively bad.

Owing to the photometer star being reduced in light to match the real star, instead of the real star being reduced to match the photometer star, it is necessary, for bright stars, to put a cap on the object-glass of the telescope. The aperture of the telescope I have used is 37 millimetres, the aperture of cap 1 is 26 millimetres and the aperture of cap 2 is 13 millimetres. I found that these caps seldom or never produced their theoretical effects. Moreover, the effects actually produced varied very much. I attribute this chiefly to the want of polish of the reflecting surface of the totally reflecting prism, which, being close up to a piece of brass, would get spotted with oxide of zinc and according as it was worse, in this respect, about where the outside or the inside of the pencil of rays fell, it would give a greater or a less value to the cap constant. The irregularity was increased by the dirt which would get on the outer surface of the prism and which the eye-piece diaphragm, fixed in its position with screws, would prevent me from entirely removing from the edges of the ocular aperture. This difficulty was at its worst in winter, when frost would have to be frequently wiped off in the midst of the observations. I have found it necessary to assume five different sets of values of the cap constants for observations made at different periods. I express these in a scale for which  $\rho = 2.25$ .

*First Period.* Observations at Cambridge from 1872 Feb. 14 to March 22, and observations in Washington from 1872 May 4 to 1873 March 22. After this, the instrument was taken to pieces and some alterations were made in it.

*Second Period.* Observations in Washington from 1873 May 13 to July 1, observations on the Hoosac Mountain from 1873 September 6 to October 15, and observations in Washington from 1874 January 23 to March 29.

*Third Period.* Observations at Cambridge from 1874 April 13 to May 3.

*Fourth Period.* Observations on the Hoosac Mountain from 1874 May 28 to August 22.

*Fifth Period.* Observations at Northampton, 1874 October 21 and 22, and observations at Cambridge 1874 November 26 to 1875 January 15.

The following are the adopted values.

Period	Cap 1	Cap 2
	m.	
1	0.46	1.79
2	0.70	2.19
3	0.80	2.11
4	0.72	2.44
5	0.74	2.15

The following table shows the separate observations for the determinations of these constants. Their disagreements are very great. I write "Cap 0" for no Cap.

Observations on Cap Constants  
*Difference between Cap 1 and Cap 0*

PERIOD I						
	Date	Star	Value	No. of settings	Remarks	
1872	March 8		0.55	5	Good. Pinhole 3.	
	" "		0.43	5	Good. Pinhole 3.	
	" 16	<i>g</i> Urs. maj.	0.56	5	Good.	
	May 4	$\nu_1$ and $\nu_2$ Bootae	0.42			
	" 8	"	0.38			
1873	June 11	13 Lyrae	0.54	4	Good. Rather too bright.	
	Feb. 28	$\zeta$ Urs. maj.	0.24	1	Much too bright. Reject.	

PERIOD II						
1873	June 1	$\omega$ Urs. maj.	0.69	3	Very good.	
"	" 47	" "	0.77	2	Very good.	
"	" 49	" "	0.79	2	Very good.	
"	20	$\theta$ Herculis	0.64	4	Very accordant, but altogether too bright.	
"	Sept. 8	<i>i</i> Herc.	0.56	3	Very accordant.	
"	30	66 B Cygni	0.38	4	Very good.	
"	" 10	B "	0.47	4	Good.	
"	" $\theta$	" "	0.56	4	Pretty good.	
"	" 16	Lyrae	0.55	4	Pretty good.	
"	Oct. 8	$\lambda$ Androm.	0.59	2	Very good.	
"	" $\varphi$	" "	0.68	4	Pretty good.	
"	15	22 "	0.61	2	Poor.	
1874	Feb. 11	$\gamma$ Persei	0.38	4	Accordant, but much too bright. Reject.	
"	12	$\gamma$ "	0.65	2	Poor.	

## PERIOD III

Date	Star	Value	No. of settings	Remarks
1874 Apr. 30	10 Urs. maj.	0.69	3	Good.
" May 2	38 Lyncis	0.55	4	Pretty accordant, but much too bright. Reject.

## PERIOD IV

1874 June 13	$\tau$ Cynni	0.43	3	Accordant, but much too bright. Reject.
" " "	$\sigma$ "	0.50	3	Pretty accordant, but too bright.
" " 27	8 Can. ven.	0.58	2	Good.
" " 28	$\theta$ Bootae	0.67	4	Pretty good.
" " "	$\iota$ "	0.71	4	Good.
" " "	$\kappa$ "	0.89	4	Tolerable.
" " "	$\lambda$ "	0.57	4	"
" July 17	13 "	0.77	2	Pretty good.
" " 13	"	0.69	2	Tolerable.
" " 22	$\varphi$ "	0.91	3	"
" " "	$\nu^2$ "	0.61	3	Good.
" " "	$\nu^1$ "	0.75	3	Tolerable.
" " 30	g Cynni	0.72	1	
" " "	$\pi^2$ "	1.15	1	
" " "	g "	0.72	4	Very good.
" " "	$\pi^2$ "	0.54	4	Pretty good—Some doubt about one observation.
" " "	$\sigma$ Lacertae	0.78	4	Good.
" " "	11 "	0.81	4	Very good.
" " 31	$\circ$ Androm.	0.84	1	
" Aug. 2	$\nu$ "	0.74	4	Good.
" " 18 H	"	0.82	4	Pretty good.
" " 3	$\varphi$ Persei	0.64	4	" "
" " "	$\chi$ Androm.	0.80	4	" "
" " "	41 H	0.72	4	Very good.
" " "	$\nu$ "	0.88	4	Good.
" " 4	$\varphi$ Persei	0.72	4	Very good.
" " "	$\nu$ "	0.67	4	Good.
" " "	$\omega$ Androm.	0.94	4	Pretty good.
" " "	$\xi$ "	0.86	4	" "
" " "	$\nu$ "	0.83	4	Good.
" " 22	$\theta$ Persei	0.90	2	Poor.
" " "	$\iota$ "	0.77	2	"
" " "	$c$	0.66	2	Pretty good. Doubtful identity.

## PERIOD V

	Date	Star	Value	No. of settings	Remarks
1874	Oct. 22	$\delta$ Persei	0.74	2	Too bright.
"	Nov. 26	$\nu$ "	0.60	2	Pretty good.
"	"	$\lambda$ "	0.66	2	" "
"	" 30	$\nu$ "	0.81	4	Poor.
"	"	$\epsilon$ Aurigae	0.77	3	Tolerable.
"	"	$\chi$ Urs. maj.	0.64	4	Poor.
"	"	$e$ Persei	0.58	4	Pretty good.
"	Dec. 21	$e$ "	0.84	2	Poor.

## Difference between Cap 2 and Cap 1

## PERIOD I

1872	March 8	$\alpha$ Leonis	1.09	9	Pretty good.
"	" "	$\nu$ "	1.39	9	"
"	" 16	$g$ Urs. maj.	1.41	6	Good.
"	" 22	12 Can. ven.	1.52	5	Good. Involves constant for Pinhole 6.
"	May 4	$\nu_1$ and $\nu_2$ Bootae	1.37		
"	" 8	$\tau$ Herculis	1.36	6	Pretty good. Involves constant for Pinhole 6.
"	" "	$\nu$ "	1.31	6	Pretty good.
"	" "	$\varphi$ "	1.40	6	" "
"	" 26	52 "	1.26	2	Poor.
"	July 8	$R$ Lyrae	1.54	3	Pretty accordant, too bright.
"	" "	$\epsilon^1$ "	1.52	3	Pretty good.
"	" "	$\epsilon^2$ "	1.42	3	" "
"	" 20	$\chi$ Herculis	1.23	4	Accordant, but too bright. Reject.
"	" 28	$\omega^2$ Cygni	1.25	4	Accordant, but too bright.
1873	Feb. 28	$\xi$ Urs. maj.	1.23	1	Too bright. Reject.
"	March 6	$\xi$ "	1.07	4	Tolerable.
"	" "	38 Lyncis	1.22	4	Pretty accordant, but much too bright.

## PERIOD II

1873	June 1	$\omega$ Urs. maj.	1.52	3	Very good.
"	" "	47 "	1.51	2	" "
"	" 20	$\eta$ Herculis	1.36	4	Very good. Rather bright.
"	" "	$\theta$ "	1.63	4	Very good.
"	Sept. 8	$\iota$ "	1.34	3	" "
"	" 16	$\iota$ "	1.22	1	Too bright. Reject.
"	" "	$\theta$ "	1.38	1	Too bright.
"	Oct. 8	$\lambda$ Androm.	1.31	2	

## PERIOD II (Continued)

	Date		Star	Value	No. of settings	Remarks
1873	Oct.	11	$\rho$ Cygni	1.95	3	Accordant. Too bright.
"	"	"	$\pi$ "	1.92	3	Very good. Perhaps too bright.
"	"	13	$\varphi$ Androm.	1.21	2	Good.
"	"	15	$\iota$ "	1.05	1	Very poor.
"	"	"	$\kappa$ "	1.42	2	Pretty good.
"	"	"	$\lambda$ "	1.36	2	Tolerable.
1874	Feb.	11	$\gamma$ Persei	1.45	4	Pretty good.
"	"	12	$\gamma$ "	1.59	2	Poor.

## PERIOD III

1874	Apr.	30	10 Urs. maj.	1.20	3	Good.
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## PERIOD IV

1874	June	13	$\tau$ Cygni	1.89	3	Tolerable.
"	"	28	$\theta$ Bootae	1.81	4	Pretty good.
"	"	"	$\kappa$ "	1.45	4	Pretty good.
"	"	"	$\lambda$ "	1.32	4	Tolerable.
"	July	17	13 "	1.78	2	Very good.
"	"	"	$\lambda$ "	2.01	2	Poor.
"	"	"	13 "	1.75	2	"
"	"	"	$\lambda$ "	1.82	2	Pretty good.
"	"	22	$\mu$ "	1.79	3	Very good.
"	"	30	$\rho$ "	1.26	1	
"	"	"	$\rho$ "	1.81	4	Good.
"	"	31	$\sigma$ Androm.	1.88	1	
"	Aug.	2	$\sigma$ "	1.56	4	Good.
"	"	"	$\iota$ "	1.62	4	"
"	"	"	$\kappa$ "	1.88	4	Very good.
"	"	"	$\varphi$ "	1.90	4	Poor.
"	"	3	$\varphi$ Persei	1.48	4	Pretty good.
"	"	"	$\nu$ "	1.68	4	" "
"	"	"	$\nu$ Androm.	1.44	4	" "
"	"	4	$\varphi$ Persei	1.92	4	Tolerable.
"	"	"	$\nu$ "	1.72	4	Pretty good.
"	"	"	$\nu$ Androm.	1.65	4	Good.
"	"	22	$\tau$ Persei	1.52	2	Tolerable.

## PERIOD V

1874	Oct.	22	$\delta$ Persei	1.34	2	Poor.
"	Nov.	26	$\delta$ "	1.63	2	Tolerable.
"	"	30	$\nu$ "	1.17	4	Pretty good.
"	Dec.	21	$\epsilon$ Aurigae	1.46	3	Good.
"	"	"	$\zeta$ "	1.37	3	Tolerable.
"	"	"	$\eta$ "	1.45	3	Pretty good.

## Difference between Cap 0 and Cap 2

PERIOD I					
Date		Star	Value	No. of settings	Remarks
1872	March 18		1.83	5	Good. Involves constant for Pinhole No. 3.
PERIOD II					
1873	Sept. 26	$\iota$ Androm.	2.37	2	Poor.
"	"	$\kappa$ "	2.58	2	"
"	27	7 Lacertae	1.97	4	Tolerable.
"	"	$\lambda$ Androm.	2.46	2	Good.
"	28	7 Lacertae	2.09	3	Very accordant, but too bright.
PERIOD III					
1874	May 2	$\iota$ Urs. maj.	2.04	4	Poor. Pinholes 5 and 3.
"	"	$\kappa$ " "	1.90	4	Poor. "
"	3	$\iota$ " "	2.28	4	" "
"	"	$\kappa$ " "	2.32	4	Tolerable. "
"	"	$\theta$ " "	2.27	4	Pretty good. "
PERIOD IV					
1874	May 28	38 Lyncis	1.76	3	Pretty accordant. Too bright. Reject.
PERIOD V					
1875	Jan. 10	$\chi$ Urs. maj.	2.16	3	Tolerable.

There can be no doubt, that the errors introduced by the use of these diaphragms are by far the most serious of those by which my observations are affected.

The most difficult part of the observations with this instrument consists in putting the eye straight to the telescope. It must be remembered that two pencils of rays emerge from the eye-piece, not quite parallel to one another; and both must pass entirely into the pupil. This would be comparatively easy if the observer had a long straight tube before him to show the direction of the ocular axis, instead of the shortest possible prism; or if the telescope remained fixed, instead of constantly changing its direction. I should strongly recommend that such photometers be constructed with the eye-piece in the axis of the telescope; and if this were combined with a system of large Nicol prisms arranged so that the real star instead of

the photometer star should be altered in brightness, I am sure it would result in a greater accuracy in the observations. As it was, I was obliged to have a table constructed to raise and lower with a crank, and also to turn on a horizontal axis, so as to give room to rest the arm upon it, while it enabled me to put my body close to the instrument. This table had a very smooth motion and proved exceedingly advantageous. Each time that I put my eye to the telescope I had to move it about until I was confident I had got it in the best position. It was, also, of course, necessary, that both real star and photometer star should constantly fall on fixed parts of the retina. Here came in another source of error, owing to these parts being unequally fatigued by the varying lights,—a difficulty which would not have been noticed, I think, if the real star had been brought to a fixed standard. Having spent perhaps a minute in getting my eye properly placed, I found it best not to spend much time in moving the photometer circle tentatively one way and the other, but to place it pretty promptly where it ought to be, so as not to fatigue myself in the effort to reduce an element of error which would at all events be small, as compared with the others.

The instrument always stood in a little round observatory, constructed for the purpose. In Washington, the observatory was of wood and stood on the roof of the Coast Survey office. In other places, I had an observatory which is shown in Plate III at the end of this volume, constructed with a wooden frame covered with canvas. It could be taken to pieces and packed in small compass, so as to be easily transported from place to place, as the nature of my duties as an officer of the Coast Survey required. The roof rolled on three composition billiard balls on a bed of cast brass. The wooden observatory was all painted dead black in the inside; the canvas one was hung with black cambric. This was found to be an indispensable precaution. In Washington, I communicated with a recorder within the large building by means of a speaking tube. In other places, a portable wooden building stood close to the photometric observatory, and I called out to a recorder seated in that. He always repeated what he heard, but nevertheless erroneous numbers were, no doubt, sometimes set down. Wrong designations of the stars are frequent, but to these I paid less attention. It is unfortunate that the recorder could not read the instrument, so as to avoid having the observer look at any illuminated surface, and perhaps that might be contrived, in making a new photometer.

*Plan of the Observations*

The design of the observations was to obtain the magnitudes of all the stars in Argelander's *Uranometria* between  $40^{\circ}$  and  $50^{\circ}$  of North declination (a zone having an area of about  $\frac{1}{16}$  of the sphere) with a probable error of not more than  $\frac{1}{10}$  of a magnitude, so that we might be provided with stars at all times at every altitude with which to compare others in forming a new uranography. The plan was to observe 368 stars, but others sufficiently bright being noticed, about one hundred more have actually been measured. The stars proposed to be observed were divided into seventy groups each consisting of neighboring stars, these groups lying in two zones, and so that the boundary between two adjacent groups of either zone should have as near as possible the same right ascension as the middle of a group in the other zone. The groups of the southern zone were numbered I, III, V, VII, etc., and those of the northern zone II, IV, VI, etc. Each set of observations consisted in comparing the stars of two groups with the photometer star, thus comparing these groups with one another. Every star was observed once in turn and then I began at the first one again and so went round four times, unless clouds or some other inconvenience prevented. I should have done better to have made each alternate round in the inverse order so as to eliminate any gradual uniform change in transparency of the atmosphere. Every group was thus compared with four others adjacent to it, group number  $n$  being compared with groups  $n - 2$ ,  $n - 1$ ,  $n + 1$ , and  $n + 2$ . The error accumulated in completing the circle of right ascension in this way only happened to amount to  $0^m 158$  on my usual scale. But, to avoid accumulation of error, I also compared groups I and XXI, XI and XXXI, etc., at times when the two distant groups were about equally high in opposite sides of the heavens.

The observations having been completed, all the photometer readings were separately reduced to a scale of magnitudes for which  $\rho = 2.25$ . The table by which this was done is appended to this memoir (see Plate VI). The mean difference of magnitude of each star from the photometer star during each set of observations was then taken, then the difference of the mean magnitude of the stars of the two groups from the photometer star, as well the difference of the means of the two groups from each other, and then the difference of each star from the mean of the group to which it belonged. The following is an example of this process.

Sunday, 1874, March 29

*Groups XIII and XV*

## 42 Aurigae

Cap, 0	Pinhole, 5
Readings	Mags.
+ 15°0	3.33
+165.9	3.48
-168.7	4.02
- 12.1	<u>3.79</u>
Mean,	3.65

 $\psi^9, n$ , Aurigae

Cap, 0	Pinhole, 5
Readings	Mags.
+ 15.6	3.24
+164.2	3.21
-164.0	3.18
- 18.5	<u>2.83</u>
Mean,	3.11

43 Aurigae<sup>5</sup>

Cap, 0	Pinhole, 5
+ 15.3	3.29
+166.9	3.66
-164.9	3.32
- 13.5	<u>3.59</u>
Mean,	3.46

 $\psi^9, s$ , Aurigae

Cap, 0	Pinhole, 5
+ 16.2	3.15
+165.7	3.45
-167.2	3.72
- 16.4	<u>3.12</u>
Mean,	3.36

 $\psi^1$  Aurigae

Cap, 0	Pinhole, 5
+ 28.7	1.81
+152.0	1.86
-151.6	1.83
- 28.8	<u>1.80</u>
Mean,	1.82

 $\psi^{10}$  Aurigae

Cap, 0	Pinhole, 5
+ 24.7	2.15
+157.3	2.35
-158.4	2.46
- 23.2	<u>2.30</u>
Mean,	2.31

 $\psi^6$  Aurigae

Cap, 0	Pinhole, 5
+ 23.7	2.25
+157.8	2.40
-158.2	2.44
- 25.2	<u>2.11</u>
Mean,	2.30

## 43B Lyncis

Cap, 0	Pinhole, 5
+ 19.2	2.74
+160.2	2.67
-163.9	3.16
- 19.3	<u>2.73</u>
Mean,	2.82

5. OBSERVER'S NOTE. 43 Aurigae is a little brighter than 42.

## 8H Lyncis

Cap, 0	Pinhole, 5
Readings	Mags.
+ 27.2	1.93
+ 154.7	2.10
- 156.8	2.30
- 23.1	<u>2.31</u>
Mean,	2.16

## 21 Lyncis

Cap, 0	Pinhole, 5
Readings	Mags.
+ 27.5	1.91
+ 150.2	1.72
- 154.0	2.03
- 26.5	<u>1.99</u>
Mean,	1.91

## 22 Lyncis

Cap, 0	Pinhole, 5
Readings	Mags.
+ 20.4	2.60
+ 158.4	2.46
- 159.5	2.59
- 21.0	<u>2.53</u>
Mean,	2.54

## P. VII. 92

Cap, 0	Pinhole, 5
Readings	Mags.
+ 19.3	2.73
+ 161.7	2.86
- 164.0	3.18
- 17.1	<u>3.02</u>
Mean,	2.95

Anonyma<sup>6</sup>

Cap, 0	Pinhole, 5
Readings	Mags.
+ 17.2	3.00
+ 161.0	2.77
- 161.1	2.78
- 17.2	<u>3.00</u>
Mean,	2.86

P. VII. 156<sup>7</sup>

Cap, 0	Pinhole, 5
Readings	Mags.
+ 19.5	2.71
+ 160.3	2.68
- 161.7	2.86
- 18.6	<u>2.88</u>
Mean,	2.77

25 Lyncis<sup>8</sup>

Cap, 0	Pinhole, 5
Readings	Mags.
+ 19.5	2.71
+ 159.3	2.56
- 163.2	3.06
- 19.4	<u>2.72</u>
Mean,	2.76

## 26 Lyncis

Cap, 0	Pinhole, 5
Readings	Mags.
+ 13.6	3.57
+ 164.3	3.22
- 167.4	3.76
- 13.1	<u>3.66</u>
Mean,	3.55

6. The second time round, Anonyma seems brighter. It is reddish.

7. P. VII. 156 is the s. f. of two.

8. 25 Lyncis is n. of 26.

SUBSEQUENT NOTES. The star called  $\psi^9$ , *s*, Aurigae was 130 $\delta$  Aurigae. 25 and 26 Lyncis have their designations transposed. "Anonyma," is 26 $\delta$  Lyncis.

*Group XIII*

Star	Diff. from Phot. star	Diff. from mean of group
$\psi^{10}$ Aurigae	2.31	-0.55
$\psi^1$ "	1.82	-1.04
$\psi^6$ "	2.30	-0.56
$\psi^9$ , <i>n</i> , "	3.11	+0.25
130 $\delta$ "	3.36	+0.50
43 "	3.46	+0.60
42 "	<u>3.65</u>	+0.79
Mean,	2.86	

Magnitude of Photometer star, 1.05.

*Group XV*

Star	Diff. from Phot. star	Diff. from mean of group
21 Lyncis	1.91	-0.81
8H "	2.16	-0.56
P. VII. 156	2.77	+0.05
26 Lyncis	2.76	+0.04
43B "	2.82	+0.10
26 $\delta$ "	2.86	+0.14
P. VII. 92.	2.95	+0.23
25 Lyncis	<u>3.55</u>	+0.83
Mean,	2.72	

Extra star

22 Lyncis      2.54      -0.18

Magnitude of Photometer star,      1.10

Ditto, mean of two values      1.08

Group XIII - Group XV = +0.14.

As my observations are of very various degrees of precision, I should have much diminished the probable error of my results if I had weighted the observations according to their agreement with one another during the set. But I could not have done this without taking account of the effect of clouds and without rejecting numerous observations by Peirce's criterion. Such a proceeding would have involved more labor than I was inclined to undertake, and I have, therefore, simply allowed the means of every night's observations the same weight. To this I have made a few exceptions in the case of observations which were supposed at the time to be worthless or of extremely little value, which I have sometimes rejected altogether and sometimes have allowed them  $\frac{1}{4}$  weight. In two or three cases, sets of observations which I did not suspect at the time, further than to note unfavorable circumstances attending them, have been rejected on account of enormous discrepancies showing the presence of abnormal sources of error. Three or four of the sets which have been the worst vitiated by clouds so that the residuals of successive observations would for a while have nearly the same value which would then change more or less suddenly to another value, and where the presence of clouds was mentioned in the record, have been separated into partial sets which have been separately computed, and then the mean result of the partial sets has been adopted. In a few other cases of this sort where the first residuals have been very large, with one sign, and have gradually changed until the last have been very large with the other sign, I have assumed that there was a uniform change in the transparency of the air. In every case in which the residuals have appeared to have so marked a system in their values as to require either of these procedures, I have undertaken it without knowing what the result would be, and have always adhered to that result. But I have in no case found such a proceeding to make the agreement of the observed and calculated differences greater than it was before nor of a different sign.

Having thus obtained the observed values of the differences of the means of the different groups, I had to consider (neglecting at first the comparisons of *distant* groups) sixty-nine equations between the differences of *successive* groups considered as unknown quantities. Using the usual procedure of least squares, these equations will not be of the same form, but we may introduce a quantity X so as to give seventy equations of the form

$$\begin{aligned}
 X &+ (\text{Group } n - \text{Gr.}(n + 1)) + 3(\text{Gr.}(n + 1) - \text{Gr.}(n + 2)) \\
 &+ (\text{Gr.}(n + 2) - \text{Gr.}(n + 3)) \\
 &= \text{Obs.}(\text{Gr. } n - \text{Gr.}(n + 2)) + \text{Obs.}(\text{Gr.}(n + 1) - \text{Gr.}(n + 2)) \\
 &+ \text{Obs.}(\text{Gr.}(n + 1) - \text{Gr.}(n + 3)).
 \end{aligned}$$

Group 71 must be considered to be Group 1, and group 72 to be group 2. Taking the successive differences of these equations, and using the notation

$$\begin{aligned}
 x_n &= \text{Group } n - \text{Group } (n + 1) \\
 \{a, b\} &= \text{Obs.}(\text{Group } a - \text{Group } b)
 \end{aligned}$$

we obtain seventy equations of the form

$$\begin{aligned}
 f_n &= x_{n-2} + 2x_{n-1} - 2x_n - x_{n+1} - \{n - 2, n\} \\
 &- \{n - 1, n\} + \{n, n + 1\} + \{n, n + 2\} = 0.
 \end{aligned}$$

These equations are not all independent but the quantity

$$x_{70} = \text{Group } 70 - \text{Group } 1$$

is the negative of the sum of all the other unknown quantities. Let us now write,

$$a_1f_1 + a_2f_2 + a_3f_3 + \text{etc.} = 0;$$

the coefficients being supposed to be such that when the values of  $f_1, f_2, f_3$ , etc., are substituted, all the unknown quantities are eliminated except  $x_m$  and  $x_{70}$ . In order that any unknown quantity  $x_n$  (supposed not be  $x_{69}, x_{70}$ , or  $x_1$ ) may disappear from this equation we must have

$$a_{n-1} + 2a_n - 2a_{n+1} - a_{n+2} = 0.$$

Considering this as an equation of finite differences, we solve it by means of the auxiliary equation

$$y^3 + 2y^2 - 2y - 1 = 0$$

The roots of the auxiliary equation being 1,  $\frac{-3 - \sqrt{5}}{2}$ , and  $\frac{-3 + \sqrt{5}}{2}$  the general solution of the equation of differences is

$$\begin{aligned} a_n &= c_1 + c_2 \left( \frac{-3 - \sqrt{5}}{2} \right)^n + c_3 \left( \frac{-3 + \sqrt{5}}{2} \right)^n \\ &= c_1 + c_2 \left( \frac{-3 - \sqrt{5}}{2} \right)^n + c_3 \left( \frac{-3 - \sqrt{5}}{2} \right)^{-n} \end{aligned}$$

If now we take one set of values for  $c_1$ ,  $c_2$ , and  $c_3$  for all the  $a$ 's from  $a_1$  to  $a_{m+1}$  inclusive, and another set of values of these constants (which we may denote by  $C_1$ ,  $C_2$ , and  $C_3$ ) for all the  $a$ 's from  $a_m$  to  $a_{70}$  inclusive, we shall have, in order to determine these six quantities, the following equations. First, two equations to make the two values of  $a_m$  and  $a_{m+1}$  which are included in both sets, equal.

$$\begin{aligned} c_1 + c_2 \left( \frac{-3 - \sqrt{5}}{2} \right)^m + c_3 \left( \frac{-3 - \sqrt{5}}{2} \right)^{-m} \\ = C_1 + C_2 \left( \frac{-3 - \sqrt{5}}{2} \right)^m + C_3 \left( \frac{-3 - \sqrt{5}}{2} \right)^{-m} \\ c_1 + c^2 \left( \frac{-3 - \sqrt{5}}{2} \right)^{m+1} + c_3 \left( \frac{-3 - \sqrt{5}}{2} \right)^{-m-1} \\ = C_1 + C_2 \left( \frac{-3 - \sqrt{5}}{2} \right)^{m+1} + C_3 \left( \frac{-3 - \sqrt{5}}{2} \right)^{-m-1} \end{aligned}$$

Second, an equation to make the coefficient of  $x_m$  unity,

$$\begin{aligned} &+ 3c_1 + c_2 \left( \left( \frac{-3 - \sqrt{5}}{2} \right)^{m-1} + 2 \left( \frac{-3 - \sqrt{5}}{2} \right)^m \right) \\ &+ c_3 \left( \left( \frac{-3 - \sqrt{5}}{2} \right)^{-m+1} + 2 \left( \frac{-3 - \sqrt{5}}{2} \right)^{-m} \right) \\ &- 3C_1 - C_2 \left( \left( \frac{-3 - \sqrt{5}}{2} \right)^{m+2} + 2 \left( \frac{-3 - \sqrt{5}}{2} \right)^{m+1} \right) \\ &- C_3 \left( \left( \frac{-3 - \sqrt{5}}{2} \right)^{-m-2} + 2 \left( \frac{-3 - \sqrt{5}}{2} \right)^{-m-1} \right) = 1. \end{aligned}$$

And, third, two equations to make, the coefficients  $x_{69}$  and  $x_1$  vanish,

$$\begin{aligned} c_1 + c_2 + c_3 - C_1 - C_2 \left( \frac{-3 - \sqrt{5}}{2} \right)^{70} \\ - C_3 \left( \frac{-3 - \sqrt{5}}{2} \right)^{-70} = 0 \\ c_1 + c_2 \left( \frac{-3 - \sqrt{5}}{2} \right) + c_3 \left( \frac{-3 - \sqrt{5}}{2} \right)^{-1} \\ - C_1 - C_2 \left( \frac{-3 - \sqrt{5}}{2} \right)^{71} - C_3 \left( \frac{-3 - \sqrt{5}}{2} \right)^{71} = 0. \end{aligned}$$

These equations serve to determine  $(c_1 - C_1)$ ,  $c_2$ ,  $c_3$ ,  $C_2$ , and  $C_3$  and in this way the equations might be solved without much labor. I have, however, found it more convenient to make use of the approximate formula,

$$\begin{aligned} \sqrt{5} \cdot x_n = & \{n, n+1\} + G (\{n, n+2\} + \{n-1, n+1\}) \\ & - G^2 (\{n-1, n\} + \{n+1, n+2\}) \\ & - G^3 (\{n+1, n+3\} + \{n-2, n\}) \\ & + G^4 (\{n-2, n-1\} + \{n+2, n+3\}) \\ & + G^5 (\{n+2, n+4\} + \{n-3, n-1\}) \\ & - G^6 (\{n-3, n-2\} + \{n+3, n+4\}) \\ & - \text{etc.} \end{aligned}$$

where  $G = \frac{1}{2}(\sqrt{5} - 1)$ . I do not know whether this method of solving equations has been used before, or not; but it seems to me, at all events, to have some interest. This procedure gave values so nearly right that the solution of the complete equations involving the comparisons of distant groups was then effected by adjustment by substitutions, in a very short time. The work was carried to thousandths of magnitude. The following table exhibits the results.

## Comparisons of the Groups

Groups	Observed values (Number of set in parenthesis)	Adopted mean	Calc.
I - II.	-0 <sup>m</sup> 16 (157)	-0.16	-0.22
I - III.	-0.60 (107); -0.60 (108)	-0.60	-0.61
II - III.	-0.43 (160)	-0.43	-0.40
II - IV.	-1.31 (161)	-1.31	-1.32
III - IV.	-0.98 (151)	-0.98	-0.92
III - V.	-0.10 (53); -0.02 (104)	-0.08	-0.07
IV - V.	+0.79 (162)	+0.79	+0.85
IV - VI.	-0.14 (163)	-0.14	-0.12
V - VI.	-0.94 (164)	-0.94	-0.97
V - VII.	-0.33 (109), taken at low altitude; -0.24 (111)	-0.24	-0.17
VI - VII.	+0.83 (166)	+0.83	+0.80
VI - VIII.	+0.92 (172)	+0.92	+0.92
VII - VIII.	-0.10 (180), wind blowing lamp; fluctuations of light	0.00	+0.12
VII - IX.	-0.95 (110); -0.90 (112)	-0.92	-1.01
VIII - IX.	-1.13 (173); -1.26 (181)	-1.19	-1.13
VIII - X.	-0.38 (185)	-0.38	-0.34
IX - X.	+0.78 (167)	+0.78	+0.79
IX - XI.	+0.22 (105)	+0.22	+0.20
X - XI.	-0.67 (174)	-0.67	-0.59
X - XII.	+0.23 (182)	+0.23	+0.21
XI - XII.	+0.72 (186), clouds	+0.72	+0.80
XI - XIII.	-0.12 (113)	-0.12	-0.11
XII - XIII.	-0.95 (175)	-0.95	-0.91
XII - XIV.	-0.46 (183)	-0.46	-0.43
XIII - XIV.	+0.48 (187)	+0.48	+0.48
XIII - XV.	+0.14 (114)	+0.14	+0.19
XIV - XV.	-0.35 (165)	-0.35	-0.29
XIV - XVI.	+0.61 (118)	+0.61	+0.58
XV - XVI.	+0.74 (115), clouds	+0.74	+0.87
XV - XVII.	+0.79 (168); +0.69 (184)	+0.74	+0.72
XVI - XVII.	-0.18 (116)	-0.18	-0.15
XVI - XVIII.	-0.35 (101)	-0.35	-0.29
XVII - XVIII.	-0.14 (119)	-0.14	-0.14
XVII - XIX.	+0.12 (121)	+0.12	+0.12
XVIII - XIX.	+0.20 (122)	+0.20	+0.26
XVIII - XX.	-0.02 (48), once round; +0.22 (49)	+0.17	+0.18
XIX - XX.	+0.01 (170); -0.06 (176)	-0.03	-0.08
XIX - XXI.	-0.98 (178), clouds	-0.98	-0.86
XX - XXI.	-0.74 (50)	-0.74	-0.78
XX - XXII.	-0.65 (10); -0.89 (3)	-0.77	-0.77
XXI - XXII.	0.00 (171)	0.00	+0.01
XXI - XXIII.	+1.77 (54)	+1.77	+1.74
XXII - XXIII.	+1.74 (179); +2.01 (188), low altitudes	+1.74	+1.73
XXII - XXIV.	-0.18 (58)	-0.18	-0.17

Groups	Observed values (Number of set in parenthesis)	Adopted mean	Calc.
XXIII - XXIV.	-1 <sup>m</sup> 23 (2), not yet accustomed to inst.; -1.80 (15) . . . . .	-1.80	-1.90
XXIII - XXV.	-2.76 (65) . . . . .	rejected	-2.23
XXIV - XXV.	-0.34 (124); -0.25 (126) . . . . .	-0.30	-0.33
XXIV - XXVI.	+0.89 (55); +1.01 (1), before I had learnt to observe . . . . .	+0.89	+0.83
XXV - XXVI.	+1.13 (6) . . . . .	+1.13	+1.16
XXV - XXVII.	+0.39 (67) . . . . .	+0.39	+0.32
XXVI - XXVII.	-0.92 (127) . . . . .	-0.92	-0.85
XXVI - XXVIII.	-0.87 (5); -0.77 (7) . . . . .	-0.82	-0.92
XXVII - XXVIII.	-0.04 (128) . . . . .	-0.04	-0.08
XXVII - XXIX.	+0.15 (125) . . . . .	+0.15	+0.21
XXVIII - XXIX.	+0.36 (64) . . . . .	+0.36	+0.29
XXVIII - XXX.	-0.64 (59); -0.68 (60) . . . . .	-0.66	-0.74
XXIX - XXX.	-0.97 (66) . . . . .	-0.97	-1.03
XXIX - XXXI.	+0.57 (130) . . . . .	+0.57	+0.61
XXX - XXXI.	+2.22 (132), once round; +1.54 (134); +1.72 (136) . . . . .	+1.81	+1.64
XXX - XXXII.	+0.75 (61) . . . . .	+0.75	+0.79
XXXI - XXXII.	-0.85 (133); -0.95 (69), of no value . . . . .	-0.85	-0.85
XXXI - XXXIII.	-1.02 (137) . . . . .	-1.02	-0.96
XXXII - XXXIII.	-0.15 (138) . . . . .	-0.15	-0.11
XXXII - XXXIV.	+0.83 (8) . . . . .	+0.83	+0.84
XXXIII - XXXIV.	+0.82 (139) . . . . .	+0.82	+0.95
XXXIII - XXXV.	+0.95 (140) . . . . .	+0.95	+0.94
XXXIV - XXXV.	+0.09 (9) . . . . .	+0.09	-0.01
XXXIV - XXXVI.	-0.86 (56) . . . . .	-0.86	-0.62
XXXV - XXXVI.	-0.53 (31) . . . . .	-0.53	-0.61
XXXV - XXXVII.	-0.60 (18) . . . . .	-0.60	-0.65
XXXVI - XXXVII.	-0.02 (20) . . . . .	-0.02	-0.04
XXXVI - XXXVIII.	+0.22 (23) . . . . .	+0.22	+0.41
XXXVII - XXXVIII.	+0.45 (57) . . . . .	+0.45	+0.45
XXXVII - XXXIX.	+0.01 (11); +0.09 (72) . . . . .	+0.05	-0.04
XXXVIII - XXXIX.	-0.57 (70) . . . . .	-0.57	-0.59
XXXVIII - XL.	-0.62 (63) . . . . .	-0.62	-0.49
XXXIX - XL.	+0.03 (71) . . . . .	+0.03	0.00
XXXIX - XLI.	-1.33 (13) . . . . .	-1.33	-1.31
XL - XLI.	-1.36 (27) . . . . .	-1.36	-1.31
XL - XLII.	-0.35 (68) . . . . .	-0.35	-0.32
XLI - XLII.	+0.99 (73) . . . . .	+0.99	+0.99
XLI - XLIII.	+0.62 (19) . . . . .	+0.62	+0.67
XLII - XLIII.	-0.41 (25) . . . . .	-0.41	-0.32
XLII - XLIV.	+0.37 (74); +0.38 (85) . . . . .	+0.38	+0.30
XLIII - XLIV.	+0.50 (92) . . . . .	+0.50	+0.62
XLIII - XLV.	+0.35 (17) . . . . .	+0.35	+0.39
XLIV - XLV.	-0.31 (16); -0.32 (22) . . . . .	-0.31	-0.23

Groups	Observed values (Number of set in parenthesis)	Adopted mean	Calc.
XLIV – XLVI.	+0 <sup>m</sup> .12 (21)	+0.12	+0.07
XLV – XLVI.	+0.23 (28)	+0.23	+0.30
XLV – XLVII.	+1.32 (86)	+1.32	+1.38
XLVI – XLVII.	+1.01 (24)	+1.01	+1.08
XLVI – XLVIII.	-0.39 (30)	-0.39	-0.45
XLVII – XLVIII.	-1.64 (93)	-1.64	-1.53
XLVII – XLIX.	-1.57 (26)	-1.57	-1.54
XLVIII – XLIX.	-0.06 (95)	-0.06	-0.01
XLVIII – L.	+0.87 (33)	+0.87	+0.87
XLIX – L.	+0.92 (35)	+0.92	+0.88
XLIX – LI.	+0.62 (14)	+0.62	+0.75
L – LI.	-0.10 (39)	-0.10	-0.13
L – LII.	-0.10 (29)	-0.10	-0.12
LI – LII.	-0.06 (37)	-0.06	+0.01
LI – LIII.	+0.24 (32); +0.40 (87), once round	+0.27	+0.21
LII – LIII.	+0.17 (34)	+0.17	+0.20
LII – LIV.	+0.16 (41)	+0.16	+0.15
LIII – LIV.	+0.11 (43); +0.11 (42)	+0.11	-0.01
LIII – LV.	-0.69 (94)	-0.69	-0.56
LIV – LV.	-0.58 (123)	-0.58	-0.55
LIV – LVI.	-0.49 (38), clouds	-0.49	-0.61
LV – LVI.	-0.25 (135)	-0.25	-0.06
LV – LVII.	+0.51 (96)	+0.51	+0.48
LVI – LVII.	+0.58 (44)	+0.58	+0.54
LVI – LVIII.	+0.16 (46)	+0.16	+0.28
LVII – LVIII.	-0.38 (141), once round; -0.14 (142)	-0.19	-0.26
LVII – LIX.	+0.51 (36); +0.63 (83)	+0.57	+0.55
LVIII – LIX.	+0.81 (81)	+0.81	+0.81
LVIII – LX.	-0.31 (79); once round; -0.13 (143)	-0.13	-0.07
LIX – LX.	-0.77 (45), clouds	-0.77	-0.88
LIX – LXI.	-0.77 (47)	-0.77	-0.68
LX – LXI.	+0.26 (40)	+0.26	+0.20
LX – LXII.	-0.48 (145); -0.43 (144)	-0.46	-0.45
LXI – LXII.	-0.69 (84)	-0.69	-0.65
LXI – LXIII.	+0.38 (89); +0.60 (90), bad	+0.49	+0.37
LXII – LXIII.	+1.08 (98)	+1.08	+1.02
LXII – LXIV.	+0.04 (80)	+0.04	+0.16
LXIII – LXIV.	-0.80 (146)	-0.80	-0.86
LXIII – LXV.	-0.08 (82)	-0.08	-0.22
LXIV – LXV.	+0.53 (147)	+0.53	+0.64
LXIV – LXVI.	-0.23 (75)	-0.23	-0.26
LXV – LXVI.	-0.86 (91)	-0.86	-0.90
LXV – LXVII.	+0.52 (97)	+0.52	+0.44
LXVI – LXVII.	+1.57 (148)	+1.57	+1.44
LXVI – LXVIII.	+1.46 (149)	+1.46	+1.50
LXVII – LXVIII.	+0.07 (152)	+0.07	+0.06
LXVII – LXIX.	-0.98 (103)	-0.98	-1.08
LXVIII – LXIX.	-1.16 (150)	-1.16	-1.14

Groups	Observed values (Number of set in parenthesis)	Adopted mean	Calc.
LXVIII - LXX. $-0^m.88$ (153)	.	-0.88	-0.87
LXIX - LXX. $+0.26$ (154)	.	+0.26	+0.27
LXIX - I. $+1.81$ (106)	.	+1.81	+1.72
LXX - I. $+1.53$ (156)	.	+1.53	+1.45
LXX - II. $+1.13$ (159)	.	+1.13	+1.23
I - XXI. $-1.51$ (169); $-1.49$ (177); $-1.71$ (189), low altitude	.	-1.50	-1.61
XXIII - XLI. $-3.16$ (62)	.	-3.16	-3.11
XLI - LXI. $+1.54$ (76)	.	+1.54	+1.65
LXI - XI. $-0.55$ (158)	.	-0.55	-0.34
XI - XXXI. $+0.52$ (117); $+0.53$ (120)	.	+0.53	+0.71
XXXI - LI. $-0.34$ (131), clouds	.	-0.34	-0.37
LI - I. $+1.10$ (99), too low; $+1.34$ (155)	.	+1.34	+1.33

The last step in the computation was to compare the magnitudes of about 340 of my stars with the reduced magnitudes of the *Durchmusterung*, so as to reduce my scale to that scale of equable distribution used in the comparative catalogue. I found by least squares that  $\log \rho = .443$  for that scale, after entirely rejecting the bright stars. But upon subsequent consideration, I concluded to use  $\log \rho = 0.409$ , which better represents a few bright stars.

The following table exhibits the results of the comparison of each star with the mean of its group, according to each set of observations. The first column contains the number of the set of observations. The next column shows the number of settings upon each star. When any star was observed a greater or smaller number of times a number is prefixed to the measure of that star. The other columns show the differences of the magnitude of each star from the mean of the group to which it belongs.

## I

		$\gamma$ Persei	$\iota$ Persei	$\theta$ Persei	$\tau$ Persei	$47\delta$ Persei
(99)	4.	-1.11	+0.20	-0.10	-0.20	+1.20
(106)	4.	-1.50	+0.11	+0.04	-0.07	+1.44
(107)	2.	-1.44	+0.38	+0.07	-0.07	+1.07
(108)	4.	-1.38	+0.10	+0.07	-0.16	+1.37
(155)	2.	-1.37	+0.18	-0.08	-0.13	+1.38
(156)	2.	-1.29	+0.13	+0.09	-0.16	+1.25
(157)	3.	-1.59	+0.37	-0.02	-0.04	+1.28
(169)	2.	-1.38	+0.23	+0.06	-0.16	+1.25
(177)	2.	-1.13	+0.14	-0.10	-0.23	+1.31
(189)	2.	<u>-1.17</u>	<u>+0.33</u>	<u>+0.10</u>	<u>-0.25</u>	<u>+0.99</u>
Means		-1.36	+0.22	+0.04	-0.14	+1.25

## II

		$\beta$ Persei	$\kappa$ Persei	$\omega$ Persei	$l$ Persei	$30$ Persei
(157)	3.	<u>-2.47</u>	-0.56	+0.45	+0.94	+1.64
(159)	4.	-2.38	-0.53	+0.52	+0.90	+1.49
(160)	4.	-2.18	-0.59	+0.27	+0.92	+1.56
(161)	4.	<u>-2.15</u>	<u>-0.55</u>	<u>+0.42</u>	<u>+0.85</u>	<u>+1.43</u>
Means		-2.30	-0.56	+0.42	+0.90	+1.53

## III

	$\alpha$ Persei	76δ Persei	34 Persei	29 Persei	63δ Persei	31 Persei	EXTRA STARS	73δ Persei	77δ Persei
(53) 1.	-3.10	+0.63	+0.55	+0.61	+0.67	+0.62		+0.97	+1.51
(104) 3.	-3.26	+0.37 rej.	+0.42	+0.66	+0.76	+1.04			
(107) 2.	-3.17	+0.80	+0.42	+0.69	+0.53	+0.75			
(108) 4.	-3.16	+0.58	+0.40	+0.57	+0.57	+0.68	+0.88		
(151) 4.	-3.45 rej.	+0.89	+0.23	+0.98	+0.57	+0.77			
(160) 4.	<u>-3.10</u>	<u>+0.59</u>	<u>+0.24</u>	<u>+0.94</u>	<u>+0.47</u>	<u>+0.85</u>			
Means	-3.16	+0.68	+0.37	+0.79	+0.59	+0.78		+0.92	+1.51

## IV

	$\sigma$ Persei	$\psi$ Persei	36 Persei	DM. 47847	DM. 47844	84δ Persei
(151) 4.	-1.24	-1.24	-0.49	+0.44	+1.07	+1.43
(161) 4.	-1.12	-1.06	+0.07	+0.14	+0.76	+1.19
(162) 3.	-1.13	-1.18	+0.07	+0.29	+0.67	
(163) 4.	<u>-1.05</u>	<u>-1.19</u>	<u>-0.01</u>	<u>+0.36</u>	<u>+0.72</u>	<u>+1.17</u>
Means	-1.13	-1.17	-0.09	+0.31	+0.80	+1.26

## V

	$\delta$ Persei	$\lambda$ Persei	A Persei	104δ Persei	EXTRA STAR
(53) 1.	-1.68	-0.16	+0.79	+1.05	
(104) 3.	-1.71	-0.13	+0.82	+1.01	
(109) 2.	-1.65	-0.22	+0.82	+1.04	
(111) 2.	-1.62	-0.04	+0.71	+0.97	
(162) 2.	-1.65	-0.25	+0.96	+0.93	
(164) 4.	-1.58	-0.35	+0.88	+1.06	
Means	-1.65	-0.19	+0.83	+1.01	+1.25

## VI

	v Persei	96δ Persei	99δ Persei	97δ Persei	92δ Persei
(163) 4.	-1.92	+0.20	+0.48	+0.57	+0.71
(164) 2.	-1.86	+0.28	+0.38	+0.38	+0.84
(166) 4.	-2.04	+0.34	+0.28	+0.74	+0.70
(172) 4.	-1.97	+0.33	+0.23	+0.69	+0.73
Means	-1.95	+0.29	+0.34	+0.60	+0.74

## VII

		<i>c Persei</i>	<i>μ Persei</i>	<i>b Persei</i>	<i>d Persei</i>	<i>1205 Persei</i>	EXTRA STAR <i>1216 Persei</i>
(109)	2.	-0.79	-0.45	+0.08	+0.25	+0.92	
(110)	2.	-0.55	-0.54	-0.02	+0.30	+0.83	
(111)	2.	-0.47	-0.46	-0.08	+0.35	+0.67	
(112)	2.	-0.59	-0.55	+0.04	+0.22	+0.79	
(166)	4.	-0.66	-0.69	+0.01	+0.13	+1.19	
(180)	4.	<u>-0.84</u>	<u>-0.60</u>	<u>-0.05</u>	<u>+0.39</u>	<u>+1.08</u>	<u>+1.09</u>
Means		<u>-0.65</u>	<u>-0.54</u>	<u>± 0.00</u>	<u>+0.28</u>	<u>+0.92</u>	<u>+1.09</u>

## VIII

		<i>ψ Persei</i>	<i>e Persei</i>	<i>f Persei</i>	<i>59 Persei</i>	<i>m Persei</i>
(172)	4.	-1.95	-0.58	+0.02	+0.72	+1.81
(173)	2.	-1.90	-0.56	+0.06	+0.57	+1.83
(180)	4.	-1.89	-0.60	-0.17	+0.97	+1.70
(181)	2.	-2.19	-0.62	+0.08	+0.99	+1.75
(185)	3.	<u>-1.71</u>	<u>-0.58</u>	<u>-0.18</u>	<u>+0.79</u>	<u>+1.66</u>
Means		<u>-1.93</u>	<u>-0.59</u>	<u>-0.02</u>	<u>+0.81</u>	<u>+1.75</u>

## IX

	<i>22δ Aurigae</i>	<i>4δ Aurigae</i>	<i>133δ Persei</i>	<i>135δ Persei</i>	EXTRA STAR <i>α Aurigae</i>
(105) 2.	-0.17	-0.08	-0.03	+0.29	-6.62
(110) 2.		-0.12	-0.04	+0.35	
(112) 2.		+0.01	+0.07	+0.12	-6.46
(167) 3.	-0.04	-0.19	0.00	+0.22	-6.34
(178) 2.	-0.34	-0.01	+0.04	+0.31	-5.73 rej.
(181) 2.	<u>-0.22</u>	<u>+0.18</u>	<u>-0.18</u>	<u>+0.21</u>	<u>-6.61</u>
Means	<u>-0.19</u>	<u>-0.04</u>	<u>-0.02</u>	<u>+0.25</u>	<u>-6.51</u>

## X

	<i>η Aurigae</i>	<i>ζ Aurigae</i>	<i>λ Aurigae</i>	<i>ρ Aurigae</i>	<i>37δ Aurigae</i>	<i>34δ Aurigae</i>	<i>6δ Aurigae</i>	<i>8δ Aurigae</i>
(167) 3.	-2.11	-1.64	-0.33	+0.50	+0.65	+0.76	+0.78	+1.35
(174) 3.	-2.00	-1.72	-0.32	+0.48	+0.80	+0.56	+1.00	+1.20
(182) 3.	-2.13	-1.72	-0.30	+0.27	+0.81	+0.69	+0.83	+0.57
(185) 3.	<u>-2.17</u>	<u>-1.62</u>	<u>-0.40</u>	<u>+0.41</u>	<u>+0.83</u>	<u>+0.73</u>	<u>+0.98</u>	<u>+1.24</u>
Means	<u>-2.10</u>	<u>-1.67</u>	<u>-0.34</u>	<u>+0.41</u>	<u>+0.77</u>	<u>+0.66</u>	<u>+0.90</u>	<u>+1.09</u>

EXTRA STAR  
*ε Aurigae*

(167)	-1.54
(174)	-1.49
(182)	-1.65
(185)	<u>-1.54</u>
Mean	<u>-1.55</u>

## XI

		$\pi$ Aurigae	$\circ$ Aurigae	36 Aurigae	85 $\delta$ Aurigae	41 Aurigae
(105)	2.	-1.08 rej.	-0.05	0.00	+0.44	+0.79 rej.
(113)	2.	-1.21	+0.04	+0.07	+0.50	+0.60
(117)	1.	-1.52	+0.27	+0.29	+1.03 rej.	+0.43
(120)	3.	-1.39	+0.10	+0.32	+0.64	+0.35
(158)	2.	-1.28	+0.01	+0.21	+0.58	+0.46
(174)	3.	-1.36	-0.16	+0.24	+0.68	+0.30
(186)	2.	-1.36	+0.03	+0.56 rej.	+0.76	+0.39
Means		-1.35	+0.03	+0.19	+0.57	+0.42

## XII

	$\beta$ Aurigae	$\psi_2$ Aurigae	$\psi_3$ Aurigae	39 Aurigae	38 Aurigae	DM. 43°1421	$\psi_1$ Aurigae	43 Aurigae	42 Aurigae
(175)	3.	-3.21	-0.11	+0.72	+1.12	+1.48	+1.67		
(182)	3.	-3.30	-0.01	+0.57	+1.21	+1.51			
(183)	2.	-3.31	+0.06	+0.64	+1.33	+1.26	+1.57		
(186)	2.	-3.56	+0.09	+0.76	+1.29			-0.21	+1.57
Means		-3.34	+0.01	+0.67	+1.24	+1.42	+1.62	-0.21	+1.57

## XIII

	$\psi_{10}$ Aurigae	$\psi_1$ Aurigae	$\psi_6$ Aurigae	$\psi_9$ Aurigae	130 $\delta$ Aurigae	43 Aurigae	42 Aurigae
(113)	2.	-0.81	-0.72	-0.45	+0.08	+0.43	+0.52
(114)	4.	-0.55	-1.04	-0.56	+0.25	+0.50	+0.60
(175)	3.	-0.82	-1.09	-0.38	+0.40		+0.57
(187)	3.	-0.81	-1.14	-0.56	+0.06		+0.86
Means		-0.75	-1.00	-0.49	+0.20	+0.46	+0.64

## XIV

		$\psi_4$ Aurigae	$\psi_5$ Aurigae	$\psi_7$ Aurigae	66 Aurigae	64 Aurigae
(118)	1.	-0.50	-0.25	-0.19	+0.13	+0.83
(165)	4.	-0.27	-0.01	-0.33	+0.14	+0.73
(183)	2.	-0.32	-0.01	-0.27	-0.09	+0.68
(187)	3.	<u>-0.41</u>	<u>+0.02</u>	<u>-0.38</u>	<u>+0.07</u>	<u>+0.68</u>
Means		-0.35	-0.06	-0.29	+0.06	+0.78

## XV

		21 Lyncis	8H Lyncis	32δ Lyncis	26 Lyncis	16δ Lyncis	26δ Lyncis	23δ Lyncis	25 Lyncis
(114)	4.	-0.81	-0.56	+0.05	+0.04	+0.10	+0.14	+0.23	+0.83
(115)	4.	-0.99	-0.31	+0.31	+0.03	-0.05	-0.03	+0.45	+0.66
(165)	4.	-1.02	-0.50			+0.23		+0.21	
(168)	2.	-0.96	-0.67	+0.51	-0.07	-0.09	+0.09	+0.42	+0.77
(184)	2.	<u>-0.92</u>	<u>-0.54</u>	<u>+0.14</u>	<u>-0.02</u>	<u>+0.10</u>	<u>+0.12</u>	<u>+0.31</u>	<u>+0.79</u>
Means		-0.94	-0.52	+0.25	0.00	+0.06	+0.08	+0.32	+0.76

## EXTRA STARS

	22 Lyncis	27δ Lyncis
(114)	-0.18	
(115)		
(165)	-0.31	
(168)		+0.19
(184)	-0.12	+0.58
Means	-0.20	+0.38

## XVI

		10 Urs. maj.	31 Lyncis	17H Ursae maj.	35 Lyncis	36 Lyncis
(101)	4.	-0.73	-0.55	-0.04	+0.65	+0.69
(115)	4.	-0.67	-0.65	-0.13	+0.50	+0.96
(116)	3.	-1.01	-0.40	-0.03	+0.51	+0.93
(118)	1. rej.	<u>-0.31</u>	<u>-0.48</u>	<u>+0.06</u>	<u>+0.42</u>	<u>+0.31</u>
Means		-0.80	-0.53	-0.07	+0.55	+0.86

## XVII

	$\iota$ Urs. maj.	$\kappa$ Urs. maj.	34 Lyncis	76 $\delta$ Lyncis	25 $\delta$ Urs. maj.	38 $\delta$ Urs. maj.	EXTRA STARS	
							35 Lyncis	75 $\delta$ Lyncis
(116)	3.	-2.13	-1.48	+0.60	+0.82	+0.89	+1.28	
(119)	4.	-1.88	-1.39	+0.46	+0.80	+0.70	+1.34	
(121)	4.	-1.80	-1.35	+0.55	+0.92	+0.43	+1.24	
(168)	2.	-1.94	-1.54	+0.61	+1.00	+0.61	+1.24	+0.36
(184)		<u>-2.04</u>	<u>-1.69</u>	<u>+0.67</u>	<u>+1.04</u>	<u>+0.82</u>	<u>+1.23</u>	<u>+0.36</u>
Means		-1.96	-1.49	+0.58	+0.92	+0.69	+1.27	+0.36
								+1.08
								+1.08

## XVIII

		38 Lyncis	85 $\delta$ Lyncis	19 Leon. min.	42 Lyncis	43 Lyncis
(48)	1.	-1.27	-0.35	+0.24	+0.57	+0.79
(49)	4.	-1.25	-0.12	+0.29	+0.45	+0.62
(101)	4.		-0.22	+0.38	+0.49	+0.68
(119)	4.	-1.27	-0.08	+0.23	+0.38	+0.76
(122)	3.	<u>-1.44</u>	<u>-0.07</u>	<u>+0.41</u>	<u>+0.38</u>	<u>+0.72</u>
Means		-1.31	-0.17	+0.31	+0.45	+0.71

## XIX

	$\theta$ Urs. maj.	65° Urs. maj.	22H Urs. maj.	31 Urs. maj.	26 Urs. maj.	EXTRA STAR DM. 45°1728
(121) 4.	-1.67	+0.28	+0.70	+0.92	+0.23	
(122) 3.	-2.13 rej.	+0.71 rej.	+0.88	+0.72	+0.19	
(170) 1.	-1.62			+0.66	-0.20	+2.40
(176) 3.	-1.73	+0.20	+0.80	+0.66	+0.06	
(178) 4.	<u>-1.72</u>	<u>+0.44</u>	<u>+0.67</u>	<u>+0.71</u>	<u>-0.09</u>	
Means	-1.77	+0.41	+0.77	+0.74	+0.05	+2.40

## XX

	$\lambda$ Urs. maj.	31 Leon. min.	33H Urs. maj.	DM. 41°2076	32 Leon. min.	38 Leon. min.	$\mu$ Urs. maj.
(3) 5.	-0.95 rej.	-0.52		+1.16	+1.20	+1.28	-1.91
(4) 2.	-1.58		+0.09		+1.38		-1.89
(10) 6.	-1.24	-0.30	+0.04	+0.95	+1.06	+1.17	-1.70
(48) 1.	-1.33	-0.63	+0.10	+0.61 rej.	+1.25	+2.13 rej.	-1.99
(49) 4.	-1.51	-0.61	+0.12	+1.24	+1.37	+1.55	-2.14
(50) 4.	-1.46	-0.60	+0.21	+1.26	+1.34	+1.42	-2.17
(170) 2.	-1.61	-0.74	-0.09	+1.46	+1.49	+1.66	-2.15
(176) 3.	<u>-1.34</u>	<u>-0.57</u>	<u>+0.12</u>	<u>+1.22</u>	<u>+1.36</u>	<u>+1.15</u>	<u>-1.97</u>
Means	-1.44	-0.54	+0.10	+1.21	+1.31	+1.37	-1.98

## EXTRA STARS

	Comp. $\lambda$ .	25 Leon. min.	51 Urs. maj.	35 Leon. min.	foll. $\mu$	39° Urs. maj.	36H Urs. maj.	$\chi$ Urs. maj.	136° Urs. maj.
(3)	2 +2.13	2 +2.28	+1.61	+1.02		+0.33	+0.17	-1.97	+0.88
(4)									
(10)									
Means	<u>+2.13</u>	<u>+2.28</u>	<u>+1.61</u>	<u>+1.02</u>	<u>+1.11</u>	<u>+0.33</u>	<u>+0.17</u>	<u>-1.97</u>	<u>+0.88</u>

## XXI

	$\psi$	<i>Urs. maj.</i>	36H	<i>Urs. maj.</i>	39H	<i>Urs. maj.</i>	90 $\delta$	<i>Urs. maj.</i>	DM. 49°1960	EXTRA STARS
(50)	4.	-2.92		-0.04		+0.22		+1.21		+1.55
(54)	4.	-2.85		-0.19		+0.11		+1.28		+1.66
(169)	2.	-3.05		-0.15		0.00				+1.02 rej.
(171)	3.	-2.74		-0.26		-0.10		+1.41		+1.70
(177)	2.	-2.39 rej.		-0.10		+0.09		+1.25		+1.61
(178)	4.	-2.76		-0.27		+0.20		+1.20		+1.63
(189)	2.	<u>-2.99</u>		<u>-0.06</u>		<u>+0.12</u>		<u>+1.31</u>		<u>+1.60</u>
Means		-2.88		-0.15		+0.09		+1.19		+1.62
										+0.73
										+2.37

## XXII

		56	<i>Urs. maj.</i>	47	<i>Urs. maj.</i>	49	<i>Urs. maj.</i>	57	<i>Urs. maj.</i>	59	<i>Urs. maj.</i>	58	<i>Urs. maj.</i>	122 $\delta$	<i>Urs. maj.</i>
(3)	5.			-0.24		-0.38									+0.52
(10)	6.	-0.51		-0.47		-0.33		-0.04		+0.20		+0.59		+0.55	
(58)	4.	-0.60		-0.34		-0.37		+0.08		+0.03		+0.59		+0.61	
(171)	3.	-0.32		-0.48		-0.45		-0.24		+0.18		+0.33		+0.98	
(179)	4.	-0.55		-0.53		-0.24				+0.17		+0.55		+0.67	
(188)	3.	<u>-0.55</u>		<u>-0.50</u>		<u>-0.39</u>		<u>-0.13</u>		<u>-0.03</u>		<u>+0.82</u>		<u>+0.80</u>	
Means		-0.51		-0.43		-0.36		-0.08		+0.11		+0.59		+0.72	

## EXTRA STARS

	$\omega$ Urs. maj.	DM. 43°2069	DM. 42°2162	DM. 41°2093	DM. 41°2097	130° Urs. maj.	DM. 40°2432
(3)	-0.39	+1.19	+0.53	+0.31	+1.32	+0.49	
(10)	-0.71					+1.99	
(58)	-0.83	+1.49	+0.64				
(171)	-0.60		+0.75				
(179)	-0.83						
Means	-0.67	<u>+1.34</u>	<u>+0.64</u>	<u>+0.31</u>	<u>+1.32</u>	<u>+0.49</u>	<u>+1.99</u>

## XXIII

	$\gamma$ Urs. maj.	$\chi$ Urs. maj.	3 Can. Ven.
(5) 5.	-1.55	-0.32	
(15) 4. rej.	-1.32	-0.09	+1.41
(54) 8.	-1.53	6 -0.13	5 +1.71
(62) 2.	-1.52	-0.39	+1.92
(65) 4.	-1.66	-0.29	+1.94
(179) 4.	-1.50	8 -0.37	
(188) 3.	<u>-1.82</u>	<u>6 -0.16</u>	<u>+1.99</u>
Means	-1.61	-0.27	<u>+1.89</u>

## EXTRA STARS

	1585 Urs. maj.	65p. Urs. maj.	65sq. Urs. maj.	60 Urs. maj.	DM. 49°2110?	DM. 49°2132?	65 Urs. maj.
(5)	+2.78	+2.03 rej.	+2.73				
(15)		+3.37					
(54)	5 +2.65	5 +3.48		5 +2.73	1 +3.30	1 +3.36	
(62)		+3.22					
(65)	+2.89	+3.59					
(179)	+2.71						+2.59
(188)							+3.02
Means	<u>+2.75</u>	<u>+3.43</u>	<u>+2.73</u>	<u>+2.73</u>	<u>+3.30</u>	<u>+3.36</u>	<u>+2.80</u>

## XXIV

	6 Can. Ven.	67 Urs. maj.	2 Can. Ven.	4 Can. Ven.	11 <sup>5</sup> Can. Ven.
(1)	5. rej.	-1.07	-0.13		
(2)	5.	-0.54	-0.63	-0.13	+0.71
(15)	3.	-0.66	-0.16 rej.	0.00	+0.33
(55)	4.	-0.64	-0.31	-0.17	+0.33
(58)	4.	-0.77	-0.44	-0.14	+0.41
(124)	1.	-0.77	-0.67	+0.03	+0.36
(126)	4.	-0.74	-0.47	-0.13	+0.39
Means	<u>-0.69</u>	<u>-0.54</u>	<u>-0.08</u>	<u>+0.40</u>	<u>+0.88</u>

## EXTRA STARS

	Comp. of 67 Urs. maj.	DM. 41°2251	DM. 41° 2252	1795 Urs. maj.	DM. 40°2485?	DM. 43°2221
(2)	+0.89	2 +1.13	2 +0.57	2 +0.71	2 +1.48	+1.07
(55)			+1.57	+1.10		
(58)	<u>+1.15</u>					
Means	<u>+1.15</u>	<u>+1.13</u>	<u>+1.57</u>	<u>+1.10</u>	<u>+1.48</u>	<u>+1.07</u>

## XXV

	26° Can. Ven.	42° Can. Ven.	32° Can. Ven.	36° Can. Ven.	23° Can. Ven.	11 Can. Ven.	29° Can. Ven.
(6) 5.	-0.77	-0.41	-0.09	+0.03	+0.29	+0.38	+0.58
(65) 4.	-1.09	-0.40	-0.30	+0.15	+0.40	+0.61	+0.66
(67) 4.	-0.87	-0.53	-0.27	+0.14	+0.43	+0.53	+0.58
(124) 1.	-0.66	-0.09	-0.41	+0.28	-0.11 rej.	+0.36	
(126) 4.	<u>-0.97</u>	<u>-0.29</u>	<u>-0.24</u>	<u>+0.24</u>	<u>+0.37</u>	<u>+0.28</u>	
Means	<u>-0.87</u>	<u>-0.34</u>	<u>-0.26</u>	<u>+0.17</u>	<u>+0.37</u>	<u>+0.43</u>	<u>+0.61</u>

EXTRA STAR  
21° Can. Ven.

(6)	
(65)	
(67)	+1.05
(124)	
(126)	
Mean	<u>+1.05</u>

## XXVI

	12 Can. Ven.	8 Can. Ven.	6H Can. Ven.	10 Can. Ven.	9 Can. Ven.	40° Can. Ven.
(1)					+1.38	
(5) 5.	-1.71	-0.41	+1.14	+0.97	+1.55	
(6) 5.	-1.73	-0.38	+0.98	+1.15	+1.50	
(7) 3.	-1.50 rej.	-0.26	+1.03	+1.18	+1.32	
(55) 4.	-2.16	-0.52	+1.36	+1.30	+1.62	+2.23
(127) 2.	<u>-2.24</u>	<u>-0.55</u>	<u>+1.43</u>	<u>+1.37</u>	<u>+1.79</u>	
Means	<u>-1.96</u>	<u>-0.42</u>	<u>+1.19</u>	<u>+1.19</u>	<u>+1.53</u>	<u>+2.23</u>

## EXTRA STARS

## XXVII

	24 Can. Ven.	21 Can. Ven.	58δ Can. Ven.	57δ Can. Ven.	214δ Urs. maj.	EXTRA STARS	66δ Can. Ven.	56δ Can. Ven.
(67) 4.	-1.10	-0.29	+0.41	+0.99	+0.89		+0.87	+1.04
(125) 4.	-0.89	-0.28	+0.38	+0.80				
(127) 2.	-0.74	-0.39	+0.46	+0.68				
(128) 4.	-0.84	-0.35	+0.28	+0.92				
Means	-0.89	-0.33	+0.38	+0.85	+0.89		+0.87	+1.11 +1.07

## XXVIII

	20 Can. Ven.	11H Can. Ven.	23 Can. Ven.	17 Can. Ven.	19 Can. Ven.	15 Can. Ven.
(5) 5.	-0.91	-0.53	+0.05	+0.33	+0.39	+0.67
(7) 3.	-0.63	-0.79	+0.08	+0.42	+0.22	+0.71
(59) 1.	-0.75	-0.75	+0.07	+0.30	+0.48	+0.66
(60) 3.	-0.92	-0.84	+0.09	+0.67	+0.19	+0.82
(64) 4.	-1.00	-0.69	+0.09	+0.55	+0.34	+0.69
(128) 4.	-0.87	-0.74	+0.01	+0.43	+0.38	+0.77
Means	-0.85	-0.72	+0.06	+0.45	+0.33	+0.72

## XXIX

	η Urs. maj.	60δ Can. Ven.	73δ Can. Ven.	175B Can. Ven.	EXTRA STARS	DM. 42°2403	150B Can. Ven.
(64) 4.	-4.22	+0.97	+1.39	+1.87		+2.71	
(66) 4.	-4.29	+0.87	+1.61	+1.81			
(125) 4.	-3.70	+0.66	+1.49	+1.57			
(130) 4.	-3.75	+0.71	+1.56	+1.48			
Means	-3.99	+0.80	+1.51	+1.58	+2.71		+0.80 +0.80

## XXX

	76° Can. Ven.	DM. 39°2678	75° Can. Ven.	72° Can. Ven.	59° Can. Ven.	62° Can. Ven.	78° Can. Ven.
(59) 1.	-0.55	-0.31	-0.15		+0.23	+0.36	
(60) 3.	-0.67	-0.21	-0.17	-0.03	+0.24	+0.31	+0.55
(61) 4.	-0.61	-0.32	-0.16		+0.46	+0.47	+0.17
(66) 4.	-0.74	-0.34	-0.24	+0.21	+0.37	+0.32	+0.39
(129) 1.	-0.54	-0.13	+0.10	+0.14		+0.43	+0.35 rej.
(132) 1.	-0.75	-0.40	+0.51	-0.25	+0.37	+0.07	+0.46
(134) 1. rej.			-0.55	+0.02			+0.77
(136) 2.	<u>-0.66</u>	<u>-0.14</u>	<u>-0.11</u>	<u>+0.16</u>	<u>+0.16</u>	<u>+0.30</u>	<u>+0.32</u>
Means	<u>-0.65</u>	<u>-0.26</u>	<u>-0.10</u>	<u>+0.04</u>	<u>+0.30</u>	<u>+0.32</u>	<u>+0.44</u>

EXTRA STAR  
DM. 41°2423

(59)	+2.07
(60)	+1.73
(61)	
(66)	
(129)	
(132)	
(134)	
(136)	
Mean	<u>+1.90</u>

## XXXI

	$\theta$ Bootae	$\lambda$ Bootae	$\kappa$ Bootae	$\iota$ Bootae	13 Bootae	23 $\delta$ Bootae
(117) 1.	-0.56	-0.48	-0.04	+0.06	+0.45	+0.55
(120) 3.	-0.88	-0.71	-0.08	+0.24	+0.55	+0.91
(130) 4.	-0.58	-0.66	-0.27	+0.18	+0.80	+0.54
(131) 4.	-0.56	-0.45	-0.24	+0.19	+0.59	+0.49
(132) 1.	-0.30	-0.54	-0.23	+0.05	+0.60	+0.41
(133) 2.	-0.29	-0.60	-0.33	-0.07	+0.35	+0.95
(134) 1.	-0.47	-0.49	-0.16	+0.21	+0.51	+0.39
(136) 2.	-0.57	4 -0.29	-0.26	-0.14	6 +0.73	+0.54
(137) 2.	-0.53	2 -0.50	+0.08	+0.12	6 +0.52	+0.32
Means	-0.52	-0.52	-0.17	+0.09	+0.57	+0.57

## EXTRA STARS

	22 $\delta$ Bootae	87 $\delta$ Can. Ven.	DM. 51°19'08"	g Bootae	69 $\delta$ Bootae
(117)	+1.86	+1.99			
(120)	+1.63		+1.42		
(130)		+1.79		+1.10	+1.36
(131)		+1.40 rej.		+1.21	+1.51
(132)		+2.07			
Means	<u>+1.74</u>	<u>+1.95</u>	<u>+1.42</u>	<u>+1.15</u>	<u>+1.43</u>

## XXXII

	$\gamma$ Bootae	32 $\delta$ Bootae	34 $\delta$ Bootae	42 $\delta$ Bootae	EXTRA STAR 50 $\delta$ Bootae
(8) 4.	-2.37	+0.73	+0.74	+0.92	+0.38
(61) 4.	-2.90		+1.08	+1.01	+0.66
(133) 2.	-2.47	+0.89	+0.75	+0.82	
(138) 4.	-2.86	+0.84	+1.08	+0.93	+0.86
Means	-2.65	+0.82	+0.91	+0.92	+0.63

## XXXIII

	i Bootae	39 Bootae	k Bootae	33 Bootae	h Bootae	EXTRA STAR 112 $\delta$ Bootae
(137) 2.	-0.83	+0.20	+0.14	-0.16	+0.65	+1.04
(138) 4.	-0.73	+0.13	+0.50	-0.13	+0.25	+0.83
(139) 3.	-0.85	+0.29	+0.26	-0.07	+0.35	+1.34
(140) 3.	-0.86	+0.26	+0.37	-0.15	+0.40	+1.29
Means	-0.82	+0.22	+0.32	-0.13	+0.41	+1.12

## XXXIV

	$\beta$ Bootae	$\mu$ Bootae	$\nu^2$ Bootae	$\nu^1$ Bootae	$\varphi$ Bootae	134 $\delta$ Bootae	EXTRA STAR Comp. to $\mu$
(8) 4.	-1.35	-0.22	+0.23	+0.10	+0.40	+0.85	
(9) 3.	-1.07	-0.22	+0.20	+0.08	+0.27	+0.72	
(56) 2.	-1.65	-0.59	+0.50	+0.19	+0.62	+0.93	+1.90
(139) 3.	-1.70	-0.42	+0.40	+0.29	+0.58	+0.86	
Means	-1.44	-0.36	+0.33	+0.16	+0.47	+0.84	+1.90

## XXXV

		$\tau$ <i>Herculis</i>	$\varphi$ <i>Herculis</i>	$v$ <i>Herculis</i>	$16\delta$ <i>Herculis</i>
(9)	3.	-0.40 rej.	-0.43	-0.26	2 +1.07
(18)	4.	-0.85	-0.42	+0.01	+1.24
(31)	4.	-0.65	-0.30	0.00	+0.97
(140)	3.	<u>-1.02</u>	<u>-0.47</u>	<u>0.00</u>	<u>+1.51</u>
Means		<u>-0.84</u>	<u>-0.40</u>	<u>-0.06</u>	<u>+1.20</u>

## XXXVI

		$\chi$ <i>Herculis</i>	2 <i>Herculis</i>	23 $\delta$ <i>Herculis</i>	4 <i>Herculis</i>	EXTRA STARS	19 $\delta$ <i>Herculis</i>
(20)	4.	-0.68	-0.35	+0.35	+0.69	g <i>Herculis</i>	19 $\delta$ <i>Herculis</i>
(23)	4.	-0.77	-0.08	+0.24	+0.60	-0.70	-0.64
(31)	4.	-0.75	+0.01	+0.21	+0.53	-0.14	+0.52
(56)	2.	<u>-1.22</u>	<u>+0.21</u>	<u>+0.22</u>	<u>+0.81</u>	<u>-0.65</u>	
Means		<u>-0.85</u>	<u>-0.05</u>	<u>+0.25</u>	<u>+0.66</u>	<u>-0.53</u>	<u>+0.52</u>

## XXXVII

		42 <i>Herculis</i>	52 <i>Herculis</i>	40 $\delta$ <i>Herculis</i>	46 $\delta$ <i>Herculis</i>	EXTRA STARS	Comp. of 42
(11)	2.	-0.74	-0.18	+0.57	+0.35	78 $\delta$ <i>Herculis</i>	+0.52
(18)	4.	-0.73	-0.41	+0.54	+0.59		
(20)	4.	-0.76	-0.54	+0.60	+0.70	+0.65	
(57)	2.	<u>-0.73</u>	<u>-0.38</u>	<u>+0.47</u>	<u>+0.64</u>		+1.02
Means		<u>-0.74</u>	<u>-0.38</u>	<u>+0.54</u>	<u>+0.57</u>	<u>+0.58</u>	<u>+1.02</u>

## XXXVIII

		$\eta$ <i>Herculis</i>	$\sigma$ <i>Herculis</i>	100 $\delta$ <i>Herculis</i>	121 $\delta$ <i>Herculis</i>	98 $\delta$ <i>Herculis</i>	EXTRA STAR g <i>Herculis</i>
(23)	4.	-1.48	-0.92	+0.11	+0.60	+1.57	
(57)	2.	3 -1.49	3 -0.87	+0.09	3 +0.79	+1.47	
(63)	4.	-1.89	-0.83	+0.18	+0.79	+1.73	-0.13
(70)	3.	<u>-1.85</u>	<u>-0.56</u>	<u>+0.16</u>	<u>+0.81</u>	<u>+1.42</u>	
Means		-1.68	-0.79	+0.13	+0.75	+1.55	<u>-0.13</u>

## XXXIX

	$\iota$ <i>Herculis</i>	y <i>Herculis</i>	74 <i>Herculis</i>	x <i>Herculis</i>	DM. 49°2604
(11)	2.	-1.22	-0.05	+0.12	+0.41
(12)	1.	-1.12 rej.	+0.16	-0.07	+0.57
(13)	6.	-1.48	+0.01	+0.12	+0.32
(70)	3.	-1.76	+0.12	+0.28	+0.63
(71)	3.	-1.56	+0.13	+0.08	+0.48
(72)	4.	-1.60	<u>+0.12</u>	<u>+0.06</u>	<u>+0.47</u>
Means		-1.53	<u>+0.08</u>	<u>+0.10</u>	<u>+0.48</u>

## EXTRA STARS

	120 $\delta$ <i>Herculis</i>	95 $\delta$ <i>Draconis</i>	118 $\delta$ <i>Herculis</i>	DM. 44°2695	DM. 45°2504	99 $\delta$ <i>Herculis</i>	95 $\delta$ <i>Herculis</i>
(11)	+1.17	+1.22	+1.04	+1.41	+1.41	1 +1.79 rej.	1 +0.41 rej.
(12)	+1.48 rej.		+1.33				
(13)	+1.00	+1.36				+1.29	+0.86
(71)	+1.08		+1.11				
Means	+1.08	<u>+1.29</u>	<u>+1.16</u>	<u>+1.41</u>	<u>+1.41</u>	<u>+1.29</u>	<u>+0.86</u>

## XL

	<i>θ Herculis</i>	<i>f Herculis</i>	<i>135° Herculis</i>	<i>157° Herculis</i>	<i>156° Herculis</i>
(27) 4.	-2.19	-0.28	+0.54	+0.79	+1.15
(63) 4.	-2.07	-0.37	+0.30	+1.04	+1.12
(68) 4.	-2.42	-0.36	+0.36	+1.16	+1.25
(71) 3.	<u>-2.09</u>	<u>-0.23</u>	<u>+0.39</u>	<u>+0.86</u>	<u>+1.08</u>
Means	-2.19	-0.31	+0.40	+0.96	+1.15

## EXTRA STARS

	DM. 41°2882	110° Herculis.	186° Herculis	DM. 40°3225
(27)	2 +1.88	2 +3.41		
(63)			+0.53	+1.24
(68)				
(71)			+0.46	+1.35
Means	<u>+1.88</u>	<u>+3.41</u>	<u>+0.49</u>	<u>+1.29</u>

## XLI

	<i>169° Herculis</i>	<i>164° Herculis</i>	<i>179° Herculis</i>	<i>z Herculis</i>
(13) 6.	-0.27	-0.10	+0.02	+0.34
(19) 4.	-0.33	+0.01	+0.10	+0.21
(27) 4.	-0.07	-0.03	-0.12	+0.23
(62) 2.	-0.05	+0.12	-0.20	+0.64 rej.
(73) 3.	-0.08	-0.10	+0.13	+0.03
(76) 4.	<u>-0.11</u>	<u>-0.03</u>	<u>-0.16</u>	<u>+0.29</u>
Means	-0.15	-0.04	-0.04	+0.22

## EXTRA STARS

	182 $\delta$ <i>Herculis</i>	DM. 47°2541	DM. 45°2626	DM. 45°2627	DM. 45°2629
(13)	-1.06	+0.31	+0.36	-0.68	+0.12
(19)		+0.17	+0.18	-0.94	+0.24
(27)	-0.98			-0.39	
(62)	-1.04	+0.30	+0.18	-0.15	+0.66 rej.
(73)	-0.88	+0.20			
(76)	-1.08			-0.46	
Means	<u>-1.01</u>	<u>+0.24</u>	<u>+0.24</u>	<u>-0.52</u>	<u>+0.18</u>

## XLII

	$\mu$ <i>Lyrae</i>	3 $\delta$ <i>Lyrae</i>	2 $\delta$ <i>Lyrae</i>	4 $\delta$ <i>Lyrae</i>	EXTRA STARS
					196 $\delta$ <i>Herculis</i>
(25) 4.	-0.56	-0.15	+0.27	+0.45	-0.53
(68) 4.	-0.63	-0.13	+0.35	+0.39	-0.98
(73) 3.	-0.61	-0.02	+0.16	+0.46	-0.77
(74) 1.	-1.13 rej.	+0.14	+0.67 rej.	+0.23	-1.02
(77) 1.	-0.90	+0.37 rej.	+0.01 rej.	+0.63	+0.43
(78) 1.	-0.95	-0.08	+0.54	+0.46	-0.65
(85) 4.	-0.55	-0.14	+0.33	+0.37	-0.90
Means	-0.70	-0.06	+0.33	+0.43	+0.44
					-0.81
					+0.44

## XLIII

	<i>143δ Draconis</i>	<i>164δ Draconis</i>	<i>185δ Herculis</i>	<i>202δ Herculis</i>		<b>EXTRA STARS</b>		
	(17)	4.	-1.31	+0.23	+0.34	+0.74	+0.71	DM. 49°27'28"
(19)	4.	-1.51	+0.04	+0.37	+0.74	+0.72	+0.31	+0.43
(25)	4.	-1.14	+0.09	+0.32	+0.73	+0.50		
(88)	1.						+0.37	
(92)	4.	<u>-1.02</u>	<u>+0.21</u>	<u>+0.23</u>	<u>+0.59</u>		<u>+0.47</u>	<u>-0.11</u>
Means		<u>-1.24</u>	<u>+0.14</u>	<u>+0.31</u>	<u>+0.70</u>	<u>+0.66</u>	<u>+0.38</u>	<u>+0.43</u>

## XLIV

	<i>ε. n. Lyrae</i>	<i>ε. s. Lyrae</i>	<i>36δ Lyrae</i>	<i>25δ Lyrae</i>	<i>31δ Lyrae</i>
(16)	4.	-0.99	-0.78	+0.57	+0.75
(21)	3.	-0.64	-0.73	-0.10	+0.49
(22)	3.	-0.76	-0.95	+0.10	+0.72
(74)	1.	-0.78	-0.80	-0.04	+1.05 rej.
(85)	4.	-0.78	-0.78	+0.02	+0.71
(92)	4.	<u>-0.76</u>	<u>-0.88</u>	<u>+0.04</u>	<u>+0.72</u>
Means		<u>-0.79</u>	<u>-0.83</u>	<u>+0.07</u>	<u>+0.68</u>

## XLV

	<i>16 Lyrae</i>	<i>171δ Draconis</i>	<i>1δ Cycni</i>	<i>2δ Cycni</i>	<i>R Lyrae</i>	DM. 49°2977	DM. 49°2779	EXTRA STARS	<i>ζ. n. Lyrae</i>	<i>ζ. s. Lyrae</i>
(16)	4.	-0.71	+0.09	+0.19	+0.43	-0.93				
(17)	4.	-0.67	+0.18	+0.35	+0.14	-0.97				
(22)	3.	-0.74	+0.13	+0.33	+0.29	-1.42	+0.47			
(28)	4.	-0.59	0.00	+0.35	+0.23	-1.58		+0.74	-0.54	+0.04
(86)	4.	<u>-0.80</u>		<u>+0.41</u>	<u>+0.30</u>	<u>-1.89</u>				
Means		<u>-0.70</u>	<u>+0.10</u>	<u>+0.33</u>	<u>+0.28</u>	<u>-1.56</u>	<u>+0.47</u>	<u>+0.74</u>	<u>-0.54</u>	<u>+0.04</u>

## XLVI

	<i>θ Lyrae</i>	<i>η Lyrae</i>	<i>69δ Lyrae</i>	<i>43δ Lyrae</i>	<i>52δ Lyrae</i>	<i>17δ Cycni</i>	<i>14 Cycni</i>	<i>37δ Cycni</i>	
(21)	2.			+0.54	+0.66	+1.11	-0.03	+0.04	+0.33
(24)	3.			+0.45	+0.83	+1.07	-0.03	+0.02	+0.32
(28)	4.	-1.36	-1.25	+0.37	+1.00	+0.98			
(30)	4.	<u>-1.53</u>	<u>-1.14</u>	<u>+0.34</u>	<u>+0.93</u>	<u>+1.20</u>	<u>-0.07</u>	<u>+0.02</u>	<u>+0.26</u>
Means		<u>-1.44</u>	<u>-1.19</u>	<u>+0.42</u>	<u>+0.85</u>	<u>+1.09</u>	<u>-0.04</u>	<u>+0.03</u>	<u>+0.30</u>

## EXTRA STARS

*49δ Cycni**52δ Cycni*

(21)	+0.35	
(24)	+0.06	+1.05
(28)		
(30)		
Means	<u>+0.20</u>	<u>+1.05</u>

## XLVII

	$\delta$ Cycni.	$\theta$ Cycni.	26 $\delta$ Cycni	20 $\delta$ Cycni	EXTRA STARS	7 $\delta$ Cycni
(24) 3.	-1.88	+0.37	+0.72	+0.79	DM. 44°31'33"	
(26) 4.	-1.58	-0.06	+0.82	+0.93		
(86) 4.	-1.59	+0.15	+0.69	+0.74	+2.45	
(93) 4.	<u>-1.93</u>	<u>+0.21</u>	<u>+0.90</u>	<u>+0.81</u>	<u>+2.84</u>	<u>+3.13</u>
Means	<u>-1.74</u>	<u>+0.17</u>	<u>+0.78</u>	<u>+0.82</u>	<u>+2.64</u>	<u>+3.13</u>

## XLVIII

	17 $\delta$ Cycni	14 Cycni	37 $\delta$ Cycni	49 $\delta$ Cycni.	52 $\delta$ Cycni.	27 $\delta$ Cycni.	EXTRA STAR DM. 41°34'69"
(30) 4.	-0.46	-0.37	-0.13	-0.29	+0.85	+0.40	-0.56
(33) 2.	-0.36	-0.58	-0.21	-0.26	+0.96	+0.45	-0.24
(93) 4.	-0.42	-0.38	-0.09	-0.30	+0.72	+0.47	
(95) 4.	<u>-0.45</u>	<u>-0.51</u>	<u>-0.03</u>	<u>-0.40</u>	<u>+0.93</u>	<u>+0.45</u>	<u>-0.10</u>
Means	<u>-0.42</u>	<u>-0.46</u>	<u>-0.11</u>	<u>-0.31</u>	<u>+0.86</u>	<u>+0.44</u>	<u>-0.30</u>

## XLIX

	e	<i>Cycni</i>	39°	<i>Cycni.</i>	56°	<i>Cycni</i>	40°	<i>Cycni</i>	34°	<i>Cycni.</i>	DM. 47°2945
(14)	4.	-0.77		-0.22		-0.04		+0.21		+0.26	+0.58
(26)	4.	-0.77		+0.01		+0.04		+0.08		+0.22	+0.43
(35)	2.	-0.78		-0.18		-0.02		+0.27		+0.22	+0.47
(95)	4.	-1.03		-0.08		+0.19		+0.04		+0.32	+0.55
Means		-0.84		-0.12		+0.04		+0.15		+0.25	+0.51

## EXTRA STARS

	DM. 47°2937.	DM. 47°3037.	61°	<i>Cycni.</i>
(14)	+0.94	+0.79		
(26)			+0.33	
(35)	+0.33		+0.41	
(95)	+0.68		+0.45	
Means	+0.65	+0.79	+0.40	

## L

	γ	<i>Cycni.</i>	77°	<i>Cycni.</i>	114°	<i>Cycni</i>	87°	<i>Cycni.</i>	109°	<i>Cycni</i>	EXTRA STARS	80°	<i>Cycni.</i>	DM. 40°4136
(29)	2.	-2.87		+0.24		+0.47		+0.97		+1.19				
(33)	2.	-2.81		+0.22		+0.80		+0.75		+1.05		+1.20		+1.52
(35)	2.	-3.01		+0.24		+0.66		+0.80		+1.31		+0.95		+1.40
(39)	2.	-2.83		+0.09		+0.89		+0.78		+1.09		+0.99		+1.45
Means		-2.88		+0.20		+0.70		+0.82		+1.16		+1.05		+1.46

## LI

	$\sigma^1$ sq. <i>Cycni</i>	$\sigma^2$ <i>Cycni</i>	$\sigma^1$ (pr.) <i>Cycni</i>	86° <i>Cycni.</i>	76° <i>Cycni.</i>	84° <i>Cycni.</i>	EXTRA STAR DM. 45°3139
(14) 4.	-1.48	-1.20	-0.11	+0.40	+0.81	+1.58	+1.74
(32) 4.	-1.61	-1.26	-0.03	+0.51	+0.88	+1.48	+1.46
(37) 4.	-1.47	-1.25	-0.09	+0.40	+0.82	+1.60	+1.49
(39) 2.	-1.31	-1.34	-0.12	+0.47	+0.65	+1.66	+1.23
(87) 1.	-1.29	-1.20	-0.07	+0.38	+0.80	+1.36	+1.07
(99) 4.	-1.36	-1.41	-0.19	+0.45	+1.09	+1.40	+1.81
(131) 4.	-1.52	-1.21	± 0.00	+0.47	+1.05	+1.22	+1.12
(155) 2.	-1.51	-1.32	-0.20	+0.79 rej.	+1.14	+1.42	
Means	-1.44	-1.28	-0.10	+0.44	+0.91	+1.47	+1.43

## LII

	$v$ <i>Cycni.</i>	57 <i>Cycni.</i>	56 <i>Cycni</i>	132° <i>Cycni</i>	139° <i>Cycni</i>	110° <i>Cycni.</i>	EXTRA STAR DM. 44°3639
(29) 2.	-1.35	-0.16	-0.05	+0.38	+0.46	+0.71	
(34) 2.	-1.24	-0.25	-0.10	+0.30	+0.50	+0.80	
(37) 4.	-1.10	-0.29	-0.09	+0.29	+0.37	+0.85	+0.83
(41) 3.	-1.07	-0.23	-0.02	+0.20	+0.33	+0.79	+0.96
Means	-1.19	-0.23	-0.06	+0.29	+0.41	+0.79	+0.89

## LIII

	$\alpha$	<i>Cycni</i>	55	<i>Cycni</i>	$\omega^3$	<i>Cycni</i>	$\omega^2$	<i>Cycni</i>	51	<i>Cycni</i>	$\omega^1$	<i>Cycni</i>	104	$\delta$	<i>Cycni</i>	EXTRA STARS	123	$\delta$	<i>Cycni</i>	118	$\delta$	<i>Cycni</i>
(32)	4.	-3.72		+0.15		+0.26		+0.41		+0.83		+0.98		+1.07		+0.76						
(34)	3.	-3.46	2	-0.16		+0.56		+0.25		2	+0.85		+0.96		+1.02		2	+0.68				
(42)	1.	-3.54		-0.01		+0.35		+0.30			+0.71		+1.11		+1.05			+0.58		+2.25		
(43)	2.	-3.79		+0.11		+0.61		+0.24			+0.80		+0.84		+1.21			+0.79				
(87)	1.	-4.00		-0.07		+0.72		+0.24			+1.94		+0.99		+1.17			+0.78				
(94)	4.	-4.00		<u>+0.12</u>		<u>+0.82</u>		<u>+0.18</u>			<u>+0.79</u>		<u>+1.00</u>		<u>+1.11</u>			<u>+1.05</u>				
Means		-3.75		<u><math>\pm</math> 0.00</u>		<u>+0.56</u>		<u>+0.27</u>			<u>+0.82</u>		<u>+0.97</u>		<u>+1.10</u>			<u>+0.77</u>			<u>+2.25</u>	

## LIV

	$\tau$	<i>Cycni</i>	$\sigma$	<i>Cycni</i>	162	$\delta$	<i>Cycni</i>	A	<i>Cycni</i>	68	<i>Cycni</i>	$\xi$	<i>Cycni</i>
(38)	4.	-1.14		-0.56		+1.40			+1.49		+0.18		-1.35
(41)	2.	-1.07		-0.53		+1.32			+1.32		+0.20		-1.22
(42)	1.								+1.51		+0.26		-0.71
(43)	2.	-1.21		-0.85		+1.42			+1.38		+0.48		-1.22
(123)	3.	<u>-1.32</u>		<u>-0.97</u>					<u>+1.56</u>		<u>+0.32</u>		<u>-0.98</u>
Means		-1.18		-0.73					<u>+1.38</u>		<u>+1.45</u>		<u>-1.10</u>

## LV

		<i>f<sup>2</sup> Cycni</i>	<i>f<sup>1</sup> Cycni</i>	<i>60 Cycni</i>	<i>134<sup>b</sup> Cycni</i>	<i>164<sup>b</sup> Cycni</i>	<i>168<sup>b</sup> Cycni</i>	EXTRA STAR <i>140<sup>b</sup> Cycni</i>
(94)	4.	-1.32	-0.24	+0.02	+0.33	+0.51	+0.73	
(96)	3.	-1.07	-0.07	-0.01	+0.41	+0.28	+0.45	+0.27
(123)	3.	-1.11	-0.05	+0.08	+0.46	+0.08	+0.56	-0.02
(135)	3.	<u>-1.00</u>	<u>-0.14</u>	<u>+0.09</u>	<u>+0.43</u>	<u>+0.37</u>	<u>+0.24</u>	<u>+0.32</u>
Means		-1.12	-0.12	+0.04	+0.41	+0.31	+0.49	+0.19

## LVI

		<i>75 Cycni</i>	<i>74 Cycni</i>	<i>189<sup>b</sup> Cycni</i>	<i>77 Cycni</i>	<i>76 Cycni</i>	EXTRA STAR <i>195<sup>b</sup> Cycni</i>
(38)	4.	-0.43	-0.20	-0.32	+0.26	+0.69	
(44)	3.	-0.51	-0.51	-0.03	+0.36	+0.68	
(46)	4.	-0.54	-0.41	-0.03	+0.49	+0.50	+1.25
(135)	3.	<u>-0.34</u>	<u>-0.31</u>	<u>-0.16</u>	<u>+0.30</u>	<u>+0.51</u>	<u>+1.26</u>
Means		-0.45	-0.38	-0.13	+0.35	+0.58	+1.25

## LVII

		$\rho$ <i>Cycni</i>	$\pi^2$ <i>Cycni.</i>	173 $\delta$ <i>Cycni</i>	g <i>Cycni</i>	171 $\delta$ <i>Cycni</i>	EXTRA STARS	
(36)	3.	-0.82	-0.61	+0.33	+0.33	+0.78		
(44)	3.	-1.01	-0.74	+0.46	+0.37	+0.90	+1.12	
(83)	3.	-0.99	-0.74	+0.53	+0.43	+0.79		
(96)	3.	-0.94	-0.74	+0.47	+0.35	+0.86		
(141)	1.	-1.20	2 -0.70	+0.58	+0.37	+0.91		-0.04 rej.
(142)	4.	-1.23	-0.73	+0.51	+0.47	+0.97		+0.27
Means		-1.03	-0.71	+0.48	+0.38	+0.87	+1.12	+0.27

## LVIII

		11 <i>Lacertae</i>	6 <i>Lacertae</i>	2 $\delta$ <i>Lacertae</i>	13 <i>Lacertae.</i>	6 $\delta$ <i>Lacertae</i>	1 $\delta$ <i>Lacertae</i>	EXTRA STAR DM. 45°3813
(46)	4.	-0.30 rej.	-0.10	-0.10	+0.02	+0.45	+0.59	
(79)	1.	-0.99	-0.48	-0.53 rej.	+0.14	+0.79	+0.50	+1.65
(81)	4.	-0.74	-0.55			+0.63	+0.58	
(141)	1.			+0.12		+0.55	+0.65	
(142)	4.	-0.79	-0.59	0.00	+0.04	+0.67	+0.66	
(143)	4.	-0.81	-0.50	-0.09	-0.03	+0.70	+0.72	
Means		-0.83	-0.44	± 0.00	+0.04	+0.63	+0.62	+1.65

## LIX

	<i>7 Lacertae</i>	<i>5 Lacertae</i>	<i>2 Lacertae</i>	<i>4 Lacertae</i>	EXTRA STARS	<i>45δ Lacertae</i>	<i>46δ Lacertae</i>
(36) 3.	-0.45	-0.14	+0.26	+0.33		+0.34	+0.55
(45) 4.	-0.62	+0.04	+0.34	+0.25		+0.42	+0.88
(47) 4.	-0.50	-0.14	+0.30	+0.32		+0.57	+0.81
(81) 4.	-0.62	-0.06	+0.31	+0.38		+0.73	+0.99
(83) 3.	-0.80	+0.03	+0.35	+0.41		+0.89	+1.22
Means	-0.60	-0.05	+0.31	+0.34		+0.59	+0.89

## LX

	<i>o Androm.</i>	<i>15 Lacertae</i>	<i>2 Androm.</i>	<i>16 Lacertae</i>	<i>14 Lacertae</i>	<i>40δ Lacertae</i>
(40) 4.	-1.46	-0.25	-0.12	+0.37	+0.70	+0.73
(45) 4.	-1.47	-0.39	+0.22	+0.52	+0.59	+0.57
(79) 1.	-2.17	-0.08	+0.04	+0.40	+0.87	+0.91
(143) 4.	-1.82	-0.48	+0.01	+0.50	+0.93	+0.83
(144) 1.	-1.95	-0.36	+0.18	+0.79	+0.86	+0.81
(145) 4.	-1.93	-0.47	0.00	+0.65	+0.94	+0.79
Means	-1.80	-0.30	+0.06	+0.54	+0.82	+0.77

## LXI

	7 Androm.	8 Androm.	4 Androm.	11 Androm.	6 Androm.	3 Androm.
(40)	4. -0.47	-0.39	+0.19	+0.36	+0.92	
(47)	4. -0.48	-0.26	+0.05	+0.38	+0.80	-0.51
(76)	4. -0.69	-0.18	+0.40	+0.30	+0.85	-0.67
(84)	3. -0.56	-0.38	+0.25	+0.36		-0.55
(89)	2. -0.51	-0.57	+0.07	+0.49	+1.13	-0.61
(90)	2. -0.55	-0.30	+0.29	+0.19		-0.52
(158)	2. <u>-0.54</u>	<u>-0.42</u>	<u>+0.23</u>	<u>+0.47</u>		<u>-0.65</u>
Means	-0.54	-0.36	+0.21	+0.36	+0.92	-0.58

## EXTRA STARS

	5 Androm.	DM. 47°4114	DM. 44°4347?	DM. 44°4373	11§ Androm.	DM. 44°4378	DM. 47°4107
(47)	+0.50	+1.04					
(76)	+0.78						
(84)	+0.67	+1.62			+1.47		
(89)	<u>+0.42</u>		<u>+1.52</u>	<u>+1.36</u>	<u>+1.64</u>	<u>+1.64</u>	<u>+2.40</u>
Means	+0.59	<u>+1.33</u>	<u>+1.52</u>	<u>+1.36</u>	<u>+1.55</u>	<u>+1.64</u>	<u>+2.40</u>

## LXII

	$\kappa$ Androm.	$\iota$ Androm.	10 Androm.	13 Androm.	39 $\delta$ Androm.	23 Androm.	DM. 41°49'33.	9 Androm.	42 $\delta$ Androm.
(80)	2. 4 -1.79	4 -1.65	+0.06	+0.20	+0.57	+0.29	+0.83	+0.40	+1.09
(84)	3. -1.59	-1.46	+0.02	+0.36	+0.77	+0.27		+0.26	+0.75
(98)	2. 4 -1.65	4 -1.59	+0.37	+0.31	+0.61	+0.06	+0.69	+0.47	+0.75
(144)	1. 2 -1.63	2 -1.60							
(145)	4. 8 -1.63	8 -1.55	+0.28	+0.15	+0.49	+0.24	+0.50	+0.78	+0.78
Means			-1.66	-1.57	+0.18	+0.25	+0.61	+0.48	+0.84

## EXTRA STARS

DM. 41°49'25. 25 $\delta$  Androm.

(80)	+1.09	
(84)		+0.45
(98)		-0.12
Means	+1.09	+0.16

## LXIII

	$\lambda$ Androm.	$\psi$ Androm.	22 Androm.	18 Androm.	DM. 47°50	34 $\delta$ Androm.	DM. 48°41'12	DM. 45°26
(82)	2. -1.12	+0.20	+0.24	+0.67	+0.94			
(89)	2. -1.27	+0.19	+0.25	+0.84	+1.41	+1.48		
(90)	2. -1.65 rej.	+0.17	1 +0.34	+0.71				
(98)	2. 4 -1.11	+0.18	4 +0.42	+0.51		+1.58	+1.72	
(146)	4. -1.28	+0.12	+0.39	+0.76				+2.20
Means	-1.20	+0.17	+0.33	+0.70	+1.17	+1.53	+1.72	+2.20

## LXIV

	79 $\delta$ Androm.	$\nu$ Androm.	72 $\delta$ Androm.	88 $\delta$ Androm.	91 $\delta$ Androm.
(75)	4. -0.62	-0.91	-0.40	+0.90	+1.01
(80)	2. -0.41	-0.77	-0.28	+0.77	+0.71
(146)	4. 8 -0.45	8 -1.02	-0.15	+0.80	+0.82
(147)	4. -0.54	-1.03	-0.18	+0.64	+1.10
Means	-0.50	-0.93	-0.25	+0.78	+0.91

## LXV

		$\varphi$ Androm.	$\omega$ Cassiep.	$\xi$ Cassiep.	$\pi$ Cassiep.	51 $\delta$ Cassiep.	44 $\delta$ Cassiep.	EXTRA STARS
(82)	2.	-0.71	-0.39	-0.02	+0.20	+0.92	+0.50	$\mu$ Cassiep.
(91)	4.	-0.88	-0.18	-0.03	+0.14	+0.97		$\zeta$ Cassiep.
(97)	2.	-0.98	-0.14	0.00	+0.12	+0.98		
(147)	4.	8 -0.65	-0.19	-0.23	+0.04	+1.03		
Means		-0.80	-0.22	-0.07	+0.12	+0.97	+0.50	+0.10
							+0.10	-1.51
								-1.51

## LXVI

		41 Androm.	44 Androm.	39 Androm.	97 $\delta$ Androm.	112 $\delta$ Androm.	EXTRA STARS
(75)	4.	-0.70	-0.02	+0.22	+0.29	+0.21	DM. 42°293
(91)	4.	-0.85	-0.16	+0.46	+0.12	+0.43	2 +1.15
(100)	1.	-0.92 rej.	-0.23 rej.	+0.67 rej.	+0.14 rej.		DM. 42°283
(148)	4.	-0.78	-0.06	+0.47	0.00	+0.36	
(149)	4.	-0.74	-0.06	+0.30	+0.15	+0.34	
Means		-0.80	-0.11	+0.42	+0.14	+0.27	+1.15
							+0.99

## LXVII

		$\nu$ Persei	$\varphi$ Persei	$\xi$ Androm.	$\omega$ Androm.	A Androm.
(97)	2.	-1.31	-0.32	+0.27	+0.64	+0.70
(103)	3.	-1.02	0.00	+0.08	+0.24	+0.71
(148)	4.	8 -1.27	12 -0.47	+0.29	+0.44	+0.99
(152)	4.	12 -1.29	12 -0.37	8 +0.25	8 +0.48	+0.91
Means		-1.22	-0.29	+0.22	+0.45	+0.83

## LXVIII

	$\gamma$ Androm.	$\nu$ Androm.	122 $\delta$ Androm.	$\chi$ Androm.	55 Androm.	120 $\delta$ Androm.
(149) 4.	8 -3.13	12 -0.59	8 +0.54	8 +0.60	+1.11	+1.46
(150) 4.	-3.06	-0.42	+0.59	+0.53	+1.00	+1.33
(152) 4.	-3.11	-0.58	+0.53	+0.53	+1.11	+1.53
(153) 4.	<u>-2.98</u>	<u>-0.47</u>	<u>+0.55</u>	<u>+0.56</u>	<u>+0.96</u>	<u>+1.37</u>
Means	<u>-3.07</u>	<u>-0.51</u>	<u>+0.55</u>	<u>+0.55</u>	<u>+1.04</u>	<u>+1.42</u>

## LXIX

	65 Androm.	64 Androm.	6 Persei	2 Persei	63 Androm.	35 Persei	3 Persei
(103) 3.	-0.79	-0.22	-0.22	+0.11	+0.10 rej.	+0.22	+0.29
(106) 4.	-0.97	-0.30	-0.23	+0.36	+0.12	+0.67	+0.32
(150) 4.	-1.18	-0.56	-0.10	+0.44	+0.22	+0.84	+0.31
(154) 4.	<u>-0.97</u>	<u>-0.49</u>	<u>-0.17</u>	<u>+0.38</u>	<u>+0.16</u>	<u>+0.76</u>	<u>+0.31</u>
Means	<u>-0.97</u>	<u>-0.39</u>	<u>-0.18</u>	<u>+0.32</u>	<u>+0.15</u>	<u>+0.76</u>	<u>+0.31</u>

## LXX

	b Androm.	12 Persei	c Androm.	14 Persei	137 $\delta$ Androm.
(153) 4.	-0.58	-0.43	+0.11	+0.29	+0.60
(154) 4.	-0.65	-0.50	+0.18	+0.29	+0.69
(156) 2.	-0.76	-0.34	+0.07	+0.33	
(159) 4.	<u>-0.63</u>	<u>-0.49</u>	<u>+0.19</u>	<u>+0.16</u>	<u>+0.77</u>
Means	<u>-0.65</u>	<u>-0.44</u>	<u>+0.14</u>	<u>+0.27</u>	<u>+0.69</u>

Catalogue of the Stars observed by the author, with the magnitudes of various authorities  
 The places are given for 1855

No.	Name	RA.	Dec.	W.	A.	S.	DM.	h.	G.	Z.	P.
1	22 Andromedae	0 <sup>h</sup> 2 <sup>m</sup> 8 <sup>s</sup>	45° 16'	4.6	5.2	5.2	5.1	—	—	—	5.20
2	DM. 45° 26 . . .	5.2	45 18	—	—	—	6.9	—	—	—	6.81:
3	23 Andromedae	6.0	40 14	5.7	5.8	5.8	5.5	—	—	—	5.98
4	DM. 47° 50 . . .	9.5	47 8	—	—	—	5.8	—	—	—	5.93:
5	72δ Andromedae	20.5	43 37	—	5.8	5.4	5.4	—	—	—	5.44
6	79δ Andromedae	28.9	43 41	—	5.8	5.8	5.3	—	—	—	5.23
7	ζ Cassiepeiae . .	28.9	53 6	4.4	4.1	4.1	4.2	—	3.63	3.78	3.80:
8	44δ Cassiepeiae .	31.2	48 33	—	—	5.8	5.8	—	—	—	5.54:
9	88δ Andromedae	33.2	44 3	—	5.8	5.4	5.4	—	—	—	6.32
10	ξ Cassiepeiae . .	34.0	49 43	4.9	5.8	4.6	5.0	—	—	—	5.04
11	π Cassiepeiae . .	35.5	46 15	4.7	5.8	4.9	4.4	—	—	—	5.20
12	51δ Cassiepeiae .	36.4	47 4	—	5.8	5.8	5.6	—	—	—	5.94
13	ο Cassiepeiae . .	36.7	47 29	4.5	4.9	4.9	4.7	—	—	—	4.91
14	ν Andromedae . .	41.9	40 18	4.8	4.4	4.4	4.6	—	—	—	4.86
15	91δ Andromedae	42.2	44 12	—	5.8	6.3	6.7	—	—	—	6.44
16	97δ Andromedae	51.9	43 55	—	5.8	6.3	6.1	—	—	—	6.00
17	39 Andromedae	54.8	40 33	5.8	5.8	6.3	5.8	—	—	—	6.24
18	μ Cassiepeiae . .	58.7	54 12	5.9	5.8	5.2	5.5	—	—	—	5.19:
19	41 Andromedae	59.7	43 10	5.6	4.9	4.9	5.1	—	—	—	5.19
20	φ Andromedae . .	1	46 28	4.7	4.4	4.4	4.3	—	—	4.39	4.42
21	44 Andromedae	2.1	41 18	5.6	5.8	5.8	6.1	—	—	—	5.79
22	DM. 42° 288 . . .	13.8	42 50	—	—	—	6.4	—	—	—	6.73:
23	ξ Andromedae . .	13.8	44 45	5.6	4.9	4.9	5.1	—	—	4.94	4.83
24	DM. 42° 293 . . .	15.9	42 23	—	—	—	6.4	—	—	—	6.87:
25	112δ Andromedae	17.8	42 41	—	5.8	5.8	5.8	—	—	—	6.11
26	ω Andromedae . .	19.0	44 39	5.3	4.9	4.9	4.9	—	—	4.93	5.03
27	A Andromedae . .	21.4	46 16	5.8	5.8	5.8	5.6	—	—	—	5.36

No.	Name	RA.	Dec.	$\text{M.}$	A.	$\text{S.}$	DM.	h.	$\text{G.}$	Z.	P.
28	$\nu$ Andromedae .	1 <sup>h</sup> 28 <sup>m</sup> 4	40° 41'	4.1	4.4	4.1	4.0	—	—	—	4.15
29	$\nu$ Persei . . . . .	29.1	47 53	3.3	3.8	3.8	3.8	—	4.00	4.01	3.59
30	$\chi$ Andromedae .	30.7	43 37	5.3	5.2	5.4	4.9	—	—	—	5.06
31	120 $\delta$ Andromedae	32.0	42 34	—	5.8	5.8	5.8	—	—	—	5.81
32	122 $\delta$ Andromedae	33.0	41 53	—	5.8	5.4	5.2	—	—	—	5.06
33	$\varphi$ Persei . . . . .	34.6	49 57	4.6	4.1	4.1	4.2	—	—	3.56	4.39
34	3 $\delta$ Persei . . . . .	40.3	47 11	—	5.8	5.8	6.1	—	—	—	6.22
35	2 Persei . . . . .	43.0	50 5	6.1	5.8	5.8	6.0	—	—	—	5.84
36	55 Andromedae	44.6	40 2	5.3	5.8	5.8	5.8	—	—	—	5.48
37	3 Persei . . . . .	49.4	48 30	5.9	5.8	5.8	6.1	—	—	—	5.83
38	$\gamma$ Andromedae .	55.0	41 38	2.3	2.6	2.6	2.0	2.17	2.31	—	1.94
39	6 Persei . . . . .	2 4.0	50 23	5.7	5.8	5.8	6.0	—	—	—	5.41
40	$b$ Andromedae .	4.2	43 33	5.3	5.2	5.2	5.1	—	—	—	4.77
41	$c$ Andromedae .	9.9	46 42	5.7	5.2	5.2	5.1	—	—	—	5.46
42	63 Andromedae	11.4	49 29	5.9	5.8	5.8	6.1	—	—	—	5.70
43	137 $\delta$ Andromedae	13.9	40 44	—	5.8	5.8	6.1	—	—	—	5.94
44	64 Andromedae	14.8	49 21	5.9	5.8	5.8	5.6	—	—	—	5.23
45	65 Andromedae	16.0	49 37	5.3	4.9	5.4	4.7	—	—	—	4.73
46	12 Persei . . . . .	33.1	39 34	5.2	4.9	5.2	4.7	—	—	—	4.96
47	$\theta$ Persei . . . . .	34.3	48 37	4.6	4.1	4.1	4.2	—	—	—	4.13
48	14 Persei . . . . .	34.7	43 41	5.2	5.8	5.8	5.4	—	—	—	5.57
49	$\tau$ Persei . . . . .	44.0	52 9	4.5	4.1	4.1	4.2	—	—	—	3.97
50	47 $\delta$ Persei . . . . .	50.6	51 47	—	4.9	5.4	5.5	—	—	—	5.16
51	$\gamma$ Persei . . . . .	54.3	52 56	—	3.2	3.1	3.4	3.14	3.09	3.00	2.92
52	$\iota$ Persei . . . . .	58.6	49 3	—	4.1	4.1	4.3	—	—	—	4.28
53	$\beta$ Persei . . . . .	58.7	40 24	—	Var.	Var.	Var.	2.32	2.08	—	2.30
54	$\kappa$ Persei . . . . .	59.7	44 18	3.9	4.4	4.4	4.4	—	—	—	3.79
55	$\omega$ Persei . . . . .	3 2.0	39 4	5.1	4.9	4.9	5.1	—	—	—	4.64
56	63 $\delta$ Persei . . . . .	5.9	50 23	—	4.9	4.9	5.8	—	—	—	5.13

No.	Name	RA.	Dec.	III.	A.	§.	DM.	h.	g.	Z.	P.
57	30 Persei	3 <sup>h</sup> 8 <sup>m</sup> 1	43°29'	5.9	5.8	5.8	5.8	—	—	—	5.59
58	29 Persei	8.3	49 41	5.6}	—	—	5.5	—	—	—	5.30
59	31 Persei	8.8	49 35	5.6}	4.9	4.9	5.5	—	—	—	5.29
60	<i>l</i> Persei	11.8	42 48	—	—	6.3	6.1	—	—	—	5.05
61	73δ Persei	13.0	48 41	—	—	6.3	6.1	—	—	—	5.42
62	$\alpha$ Persei	14.0	49 21	2.3	2.1	2.1	2.0	1.68	1.88	1.77	1.90
63	76δ Persei	17.8	48 33	—	5.8	5.8	5.8	—	—	—	5.22
64	77δ Persei	18.5	49 21	—	—	6.3	6.4	—	—	—	5.93:
65	34 Persei	19.0	49 1	5.0	4.9	5.2	5.2	—	—	—	4.95
66	$\sigma$ Persei	20.4	47 28	4.6	4.9	4.6	4.7	—	—	—	4.44
67	DM. 47°844	20.4	47 36	—	—	—	6.9	—	—	—	6.11
68	DM. 47°847	21.9	47 31	—	—	—	6.4	—	—	—	5.68
69	36 Persei	22.4	45 34	5.6	5.8	5.8	5.8	—	—	—	5.33
70	84δ Persei	22.7	44 21	—	5.8	5.8	6.4	—	—	—	6.50
71	$\psi$ Persei	26.2	47 43	4.2	4.9	4.9	5.2	—	—	—	4.41
72	$\delta$ Persei	32.6	47 20	3.8	3.2	3.5	3.7	3.16	2.99	3.06	3.27
73	92δ Persei	34.6	45 39	—	5.8	6.3	6.4	—	—	—	6.15
74	$\nu$ Persei	35.4	42 8	3.7	4.1	4.1	4.2	—	—	4.39	3.84
75	96δ Persei	35.5	45 14	—	5.8	5.8	6.1	—	—	—	5.76
76	97δ Persei	39.2	43 30	—	—	5.8	6.1	—	—	—	6.03
77	99δ Persei	40.0	44 30	—	5.8	5.8	5.6	—	—	—	5.81
78	102δ Persei	43.2	48 13	—	—	6.3	5.8	—	—	—	5.76:
79	104δ Persei	45.5	47 27	—	5.3	5.4	5.8	—	—	—	5.55
80	$\Delta$ Persei	45.9	50 16	5.4	5.2	5.2	5.5	—	—	—	5.40
81	$\epsilon$ Persei	48.2	39 35	3.1	3.5	3.5	3.4	3.05	2.86	2.98	3.06
82	$\lambda$ Persei	55.8	49 57	4.7	4.4	4.4	4.2	—	—	—	4.52
83	$c$ Persei	58.1	47 20	4.5	4.1	4.1	4.4	—	—	—	4.27
84	$\mu$ Persei	4 4.3	48 2	4.7	4.4	3.8	4.2	—	4.29	—	4.35
85	$f$ Persei	5.0	40 6	5.0	4.9	4.9	4.7	—	—	—	4.71

No.	Name	RA.	Dec.	W.	A.	δ.	DM.	h.	ε.	Z.	P.
86	<i>b</i> Persei . .	4 <sup>h</sup> 7 <sup>m</sup> 4	49°56'	—	4.9	4.9	4.7	—	—	—	4.82
87	120δ Persei	9.2	50 34	—	5.8	6.3	6.0	—	—	—	5.61
88	121δ Persei	10.1	49 54	—	—	5.8	7.7	—	—	—	5.76:
89	<i>d</i> Persei . .	11.1	46 9	5.0	4.9	4.9	5.0	—	—	—	5.06
90	<i>m</i> Persei . .	23.2	42 45	6.9	5.8	5.8	6.4	—	—	—	6.24
91	<i>e</i> Persei . .	26.7	40 58	4.5	4.9	4.9	4.9	—	—	4.38	4.21
92	133δ Persei	30.6	48 1	—	5.8	5.8	5.8	—	—	—	5.68
93	135δ Persei	32.4	49 42	—	5.8	5.8	5.4	—	—	—	5.92
94	59 Persei . .	32.6	43 5	6.9	5.8	5.8	5.5	—	—	—	5.42
95	4δ Aurigae	40.3	48 29	—	5.8	5.8	5.6	—	—	—	5.67
96	6δ Aurigae	42.6	42 20	—	5.8	5.8	6.1	—	—	—	5.80
97	8δ Aurigae	44.5	43 49	—	5.8	5.8	6.1	—	—	—	6.18
98	ε Aurigae .	51.6	43 36	3.1	3.5	Var.	Var.	—	3.21	2.96	3.69:
99	ζ Aurigae .	52.4	40 52	4.1	4.1	4.1	3.8	—	—	3.81	3.58
100	η Aurigae .	56.4	41 2	3.6	3.8	3.8	3.5	—	—	3.40	3.21
101	22δ Aurigae	59.9	46 47	—	—	5.8	5.8	—	—	—	5.54
102	α Aurigae .	5 6.0	45 51	0.1	0.3	0.2	0.0	0.2:	0.33	-20	0.09:
103	λ Aurigae .	8.9	39 59	4.8	4.9	4.9	4.7	—	—	—	4.72
104	34δ Aurigae	10.1	40 56	—	5.8	5.8	5.6	—	—	—	5.59
105	ρ Aurigae .	11.6	41 39	5.6	5.3	5.8	5.8	—	—	—	5.38
106	37δ Aurigae	12.6	40 53	—	5.8	5.8	5.8	—	—	—	5.68
107	ο Aurigae .	34.7	49 46	5.2	5.4	5.8	5.8	—	—	—	5.56
108	β Aurigae .	48.9	44 56	1.6	2.1	2.1	2.0	2.15	1.84	1.89	1.96
109	π Aurigae .	49.2	45 55	5.0	4.9	4.9	4.7	—	—	—	4.36
110	36 Aurigae	50.0	47 52	5.8	5.8	5.8	5.8	—	—	—	5.69
111	85δ Aurigae	51.6	49 52	—	5.8	6.3	5.8	—	—	—	6.02
112	DM. 43°1421	52.4	43 22	—	—	—	7.2	—	—	—	6.24:
113	38 Aurigae	52.9	42 55	6.2	5.8	5.8	6.2	—	—	—	6.07
114	39 Aurigae	54.6	42 59	6.2	—	6.3	6.7	—	—	—	5.90

No.	Name	RA.	Dec.	W.	A.	S.	DM.	h.	G.	Z.	P.
115	41 Aurigae	6 <sup>h</sup> 0 <sup>m</sup> .5	48°45'	5.8	5.8	5.8	5.8	—	—	—	5.88
116	42 Aurigae	6.8	46 27	6.5 }	5.8	5.8	{ 6.4	—	—	—	6.37
117	43 Aurigae	7.5	46 23	6.5 }	5.8	5.8	{ 6.4	—	—	—	6.17
118	ψ <sup>1</sup> Aurigae	13.7	49 21	5.3	4.9	5.2	5.8	—	—	—	4.74
119	ψ <sup>3</sup> Aurigae	28.7	40 1	5.9	5.8	5.8	6.0	—	—	—	5.42
120	ψ <sup>2</sup> Aurigae	29.0	42 37	5.3	4.9	5.2	4.7	—	—	—	4.85
121	ψ <sup>4</sup> Aurigae	32.5	44 39	4.8	4.9	5.2	5.4	—	—	—	4.90
122	ψ <sup>5</sup> Aurigae	36.3	43 43	5.7	5.8	5.4	5.5	—	—	—	5.16
123	ψ <sup>6</sup> Aurigae	36.6	48 55	5.7	5.8	5.8	5.4	—	—	—	5.20
124	ψ <sup>7</sup> Aurigae	40.5	41 57	5.3	4.9	4.9	5.1	—	—	—	4.96
125	130δ Aurigae	44.6	46 0	—	—	6.3	6.7	—	—	—	6.02
126	ψ <sup>9</sup> Aurigae	45.8	46 28	—	5.8	5.8	6.1	—	—	—	5.80
127	ψ <sup>10</sup> Aurigae	47.0	45 19	—	4.9	5.2	5.8	—	—	—	4.98
128	16δ Lyncis	7	5.1	47 31	—	5.8	5.8	5.5	—	—	5.51
129	18δ Lyncis	7.5	49 42	—	5.8	6.3	5.2	—	—	—	5.01
130	64 Aurigae	8.0	41 7	5.7	5.8	5.8	5.8	—	—	—	5.88
131	66 Aurigae	14.1	40 57	5.6	5.8	5.8	5.6	—	—	—	5.26
132	21 Lyncis	15.8	49 30	6.1	4.9	4.9	4.4	—	—	—	4.65
133	23δ Lyncis	18.1	48 29	—	5.8	6.3	5.8	—	—	—	5.73
134	22 Lyncis	19.0	49 57	5.7	5.8	5.8	5.8	—	—	—	5.28:
135	26δ Lyncis	26.0	46 30	—	5.8	5.8	5.8	—	—	—	5.53
136	27δ Lyncis	27.1	49 5	—	—	6.3	6.2	—	—	—	5.79:
137	32δ Lyncis	30.5	48 28	—	5.8	6.3	5.6	—	—	—	5.67
138	25 Lyncis	43.9	47 45	6.0	—	—	6.0	—	—	—	6.11
139	26 Lyncis	44.2	47 56	5.2	5.8	5.8	5.4	—	—	—	5.46
140	31 Lyncis	8	12.9	43 39	4.5	4.9	4.9	4.9	—	—	4.26
141	34 Lyncis	31.1	46 21	5.4	5.8	5.8	5.5	—	—	—	5.33
142	35 Lyncis	42.2	44 17	5.2	5.8	5.8	5.4	—	—	—	5.71
143	75δ Lyncis	45.0	44 8	—	—	6.3	7.7	—	—	—	5.76:

No.	Name	RA.	Dec.	■	A.	§.	DM.	h.	©.	Z.	P.
144	76 $\delta$ Lyncis . . .	8 <sup>h</sup> 47 <sup>m</sup> 0	46°12'	—	5.8	5.8	6.4	—	—	—	5.64
145	$\iota$ Ursae majoris .	49.3	48 36	—	3.2	3.1	3.3	—	3.13	3.10	3.15
146	10 Ursae majoris	51.2	42 21	4.6	4.1	4.1	4.2	—	—	3.94	4.02
147	$\kappa$ Ursae majoris	53.7	47 44	3.7	3.5	3.5	3.7	—	3.44	3.66	3.56
148	25 $\delta$ Ursae majoris	55.4	49 6	—	—	5.8	6.4	—	—	—	5.43
149	28 $\delta$ Ursae majoris	57.3	39 2	—	4.9	4.6	4.6	—	—	—	4.64
150	36 Lyncis . . .	9 4.3	43 49	5.2	4.9	4.9	4.9	—	—	—	5.45
151	38 $\delta$ Ursae majoris	7.8	47 26	—	5.8	6.3	6.4	—	—	—	5.94
152	38 Lyncis . . .	9.8	37 26	3.9	4.1	4.1	4.2	—	3.87	3.89	3.83
153	46 $\delta$ Ursae majoris	19.2	46 14	5.4	5.8	5.4	5.4	—	—	—	5.39
154	DM. 45°1728 . .	21.5	45 19	—	—	—	7.4	—	—	—	6.80:
155	$\theta$ Ursae majoris .	23.1	52 21	3.2	3.2	3.1	3.1	—	3.24	3.22	3.28
156	26 Ursae majoris	24.9	52 42	—	4.9	4.9	4.9	—	—	—	4.77
157	85 $\delta$ Lyncis . . .	26.0	40 16	—	4.9	5.2	4.6	—	—	—	4.82
158	42 Lyncis . . .	29.3	40 54	5.2	5.8	5.8	5.4	—	—	—	5.34
159	43 Lyncis . . .	33.0	40 25	5.7	5.8	5.8	5.6	—	—	—	5.57
160	65 $\delta$ Ursae majoris	39.2	46 42	5.5	4.9	5.4	5.7	—	—	—	5.00
161	31 Ursae majoris	46.2	50 30	—	4.9	5.4	5.4	—	—	—	5.37
162	19 Leonis minoris	48.8	41 45	—	4.9	5.2	5.1	—	—	—	5.23
163	$\lambda$ Ursae majoris	10 8.3	43 40	—	3.5	3.5	3.7	—	3.22	3.51	3.66
164	DM. 43°2007 . .	9.8	43 47	—	—	—	6.9	—	—	—	6.64:
165	84 $\delta$ Ursae majoris	10.4	49 7	—	—	6.3	5.8	—	—	—	6.11:
166	25 Leonis minoris	12.4	42 34	—	—	—	6.6	—	—	—	6.77:
167	DM. 41°2076 . .	13.6	41 57	—	—	—	6.4	—	—	—	5.85
168	$\mu$ Ursae majoris	13.7	42 14	—	3.2	3.1	3.3	3.1:	3.33	2.91	3.10
169 <sup>a</sup>	89 $\delta$ Ursae majoris	18.9	42 20	—	—	6.3	6.9	—	—	—	5.76:
170	DM. 49°1960 . .	19.1	49 42	—	—	—	6.5	—	—	—	6.88

9. 169. Some uncertainty about the identification.

No.	Name	RA.	Dec.	■	A.	§	DM.	h.	§	Z.	P.
171	90 $\delta$ Ursae majoris	10 <sup>h</sup> 19 <sup>m</sup> 1	49°33'	—	5.8	5.8	6.1	—	—	—	6.54
172	DM. 41°2093 . . .	19.2	41 57	—	—	—	7.4	—	—	—	5.73:
173	31 Leonis minoris	19.5	37 26	—	4.4	4.4	4.0	—	—	—	4.34
174	DM. 41°2097 . . .	21.6	41 13	—	—	—	7.4	—	—	—	6.60:
175	32 Leonis minoris	21.7	39 40	—	5.8	5.8	6.1	—	—	—	5.94
176	95 $\delta$ Ursae majoris	24.8	41 9	—	4.9	4.9	5.1	—	—	—	4.89
177	35 Leonis minoris	28.0	37 5	—	5.8	5.8	6.1	—	—	7.04	5.68:
178	38 Leonis minoris	30.8	38 39	—	5.8	5.4	5.5	—	—	6.39	5.98
179	105 $\delta$ Ursae majoris	35.0	46 58	—	4.9	4.9	5.1	—	—	—	5.36
180	DM. 46°1659 . . .	36.8	46 19	—	—	—	6.9	—	—	—	7.52:
181	$\omega$ Ursae majoris .	45.6	43 58	—	4.9	4.9	5.1	—	—	—	4.89:
182	DM. 42°2162 . . .	48.0	42 47	—	—	—	6.1	—	—	—	6.02:
183	47 Ursae majoris .	51.3	41 12	—	4.9	4.9	4.7	—	—	—	5.10
184	121 $\delta$ Ursae majoris	51.9	46 17	—	5.8	5.8	5.8	—	—	—	5.58
185	122 $\delta$ Ursae majoris	52.1	43 41	—	5.8	5.8	6.1	—	—	—	6.08
186	DM. 43°2069 . . .	52.5	43 30	—	—	—	6.9	—	—	—	6.63:
187	49 Ursae majoris .	52.7	39 59	—	4.9	4.9	4.7	—	—	—	5.14
188	51 Ursae majoris .	56.5	39 1	—	5.8	5.4	6.1	—	—	—	6.10:
189	130 $\delta$ Ursae majoris	11	1.5	43 59	—	—	6.3	6.4	—	—	5.89:
190	$\psi$ Ursae majoris .	1.5	45 18	—	3.2	3.5	3.7	3.16	3.27	2.97	3.02
191	136 $\delta$ Ursae majoris	8.5	50 16	—	5.8	5.8	5.8	—	—	—	5.56:
192	56 Ursae majoris .	14.9	44 17	—	5.8	5.8	5.2	—	—	—	5.03
193	DM. 40°2432 . . .	20.8	40 6	—	—	—	8.0	—	—	—	7.19:
194	57 Ursae majoris .	21.3	40 8	—	4.9	4.9	4.9	—	—	—	5.40
195	58 Ursae majoris .	22.7	43 59	—	5.8	5.8	6.1	—	—	—	5.98
196	158 $\delta$ Ursae majoris	30.0	51 26	—	5.8	6.3	5.8	—	—	—	6.35:
197	59 Ursae majoris .	30.6	44 25	—	5.8	5.8	5.8	—	—	—	5.56
198	60 Ursae majoris .	30.7	47 38	—	—	5.8	6.0	—	—	—	6.32:
199	$\chi$ Ursae majoris .	38.4	48 34	—	4.1	3.8	4.0	—	—	3.71	3.74

No.	Name	RA.	Dec.	W.	A.	S.	DM.	h.	S.	Z.	P.
200	$\gamma$ Ursae majoris .	11 <sup>h</sup> 46 <sup>m</sup> 2	54°30'	—	2.6	2.6	2.7	2.42	2.34	2.45	2.59
201	65 Ursae majoris .	47.5	47 18	—}	—	5.8	{ 7.2	—	—	—	6.93:
202 <sup>10</sup>	DM. 47°19'14 . . .	47.6	47 17	—}	—	—	{ 7.4	—	—	—	—
203	DM. 41°22'51 . . .	49.4	41 10	—	—	—	8.4	—	—	—	6.58:
204 <sup>11</sup>	DM. 41°22'52 . . .	49.6	41 5	—	—	—	6.9	—	—	—	6.97:
205	179 <sup>b</sup> Ursae majoris	49.8	41 10	—	5.8	5.8	6.4	—	—	—	6.56:
206 <sup>12</sup>	DM. 40°24'85 . . .	51.1	41 0	—	—	—	7.8	—	—	—	6.89:
207	67 Ursae majoris .	54.8	43 51	—	4.9	5.2	5.0	—	—	—	5.18
208	DM. 43°21'82 . . .	55.1	43 55	—	—	—	6.6	—	—	—	6.60:
209	DM. 49°21'10 . . .	58.8	49 58	—	—	—	6.9	—	—	—	6.82:
210	2 Canum venat. .	12 8.9	41 28	—	5.8	5.2	6.4	—	—	—	5.55
211	3 Canum venat. .	12.7	49 47	—	5.8	5.2	5.5	—	—	—	5.60
212	DM. 49°21'32 . . .	15.5	49 9	—	—	—	7.2	—	—	—	6.87:
213	4 Canum venat. .	16.7	43 21	—	5.8	5.8	6.1	—	—	—	5.96
214	DM. 43°22'21 . . .	17.9	43 40	—	—	—	6.9	—	—	—	6.54:
215	6 Canum venat. .	18.7	39 50	—	5.2	5.2	5.3	—	—	5.28	5.02
216	11 <sup>b</sup> Canum venat.	20.4	42 10	—	5.8	5.8	6.4	—	—	—	6.38
217	8 Canum venat. .	26.9	42 8	—	4.4	4.4	4.4	—	—	—	4.54
218	9 Canum venat. .	31.8	41 40	—	5.8	5.8	6.9	—	—	—	6.23:
219	21 <sup>b</sup> Canum venat.	35.4	46 40	—	—	6.3	6.7	—	—	—	6.80:
220	23 <sup>b</sup> Canum venat.	37.6	44 54	—	5.8	5.8	6.0	—	—	—	6.22
221	10 Canum venat.	38.1	40 4	—	5.8	5.4	6.0	—	—	—	5.93
222	26 <sup>b</sup> Canum venat.	38.3	46 13	—	5.2	5.2	5.4	—	—	—	5.13
223	11 Canum venat.	42.0	49 16	—	5.8	5.8	5.8	—	—	—	6.25
224	29 <sup>b</sup> Canum venat.	43.0	49 33	—	—	6.3	6.4	—	—	—	6.42

10. 202. Not observed separately: mag. of 201 + 202 = 6.39:

11. 204. Perhaps 179<sup>b</sup> includes both this star and 205.

12. 206. Some uncertainty about the identification.

No.	Name	RA.	Dec.	W.	A.	S.	DM.	h.	G.	Z.	P.
225	30 $\delta$ Canum venat.	12 <sup>h</sup> 43 <sup>m</sup> .3	38°19'	—	5.8	5.2	5.8	—	—	—	5.93
226	32 $\delta$ Canum venat.	48.3	47 59	—	5.8	5.8	5.6	—	—	—	5.66
227	12 Canum venat.	49.2	39 6	—	3.2	2.8	3.1	3.01	2.69	2.97	3.22
228	36 $\delta$ Canum venat.	50.5	46 57	—	5.8	5.8	6.4	—	—	—	6.02
229 <sup>13</sup>	40 $\delta$ Canum venat.	57.3	43 47	—	—	5.8	5.8	—	—	—	6.82:
230	42 $\delta$ Canum venat.	59.4	46 3	—	5.8	5.8	5.8	—	—	—	5.58
231	15 Canum venat.	13 3.0	39 19	{—}	4.9	4.9	{6.1}	—	—	—	6.31
232	17 Canum venat.	3.4	39 16	{—}	—	—	{6.1}	—	—	—	6.08
233	49 $\delta$ Canum venat.	7.1	40 56	—	4.9	4.9	4.9	—	—	5.21	5.08
234	19 Canum venat.	9.1	41 37	—	5.8	5.2	5.8	—	—	—	5.98
235	20 Canum venat.	11.1	41 20	—	4.6	4.6	4.3	—	—	4.94	4.96
236	21 Canum venat.	12.1	50 27	—	4.9	4.9	4.9	—	—	—	5.33
237	23 Canum venat.	13.8	40 56	—	5.3	5.4	5.4	—	—	—	5.74
238	56 $\delta$ Canum venat.	14.5	44 45	—	—	6.3	6.4	—	—	—	6.54:
239	57 $\delta$ Canum venat.	15.7	44 40	—	—	5.8	6.4	—	—	—	6.36
240	58 $\delta$ Canum venat.	20.1	46 47	—	5.8	5.8	5.8	—	—	—	5.95
241	59 $\delta$ Canum venat.	22.1	41 29	—	5.8	5.8	6.1	—	—	—	6.58
242	DM. 42°2403 . . .	22.8	42 59	—	—	—	7.1	—	—	—	7.78:
243	214 $\delta$ Ursae majoris	22.8	51 29	—	5.8	5.8	6.6	—	—	—	6.39:
244	60 $\delta$ Canum venat.	25.0	42 51	—	5.8	5.8	6.1	—	—	—	6.13
245	62 $\delta$ Canum venat.	28.0	39 33	—	5.8	6.3	6.1	—	—	—	6.60
246	24 Canum venat.	28.5	49 46	—	4.9	4.9	4.7	—	—	—	4.86
247	65 $\delta$ Canum venat.	29.1	44 56	—	5.8	5.8	6.4	—	—	—	6.88
248	66 $\delta$ Canum venat.	30.7	50 14	—	—	6.3	6.4	—	—	—	6.37:
249	72 $\delta$ Canum venat.	36.3	42 24	—	5.8	6.3	5.8	—	—	—	6.37
250	73 $\delta$ Canum venat.	37.4	46 15	—	5.8	6.3	6.4	—	—	—	6.74
251	DM. 41°2423 . . .	39.9	41 46	—	—	—	7.7	—	—	—	7.96:

13. 229. Some uncertainty about the identification.

No.	Name	RA.	Dec.	W.	A.	δ.	DM.	h.	G.	Z.	P.
252	DM. 39°26'78 . . .	13 <sup>h</sup> 40 <sup>m</sup> 0	39°14'	—	—	—	5.8	—	—	—	6.11
253	75δ Canum venat.	40.1	41 49	—	5.3	5.4	5.5	—	—	—	6.24
254	76δ Canum venat.	40.8	39 18	—	5.2	5.2	5.4	—	—	—	5.76
255	η Ursae majoris	41.8	50 3	—	2.1	2.1	2.0	1.82	1.69	1.89	2.01
256	78δ Canum venat.	42.0	42 46	—	5.8	6.3	6.4	—	—	—	6.71
257	87δ Canum venat.	56.4	46 27	—	5.8	5.8	5.8	—	—	—	6.60:
258	22δ Bootae . . . .	57.6	51 41	—	—	6.3	5.8	—	—	—	6.41:
259	23δ Bootae . . . .	14	2.1	44 32	—	5.3	5.4	5.4	—	—	5.41
260	13 Bootae . . . .		2.9	50 9	5.2	5.8	5.8	5.4	—	—	5.41
261	DM. 43°23'91 . . .		5.8	43 2	—	—	—	6.8	—	—	6.36
262	κ Bootae . . . .		8.3	52 27	4.3	4.4	4.4	4.4	—	—	4.57
263	32δ Bootae . . . .		8.5	42 13	—	5.8	5.8	6.0	—	—	6.43
264	34δ Bootae . . . .		10.5	40 25	—	5.8	5.8	6.1	—	—	6.44
265	λ Bootae . . . .		10.9	46 46	4.1	4.1	4.1	4.0	4.20	—	4.41
266	ι Bootae . . . .		11.1	52 2	4.3	4.4	4.4	4.4	—	—	4.88
267	DM. 51°19'08 . . .		12.2	51 59	—	—	—	6.7	—	—	6.14:
268	42δ Bootae . . . .		13.9	39 29	—	5.8	5.8	5.8	—	—	6.18:
269	θ Bootae . . . .		20.3	52 31	4.1	3.8	4.1	4.0	—	—	4.15
270	g Bootae . . . .		23.6	50 30	5.7	5.8	5.8	5.6	—	—	5.91:
271	γ Bootae . . . .		26.2	38 57	3.0	2.8	3.5	2.8	3.24	3.18	3.23
272	69δ Bootae . . . .		29.6	50 1	—	5.8	5.8	5.5	—	—	6.15
273	33 Bootae . . . .		33.5	45 1	5.1	5.8	5.2	5.2	—	—	5.64
274	h Bootae . . . .		44.2	46 43	5.7	5.8	5.4	5.8	—	—	6.10
275	39 Bootae . . . .		44.8	49 20	5.1	5.8	5.8	5.6	—	—	5.94
276	112δ Bootae . . .		55.7	47 51	—	—	5.8	6.1	—	—	6.71:
277	β Bootae . . . .		56.5	40 58	3.5	3.2	3.1	3.1	3.7:	3.60	3.68
278	i Bootae . . . .		59.0	48 13	4.8	4.9	4.6	4.4	—	—	5.03
279	k Bootae . . . .	15	0.6	48 43	5.1	4.9	5.2	5.4	—	—	6.02
280	134δ Bootae . . .		17.3	40 6	—	5.8	5.8	5.4	—	—	5.65

No.	Name	RA.	Dec.	W.	A.	S.	DM.	h.	G.	Z.	P.
281	$\mu$ Bootae . .	15 <sup>h</sup> 19 <sup>m</sup> 0	37°53'	4.3	3.8	4.1	4.0	—	4.21	4.45	4.61
282	DM. 37°2637	19.0	37 51	—	—	—	6.9	—	—	—	6.56:
283	$\nu$ Bootae (pr.)	25.7	41 20	4.7}	4.1	4.1	{ 4.4	—	—	—	5.06
284	$\nu$ Bootae (sq.)	26.6	41 25	4.5}	4.1	—	{ 4.7	—	—	—	5.20
285	$\varphi$ Bootae . .	32.6	40 50	4.9	4.9	4.9	4.9	—	—	—	5.33
286	1 $\delta$ Herculis . .	33.6	47 17	—	5.8	5.4	5.4	—	—	—	5.97
287	$\chi$ Herculis . .	47.7	42 51	4.2	4.4	4.6	4.3	—	—	4.81	4.73
288	2 Herculis . .	49.8	43 34	4.9	5.8	5.4	5.4	—	—	—	5.42
289	4 Herculis . .	50.7	42 58	4.8	5.8	5.8	5.8	—	—	—	6.02
290	$\upsilon$ Herculis . .	58.3	46 26	4.2	4.4	4.6	4.2	—	—	4.88	4.88
291	$\varphi$ Herculis . .	16 4.2	45 20	3.9	4.1	4.1	3.8	—	—	4.53	4.59
292	19 $\delta$ Herculis	7.0	42 45	—	—	6.3	6.1	—	—	—	5.90:
293	23 $\delta$ Herculis	15.0	40 2	—	5.8	5.8	5.4	—	—	—	5.68
294	$\tau$ Herculis . .	15.4	46 42	3.6	3.5	3.5	3.7	—	3.79	4.15	4.21
295	$g$ Herculis . .	23.9	42 12	4.6	5.2	Var.	4.7	—	—	—	5.00:
296	40 $\delta$ Herculis	27.4	45 56	—	5.8	5.8	5.5	—	—	—	5.96
297	$\sigma$ Herculis . .	29.4	42 45	4.0	4.1	4.4	4.0	—	—	4.41	4.43
298	46 $\delta$ Herculis	32.0	46 55	—	5.8	6.3	5.8	—	—	—	5.98
299	DM. 49°2530	34.6	49 9	—	—	—	7.2	—	—	—	6.37:
300	42 Herculis . .	34.8	49 13	4.5	5.2	4.9	4.9	—	—	—	4.86
301	$\eta$ Herculis . .	37.9	39 11	3.2	3.2	3.5	3.1	—	3.71	3.66	3.65
302	52 Herculis . .	45.0	46 14	4.3	4.4	4.9	4.9	—	—	—	5.16
303	78 $\delta$ Herculis	49.3	47 39	—	—	6.3	6.1	—	—	—	5.99:
304	95 $\delta$ Herculis	1.0	49 1	—	—	6.3	5.6	—	—	—	6.27:
305	98 $\delta$ Herculis	3.1	40 42	—	5.8	5.8	6.1	—	—	—	6.44
306	99 $\delta$ Herculis	3.5	48 35	—	—	5.8	6.4	—	—	—	6.64:
307	95 $\delta$ Draconis	4.7	51 2	—	—	6.3	6.4	—	—	—	6.64:
308	100 $\delta$ Herculis	4.8	40 58	—	4.9	4.9	5.1	—	—	—	5.22
309	DM. 45°2504	7.5	45 31	—	—	—	6.1	—	—	—	6.74

No.	Name	RA.	Dec.	W.	A.	S.	DM.	h.	G.	Z.	P.
310	DM. 49°2604	17 <sup>h</sup> 8 <sup>m</sup> 0	49°56'	—	—	—	6.1	—	—	—	6.14
311	110 $\delta$ Herculis	12.7	43 17	—	—	5.8	neb.	neb.	—	—	8.47:
312	112 $\delta$ Herculis	13.1	49 52	—	—	5.8	7.1	—	—	—	not. obs.
313	118 $\delta$ Herculis	15.7	45 26	—	—	6.3	6.4	—	—	—	6.53:
314	DM. 44°2695	16.2	44 21	—	—	—	6.4	—	—	—	6.74:
315	74 Herculis .	16.3	46 22	5.9	5.3	5.4	5.4	—	—	—	5.61
316	120 $\delta$ Herculis	16.6	48 20	—	—	6.3	6.4	—	—	—	6.46:
317	121 $\delta$ Herculis	17.0	40 7	—	4.9	5.2	5.1	—	—	—	5.75
318	$\alpha$ Herculis . .	22.9	48 24	5.7	5.8	5.4	5.8	—	—	—	5.94
319	135 $\delta$ Herculis	28.5	41 21	—	5.8	5.8	5.4	—	—	—	5.87
320	$\gamma$ Herculis . .	32.9	48 41	5.7	5.8	5.4	5.8	—	—	—	5.60
321	$\iota$ Herculis . .	35.4	46 5	3.8	3.5	3.5	4.0	3.55	3.98	4.28	4.21
322	DM. 41°2882	36.7	41 44	—	—	—	6.4	—	—	—	7.14:
323	156 $\delta$ Herculis	41.1	38 57	—	5.8	6.3	5.8	—	—	—	6.52
324	157 $\delta$ Herculis	41.2	39 23	—	—	6.3	5.8	—	—	—	6.36
325	164 $\delta$ Herculis	43.2	47 41	—	5.8	5.4	6.1	—	—	—	6.62
326	DM. 47°2541	44.1	47 38	—	—	—	6.7	—	—	—	6.85:
327	$\mathbf{z}$ Herculis . .	46.3	48 27	5.9	5.8	5.8	6.4	—	—	—	6.84
328	DM. 40°3225	46.5	40 6	—	—	—	6.6	—	—	—	6.64:
329	168 $\delta$ Herculis	47.4	40 1	—	—	6.3	6.1	—	—	—	5.94:
330	169 $\delta$ Herculis	48.0	46 42	—	5.8	5.8	6.2	—	—	—	6.53
331	$f$ Herculis . .	48.6	40 2	5.1	4.9	4.9	4.9	—	—	—	5.26
332	$\theta$ Herculis . .	51.3	37 16	3.8	4.1	4.1	3.7	—	4.23	3.96	3.64
333	DM. 45°2626	52.5	45 1	—	—	—	6.6	—	—	—	6.86:
334	DM. 45°2627	52.7	45 23	—	—	—	6.1	—	—	—	6.21:
335	DM. 45°2629	53.0	45 53	—	—	—	6.2	—	—	—	6.82:
336	179 $\delta$ Herculis	54.7	45 28	—	5.8	5.8	6.2	—	—	—	6.62
337	182 $\delta$ Herculis	55.8	45 31	—	5.8	6.3	5.8	—	—	—	5.80:
338	185 $\delta$ Herculis	59.4	48 29	—	5.8	6.3	6.1	—	—	—	6.35

No.	Name	RA.	Dec.	W.	A.	S.	DM.	h.	G.	Z.	P.
339	DM. 49°2728	18 <sup>h</sup> 2 <sup>m</sup> 5	49°28'	—	—	—	7.2	—	—	—	6.66:
340	196δ Herculis	3.2	43 27	—	4.9	5.2	5.1	—	—	—	5.11:
341	197δ Herculis	3.5	49 41	—	5.8	6.3	6.4	—	—	—	6.41:
342	202δ Herculis	7.8	48 16	—	5.8	6.3	6.4	—	—	—	6.68
343	1δ Lyrae . . .	8.1	41 7	—	—	5.8	5.6	—	—	—	6.19:
344	2δ Lyrae . . .	8.3	38 46	—	5.8	5.8	5.8	—	—	—	6.09
345	3δ Lyrae . . .	11.1	42 8	—	5.3	5.2	5.4	—	—	—	5.75
346	4δ Lyrae . . .	12.5	40 53	—	5.8	5.8	5.8	—	—	—	6.17
347	DM. 49°2776	17.5	49 39	—	—	—	6.4	—	—	—	6.46:
348	143δ Draconis	17.8	49 3	—	4.9	4.9	5.0	—	—	—	5.01
349	μ Lyrae . . .	19.5	39 26	5.5	5.2	4.9	5.0	—	—	—	5.20
350	ε Lyrae (bor.)	39.5	39 31	4.2}	4.1	{ 4.4	4.3	—	—	—	4.86
351	ε Lyrae (aust.)	39.6	39 28	4.2}	4.1	{ 4.6	4.5	—	—	—	4.82
352	ζ Lyrae (bor.)	39.8	37 28	4.1}	4.4	{ 4.4	—	—	—	—	{ 5.28:
353	ζ Lyrae (aust.)	39.8	37 27	4.2}	4.4	{ 4.4	{ 5.4	—	—	4.32	{ 5.78:
354	25δ Lyrae . .	41.6	41 17	—	5.8	5.8	5.8	—	—	—	6.13
355	164δ Draconis	44.4	48 36	—	5.8	5.8	5.8	—	—	—	6.20
356	31δ Lyrae . .	47.4	41 13	—	5.8	5.8	6.4	—	—	—	6.25
357	36δ Lyrae . .	50.2	41 25	—	5.8	5.8	5.4	—	—	—	5.62
358	R Lyrae . . .	50.9	43 45	4.8	4.6	Var.	Var.	—	—	—	4.40:
359	171δ Draconis	51.0	48 41	—	5.8	5.8	5.4	—	—	—	5.83
360	43δ Lyrae . .	54.0	40 29	—	5.8	5.8	6.4	—	—	—	6.22
361	16 Lyrae . . .	57.4	46 44	5.3	4.9	5.2	5.4	—	—	—	5.14
362	52δ Lyrae . .	19	1.6	41 12	—	5.8	5.8	6.1	—	—	6.42
363	DM. 47°2779	8.2	47 8	—	—	—	6.4	—	—	—	6.38:
364	1δ Cycni . . .	8.3	49 35	—	5.8	5.8	6.1	—	—	—	6.02
365	η Lyrae . . .	8.8	38 54	4.6	4.4	4.4	4.4	—	—	4.64	4.46
366	θ Lyrae . . .	11.4	37 53	4.7	4.4	4.4	4.5	—	—	4.54	4.25
367	2δ Cycni . . .	12.7	46 44	—	5.8	5.8	6.0	—	—	—	5.98

No.	Name	RA.	Dec.	W.	A.	S.	DM.	h.	G.	Z.	P.	
368	DM. 49°2977	19 <sup>h</sup> 14 <sup>m</sup> 8	49°18'	—	—	5.8	5.4	5.4	—	—	6.15:	
369	69 $\delta$ Lyrae .	19.4	43 6	—	—	5.8	5.4	5.4	—	—	5.85	
370	DM. 44°3133	21.5	44 39	—	—	5.8	6.3	6.1	—	—	6.82:	
371	7 $\delta$ Cygni . .	21.6	44 43	—	—	5.8	5.4	6.7	—	—	7.24:	
372	17 $\delta$ Cygni . .	30.0	42 5	—	—	5.8	5.4	5.6	—	—	5.43	
373	20 $\delta$ Cygni . .	32.1	44 23	—	—	5.8	5.4	5.2	—	—	5.26	
374	θ Cygni . .	32.6	49 53	5.0	4.6	4.6	4.6	4.7	—	—	4.70	
375	14 Cygni . .	34.7	42 29	5.2	5.3	5.4	5.4	5.8	—	—	5.44	
376	26 $\delta$ Cygni . .	36.4	45 11	—	—	5.4	5.4	5.4	—	—	5.23	
377	27 $\delta$ Cygni . .	37.0	39 55	—	—	5.8	5.8	6.1	—	—	6.25	
378	DM. 41°3469	38.9	41 26	—	—	—	—	5.8	—	—	5.61:	
379	δ Cygni . .	40.5	44 46	2.9	3.2	2.8	2.8	3.1	3.03	2.87	2.94	3.05
380	34 $\delta$ Cygni . .	43.2	47 34	—	—	5.8	5.8	5.6	—	—	6.10	
381	37 $\delta$ Cygni . .	45.7	40 14	—	—	5.8	5.8	5.4	—	—	5.78	
382	DM. 47°2937	47.0	47 2	—	—	—	—	6.1	—	—	6.44:	
383	39 $\delta$ Cygni . .	47.6	46 40	—	—	5.8	5.8	5.6	—	—	5.78	
384	40 $\delta$ Cygni . .	47.9	47 35	—	—	5.8	5.8	5.6	—	—	6.01	
385	DM. 47°2945	48.8	48 27	—	—	—	—	6.7	—	—	6.32	
386	49 $\delta$ Cygni . .	52.2	39 59	—	—	5.3	5.4	5.6	—	—	5.61	
387	52 $\delta$ Cygni . .	53.1	41 52	—	—	5.8	5.8	6.4	—	—	6.57	
388	56 $\delta$ Cygni . .	54.8	45 23	—	—	5.8	5.8	5.6	—	—	5.92	
389	e Cygni . .	57.3	49 42	5.3	5.3	5.3	5.2	5.0	—	—	5.16	
390	61 $\delta$ Cygni . .	20 0.2	47 49	—	—	5.8	5.8	6.1	—	—	6.23:	
391	DM. 47°3037	6.2	47 48	—	—	—	—	6.2	—	—	6.56:	
392	o <sub>1</sub> Cygni (pr.)	8.8	46 23	5.5}	4.1	4.4	{ 4.9	—	—	—	5.15	
393	o <sub>1</sub> Cygni (sq.)	9.1	46 18	4.5}	—	—	{ 4.0	—	—	4.20	4.00	
394	o <sub>2</sub> Cygni . .	11.0	47 16	4.8	4.4	4.6	4.6	4.9	—	4.35	4.13	
395	76 $\delta$ Cygni . .	11.3	45 8	—	—	5.8	5.8	6.1	—	—	6.02	
396	77 $\delta$ Cygni . .	11.8	39 55	—	—	5.8	5.4	5.3	—	—	5.29	

No.	Name	RA.	Dec.	W.	A.	S.	DM.	h.	G.	Z.	P.
397	80 $\delta$ Cygni . .	20 <sup>h</sup> 13 <sup>m</sup> 0	40°17'	—	—	6.3	6.1	—	—	—	6.02
398	DM. 45°3139	14.2	45 52	—	—	—	6.5	—	—	—	6.46:
399	84 $\delta$ Cygni . .	15.3	46 23	—	5.8	5.8	6.4	—	—	—	6.45
400	DM. 40°4136	16.9	40 40	—	—	—	6.4	—	—	—	6.38
401	$\gamma$ Cygni . . .	17.0	39 47	2.2	2.6	2.6	2.8	2.33	2.42	2.09	2.64
402	86 $\delta$ Cygni . .	17.4	45 19	—	5.8	5.8	5.6	—	—	—	5.66
403	87 $\delta$ Cygni . .	17.6	40 34	—	5.8	5.8	5.8	—	—	—	5.83
404	$\omega_1$ Cygni . . .	22.6	48 54	5.7	—	5.8	5.8	—	—	—	5.88
405	$\omega_2$ Cygni (pr.)	25.6	48 28	5.2	4.9	4.9	4.7	—	—	—	5.28
406	$\omega_2$ Cygni (sq.)	26.8	48 44	5.5	5.3	5.4	5.7	—	—	—	5.53
407	104 $\delta$ Cygni . .	29.2	46 12	—	5.8	5.8	5.8	—	—	—	5.99
408	109 $\delta$ Cygni . .	34.3	40 4	—	5.8	5.8	5.8	—	—	—	6.12
409	110 $\delta$ Cygni . .	35.0	42 56	—	5.8	5.8	5.8	—	—	—	5.90
410	$\alpha$ Cygni . . .	36.5	44 46	1.6	1.7	1.5	1.5	1.49	1.28	1.46	1.82
411	114 $\delta$ Cygni . .	36.7	41 12	—	5.8	5.4	5.2	—	—	—	5.72
412	51 Cygni . . .	37.7	49 49	5.3	5.3	5.2	5.5	—	—	—	5.76
413	118 $\delta$ Cygni . .	39.8	46 46	—	5.8	5.8	6.4	—	—	—	6.99:
414	123 $\delta$ Cygni . .	43.1	47 18	—	—	6.3	5.4	—	—	—	5.71:
415	55 Cygni . . .	44.0	45 35	5.1	5.3	5.4	5.4	—	—	—	5.06
416	56 Cygni . . .	45.0	43 31	5.1	5.2	5.4	5.4	—	—	—	5.17
417	57 Cygni . . .	48.1	43 50	4.8	5.2	5.2	5.3	—	—	—	5.03
418	132 $\delta$ Cygni . .	48.3	44 38	—	5.8	5.8	6.1	—	—	—	5.47
419	134 $\delta$ Cygni . .	50.9	46 52	—	5.8	6.3	5.8	—	—	—	5.89
420	DM. 44°3639	51.5	44 23	—	—	—	6.6	—	—	—	5.99:
421	$\nu$ Cygni . . .	51.8	40 37	4.7	4.1	4.1	4.0	—	—	—	4.21
422	139 $\delta$ Cygni . .	53.1	43 54	—	5.8	5.4	5.8	—	—	—	5.58
423	140 $\delta$ Cygni . .	53.9	49 54	—	5.8	5.4	5.4	—	—	—	5.70:
424	$f_1$ Cygni . . .	54.9	46 57	5.2	5.2	5.2	5.2	—	—	—	5.43
425	60 Cygni . . .	56.1	45 35	5.6	5.8	5.2	5.5	—	—	—	5.57

No.	Name	RA.	Dec.	W.	A.	S.	DM.	h.	G.	Z.	P.
426	$\xi$ Cycni . . . .	20 <sup>h</sup> 59 <sup>m</sup> 7	43°21'	4.2	4.1	4.1	4.0	—	—	—	4.12
427	$f_2$ Cycni . . . .	21 1.6	47 4	5.1	5.2	5.2	4.5	—	—	—	4.57
428	$\tau$ Cycni . . . .	9.0	37 25	4.0	4.1	4.1	4.0	—	—	3.82	4.04
429	$\sigma$ Cycni . . . .	11.7	38 47	4.7	4.4	4.4	4.4	—	—	4.48	4.43
430	$A$ Cycni . . . .	11.9	42 5	—	5.8	5.8	6.2	—	—	—	6.31
431	68 Cycni . . . .	13.1	43 20	5.7	4.9	4.9	5.2	—	—	—	5.31
432	162 $\delta$ Cycni . . . .	13.3	40 26	—	—	6.3	6.4	—	—	—	6.25
433	164 $\delta$ Cycni . . . .	14.5	48 54	—	5.2	5.4	5.3	—	—	—	5.81
434	168 $\delta$ Cycni . . . .	17.0	48 46	—	5.3	5.8	5.4	—	—	—	5.96
435	171 $\delta$ Cycni . . . .	20.0	46 5	—	5.8	5.8	5.8	—	—	—	5.88
436	173 $\delta$ Cycni . . . .	21.7	48 12	—	4.9	5.2	5.4	—	—	—	5.53
437	$g$ Cycni . . . .	24.1	45 54	5.1	4.9	4.9	5.3	—	—	—	5.46
438	180 $\delta$ Cycni . . . .	27.8	49 18	—	—	5.8	5.8	—	—	—	6.09:
439	$\rho$ Cycni . . . .	28.5	44 57	4.0	4.4	4.1	4.2	—	—	4.12	4.24
440	Following $\rho$ Cycni	—	—	—	—	—	—	—	—	—	5.35:
441	74 Cycni . . . .	31.1	39 46	5.0	4.9	4.9	4.9	—	—	—	5.26
442	75 Cycni . . . .	34.5	42 37	5.0	5.3	5.4	5.1	—	—	—	5.20
443	76 Cycni . . . .	35.8	40 9	5.9	—	—	6.1	—	—	—	6.09
444	77 Cycni . . . .	36.6	40 25	5.6	—	—	5.7	—	—	—	5.89
445	189 $\delta$ Cycni . . . .	37.3	40 30	—	5.2	5.4	5.0	—	—	—	5.47
446	$\pi_2$ Cycni . . . .	41.4	48 38	4.6	4.4	4.4	4.4	—	—	—	4.52
447	195 $\delta$ Cycni . . . .	43.8	40 28	—	5.8	5.8	6.1	—	—	—	6.67:
448	1 $\delta$ Lacertae . . . .	57.1	43 57	—	5.8	5.4	5.8	—	—	—	5.88
449	2 $\delta$ Lacertae . . . .	22 0.2	44 19	—	5.3	5.4	4.6	—	—	—	5.35
450	DM. 45°38'13 . . .	2.8	45 2	—	—	—	6.1	—	—	—	6.77:
451	6 $\delta$ Lacertae . . . .	7.9	44 43	—	5.8	5.8	5.8	—	—	—	5.89
452	2 Lacertae . . . .	15.0	45 48	4.7	4.6	4.6	4.9	—	—	—	4.91
453	4 Lacertae . . . .	18.6	48 45	4.9	4.9	4.9	4.9	—	—	—	4.95
454	5 Lacertae . . . .	23.5	46 58	4.9	4.9	4.9	4.7	—	—	—	4.60

No.	Name	RA.	Dec.	W.	A.	S.	DM.	h.	G.	Z.	P.
455	6 Lacertae . . .	22 <sup>h</sup> 24 <sup>m</sup> 2	42°23'	4.9	4.9	4.9	4.9	—	—	—	4.97
456	7 Lacertae . . .	25.3	49 32	4.4	4.1	4.1	4.0	—	—	—	4.13
457	11 Lacertae . . .	34.2	43 31	5.1	4.9	4.9	4.7	—	—	—	4.63
458	13 Lacertae . . .	37.7	41 4	5.3	5.8	5.8	5.1	—	—	—	5.38
459	14 Lacertae . . .	43.8	41 11	5.7	5.8	5.8	6.0	—	—	—	6.11
460	15 Lacertae . . .	45.5	42 31	5.3	5.8	5.4	4.9	—	—	—	5.15
461	40 <sup>h</sup> Lacertae . .	47.2	43 59	—	5.8	6.3	5.8	—	—	—	6.07
462	16 Lacertae . . .	49.8	40 51	5.7	5.8	5.4	6.1	—	—	—	5.87
463	45 <sup>h</sup> Lacertae . .	50.1	48 58	—	5.8	5.4	4.5	—	—	—	5.16:
464	46 <sup>h</sup> Lacertae . .	50.7	47 55	—	5.8	5.2	5.2	—	—	—	5.42:
465	o Andromedae .	55.3	41 33	3.7	3.8	3.8	3.8	—	—	3.72	3.86
466	2 Andromedae .	56.0	41 58	5.6	5.8	5.8	6.1	—	—	—	5.46
467	3 Andromedae .	57.7	49 16	5.1	5.2	4.9	4.7	—	—	—	4.73
468	4 Andromedae .	23 1.0	45 36	5.9	5.8	5.8	5.4	—	—	—	5.42
469	5 Andromedae .	1.2	48 31	4.6	4.9	4.9	4.9	—	—	—	5.74:
470	6 Andromedae .	3.8	42 47	6.1	—	6.3	5.8	—	—	—	6.02
471 <sup>14</sup>	DM. 44°4347 . .	4.2	44 44	—	—	—	7.2	—	—	—	6.54:
472	7 Andromedae .	6.0	48 37	4.6	4.9	4.9	4.9	—	—	—	4.77
473	11 <sup>h</sup> Andromedae	10.5	44 24	—	5.8	5.8	6.1	—	—	—	6.57:
474	8 Andromedae .	11.1	48 13	5.1	5.2	5.2	4.7	—	—	—	4.92
475	DM. 44°4373 . .	11.1	44 42	—	—	—	6.4	—	—	—	6.41:
476	9 Andromedae .	11.5	40 59	6.4	5.8	5.8	6.1	—	—	—	6.21
477	DM. 47°4107 . .	12.2	47 42	—	—	—	7.4	—	—	—	7.30:
478	DM. 44°4378 . .	12.2	44 21	—	—	—	6.7	—	—	—	6.65:
479	11 Andromedae	12.7	47 50	5.4	5.8	5.8	5.8	—	—	—	5.55
480	DM. 47°4114 . .	12.9	47 36	—	—	—	6.4	—	—	—	6.38:

14. 471. Some uncertainty about the identification.

No.	Name	RA.	Dec.	M.	A.	S.	DM.	h.	G.	Z.	P.
481	10 Andromedae	23 <sup>h</sup> 13 <sup>m</sup> 0	41°16'	6.4	—	5.8	5.7	—	—	—	5.95
482	13 Andromedae	20.2	42 7	5.8	5.8	5.8	6.1	—	—	—	6.01
483	25 $\delta$ Andromedae	30.5	43 37	—	—	6.3	6.1	—	—	—	5.94:
484	$\lambda$ Andromedae .	30.5	45 41	4.1	4.1	4.1	3.7	—	—	3.85	3.89
485	DM. 48°4112 . .	30.9	48 12	—	—	—	7.3	—	—	—	6.40:
486	$\iota$ Andromedae .	31.1	42 28	4.6	4.1	4.1	4.2	—	—	4.41	4.44
487	18 Andromedae	32.1	49 40	5.1	5.3	5.4	5.5	—	—	—	5.53
488	$\kappa$ Andromedae .	33.3	43 31	4.6	4.1	4.4	4.6	—	—	4.28	4.36
489	$\psi$ Andromedae .	38.9	45 37	5.1	4.9	5.4	5.1	—	—	—	5.07
490	34 $\delta$ Andromedae	40.4	46 1	—	—	6.3	6.4	—	—	—	6.24
491	39 $\delta$ Andromedae	49.7	41 51	—	5.8	5.8	6.1	—	—	—	6.32
492	42 $\delta$ Andromedae	54.3	41 33	—	5.8	5.8	6.4	—	—	—	6.52
493	DM. 41°4925 . .	55.7	41 56	—	—	—	6.7	—	—	—	6.73:
494	DM. 41°4933 . .	57.2	41 18	—	—	—	5.8	—	—	—	6.37

A number of rejections of observations have been made in taking the differences of the magnitudes of stars from the means of their groups, which, having been made after the calculations of the differences of the means of the groups, do not affect these differences. The following table will give some idea of what effect these rejections would have had.

Groups	Number of set	Obs. before rejection	Obs. after rejection	Calc.	No. of rounds	Remarks
IX – XI	(105)	+0.22	+0.25	+0.20	2	
XI – XII	(186)	+0.72	+0.63	+0.80	2	Clouds.
XVIII – XIX	(122)	+0.20	+0.18	+0.26	3	Posture very uncomfortable.
XVIII – XX	(48)	-0.02	+0.01	+0.18	1	Hazy, windy, altitude low. Comp. not used.
XXIII – XXIV	(15)	-1.80	-1.89	-1.90	3	Only night when stars were observed on their own color.
XLII – XLIV	(74)	+0.37	+0.32	+0.30	1	Strong wind; lamp blown out.
LVI – LXVIII	(46)	+0.16	+0.27	+0.28	4	Great changes in brightness of lamp.
LVIII – LX	(79)	-0.31	-0.20	-0.07	1	Bad seeing, lamp blown. Comp. not used.
LXI – LXIII	(90)	+0.60	+0.45	+0.37	2	Very windy. Effect of pin-hole correction. Comp. not used.
LXVII – LXIX	(103)	-0.97	-1.07	-1.08	3	36 <i>Persei</i> rej. Possibly variable. Obs. good.
I – XXI	(177)	-1.49	-1.61	-1.61	2	Presence of clouds suspected.
XXIII – XLI	(62)	-3.16	-3.02	-3.11	2	<i>z Herculis</i> rej. Suspected variable.
XI – XXXI	(117)	+0.52	+0.40	+0.71	1	Wrong star observed.
LI – I	(155)	+1.34	+1.29	+1.33	2	Obs. of rejected star are very discordant.

The following stars were observed with more than one group and we have therefore different independent results for their magnitudes.

Name of star	Group	No. of nights	Mag.	Group	No. of nights	Mag.	Adopted mag.
42 Aurigae	XIII	4	6.41	Extra XII	1	6.20:	6.37
43 Aurigae	XIII	4	6.17	Extra XII	1	6.20:	6.18
$\psi_1$ Aurigae	XIII	4	4.76	Extra XII	1	4.66:	4.74
35 Lyncis	XVI	4	5.18	Extra XVII	2	5.15:	5.17
105 $\delta$ Ursae majoris	XXI	7	5.36	Extra XX	1 rej.	4.95:	5.36
121 $\delta$ Ursae majoris	XXI	7	5.53	Extra XX	1 rej.	5.09:	5.53
$\chi$ Ursae majoris	XXIII	7	3.74	Extra XX	1 rej.	3.10:	3.74
59 $\delta$ Canum venat.	XXX	6	6.58	Extra XXIX	1 rej.	6.13:	6.58
$g$ Herculis	Extra XXXVI	4	5.00:	Extra XXXVIII	1	4.99:	5.00:
182 $\delta$ Herculis	Extra XLI	5	5.76:	Extra XLIII	1	5.98:	5.80:
17 $\delta$ Cygni	XLVIII	4	5.42	XLVI	3	5.44	5.43
14 Cygni	XLVIII	4	5.38	XLVI	3	5.52	5.44
37 $\delta$ Cygni	XLVIII	4	5.75	XLVI	3	5.81	5.78
49 $\delta$ Cygni	XLVIII	4	5.60	Extra XLVI	2	5.66:	5.61
52 $\delta$ Cygni	XLVIII	4	6.61	Extra XLVI	1	6.39:	6.57

The following remarks relate to known or suspected variables.

10.  $\xi$  Cassiepeae. Argelander makes this star 5.8, and Heis, 4.6. Wm. Herschel makes it 0.4 fainter than  $\alpha$ ; Argelander, 0.9 fainter; Heis, 0.3 brighter; the DM., 0.3 fainter; and I, 0.1 fainter. 1876, Apr. 8, it seems about equal to  $\alpha$ .
11.  $\pi$  Cassiepeae. Argelander makes this star 5.8, and the DM., 4.4. Wm. Herschel makes it 0.2 fainter than  $\alpha$ ; Argelander, 0.9 fainter; Heis, equal; the DM., 0.3 brighter; and I, 0.3 fainter. 1876, Apr. 8, I find it but little than  $\alpha$ .
53.  $\beta$  Persei. This variable was always observed near its maximum.
88. 121 $\delta$  Persei. The absence of this star from the *Uranometria*, and the great difference between the magnitude assigned to it by Heis and me (5.8) and that of the *Durchmusterung* (7.7) certainly creates a suspicion of variability.
98.  $\epsilon$  Aurigae. An irregular variable.
129. 18 $\delta$  Lyncis. Heis makes this star 6.3, and I, 5.0. Argelander makes it equal to 26 Lyncis; Heis makes it 0.5 fainter; the DM., 0.2 brighter; and I, 0.4 brighter. Argelander makes it equal to 16 $\delta$  Lyncis, Heis makes it 0.5 fainter; the DM., 0.3 brighter; and I, 0.5 brighter. 1876, April 8, it seems not to have changed since my measures.
190.  $\psi$  Ursae majoris. Ptolemy made this star 4.5 and 1.2 fainter than  $\mu$ , which it now equals. 1876, April 8, I find  $\mu > \psi > \theta$ . The star is orange.
201. 65 Ursae majoris. I have rejected this star from the mean of its group, not because I think it variable but because its near companion makes accordant measures very difficult to obtain.
218. 9 Canum venaticorum. The magnitudes of Argelander and Heis differ very much from that of the DM., and my measures are discordant. But I have excluded the star from the group, not so much on suspicion of variability, as because I do not feel sure that I always observed the same star, as there are three near together.
247. 65 $\delta$  Canum venaticorum. Argelander and Heis make this 5.8; I, 6.9. They make it equal to 60 $\delta$ ; the DM. makes it 0.3 fainter; and I, 0.7 fainter. Argelander and the DM. make it equal to 73 $\delta$ ; Heis makes it 0.5 brighter; and I, 0.1 fainter.
255.  $\eta$  Ursae majoris. Two measures in June, 1873, make this star 0.5 brighter than in June, 1874. But unfortunately the values

- of the cap constants are so uncertain that nothing can be concluded.
268.  $42\frac{1}{2}$  *Bootae*. The configuration of stars in this neighborhood is perplexing, and possibly two different stars are confounded.
269.  $\theta$  *Bootae*. The disagreement of different observers in reference to the relative magnitudes of  $\theta$  and  $\lambda$  *Bootae* is noticeable. On 1875, July 27, at Munich, I found  $\theta$  much the brighter.
276.  $112\frac{1}{2}$  *Bootae*. Although this faint star was measured every night, yet  $73\frac{1}{2}$  which is marked as 6.0 in the DM. entirely escaped observation. This was owing partly to its omission from the *Uranometria* and partly to its being near the junction of three groups.
279. k *Bootae*. Argelander makes this star 4.9; I, 6.0. Wm. Herschel makes it equal to 39; the DM., 0.2 brighter; Heis, 0.6 brighter; Argelander 0.9 brighter; and I, 0.1 fainter. 1875, July 27, at Munich, I inclined to think it brighter than 39. 1876, April 7, at Berlin, I found it a little brighter than 39, and made its magnitude by comparison with 39, *h*, and  $134\frac{1}{2}$ , to be 5.75.
295. g *Herculis*. An irregular variable.
312.  $112\frac{1}{2}$  *Herculis*. This I believe is the only star in the *Uranometria* within the assigned limits of declination which I never succeeded in observing. I have little doubt that the identification of the Argelander and Heis is wrong and that they really observed DM.  $49^{\circ}2604$ .
327. z *Herculis*. I am confident that this star is variable. It is now of the 7th magnitude and very considerably fainter than  $120\frac{1}{2}$  or  $148\frac{1}{2}$ , which Heis and the DM. call 6.3 but which are, in fact, hardly brighter than 6.6. Yet the star was seen by Ptolemy who makes it 5.6; in which he is supported by Sūfi and Ulugh. Tycho and Hevelius call it a nebula. Wm. Herschel makes it 5.9; Argelander and Heis, 5.8; the DM., 6.4; and I, 6.9. Wm. Herschel makes *z* 0.2 fainter than *y*; Argelander makes them equal; Heis makes *z* 0.4 fainter; the DM., 0.6 fainter; and I, 1.3 fainter. 1876, April 6, I find that my magnitudes of stars in this vicinity continue to represent them well.
- By mistake, *z Herculis* has been retained in the group and  $182\frac{1}{2}$ , a star which there is no reason to suspect, has been excluded.
334. DM.  $45^{\circ}2627$ . I believe this star to be variable. I found it the brightest of the group except  $182\frac{1}{2}$  *Herculis*, and yet neither

- the *Uranometria* nor Heis has it. My measures show some discrepancies, even rejecting that of set (19), which is affected by a large color correction. The star remarkably ruddy.
340. 196 $\delta$  *Herculis*. This star was rejected from the mean of the group on an inadequate suspicion of variability.
358. R *Lyrae*. A known variable.
378. DM. 41°3469. Although I make this star 5.6, and the DM. marks it 5.8. It is omitted by both Argelander and Heis, and its variability is highly probable. When I first observed it, it seemed brighter than 14 *Cycni*, but I think its light afterwards diminished.
398. DM. 45°3139. This star was omitted from the group on a suspicion of variability which is not without foundation.
- 413, 414. 118 $\delta$  and 123 $\delta$  *Cycni*. The *Uranometria* only contains the former star as 5.8; Heis calls the former 5.8 and the latter 6.3; the DM. calls the former 6.4 and the latter 5.4; and I call the former 7.0 and the latter 5.7. I only succeeded in observing Argelander's star, once; and never failed to observe the other, which seemed to me brighter than 51 *Cycni*.
440. *Star following p Cycni*. This star was perhaps DM. + 45°3637. 1875, Aug. 2, I could find no star which could possibly be identified with the star observed. The star was red.
449. 2 $\delta$  *Lacertae*. I have some slight suspicion of this star, founded on the discrepancies in my measures, and the fact that the DM. makes it 0.8 brighter than Heis. The star is reddish.
463. 45 $\delta$  *Lacertae*. This is a reddish star and there is a difference of 1.3 between the magnitudes of the *Uranometria* and the *Durchmusterung*. I have rejected not only this star but also 46 $\delta$  from the mean of the group, but my suspicion of the latter depends only on the discrepancies of my own measures.
469. 5 *Andromedae*. The magnitude of Sir Wm. Herschel differs by 1.1 from mine.

#### CHAPTER V. ON THE FORM OF THE GALACTIC CLUSTER

The chief end of observations of the magnitudes of stars is to determine the form of the cluster in which our sun is situated. Sir Wm. Herschel assumed the star-density to be homogeneous throughout the cluster, and sought to determine by his gauges the shape of its exterior surface. Struve assumed the condensation of stars to be

in parallel planes, and sought to ascertain the numerical law of the decrease in density with the distance from the central plane. I shall make no attempt to obtain numerical accuracy in my inferences upon this subject, but shall content myself with endeavoring to show the general forms of the surfaces of equal star-density throughout the cluster. I shall make the assumption that the proportions of stars of different intrinsic lights is the same in all parts of space. I shall at first proceed as if the intrinsic light of all stars were the same, and I shall afterwards inquire how far the conclusions so derived are affected by the assumption of the greatest possible variation in the intrinsic light of the stars.

- Let  $L$  be the intrinsic light of a star;
- $l$ , its apparent light;
- $r$ , its distance;
- $FL \cdot dL$ , the proportion of stars whose intrinsic light lies between  $L$  and  $L + dL$  (which I assume to be the same throughout space);
- $\mathcal{f} \cdot dl$ , the number of stars whose apparent light lies between  $l$  and  $l + dl$ ;
- $\varphi r$ , the number of stars in a unit of space at the distance  $r$ .

Our problem is to determine the general forms of the surfaces for which  $\varphi r$  is constant. The given quantities are the values of  $\int \mathcal{f} \cdot dl$ .

Let us consider a portion of the heavens equal to  $\frac{1}{4\Theta}$  of the entire sphere. The volume of space in that direction, at the distance  $r$ , is equal to  $r^2 \cdot dr$ , and the number of stars in that space is equal to  $\varphi r \cdot r^2 \cdot dr$ . Then the number of stars in that space whose intrinsic brightness lies between  $L$  and  $L + dL$  is equal to  $FL \cdot \varphi r \cdot r^2 \cdot dr \cdot dL$ . Now we have

$$(1) \quad l = \frac{L}{r^2}$$

and therefore, substituting for  $L$  its value as given by this equation, and integrating, we have for the total number of stars in the part of the heavens just defined, whose apparent brightness lies between  $l$  and  $l + dl$ ,

$$(2) \quad f\ell \cdot dl = dl \int_0^{\infty} F(lr^2) \cdot \varphi r \cdot r^4 \cdot dr.$$

If  $\varphi r$  had a constant value,  $b$ , throughout space, we should have, on substituting  $L$  for  $lr^2$ ,

$$\begin{aligned} f\ell \cdot dl &= \left( \frac{1}{2} b \int_0^{\infty} FL \cdot L^{\frac{3}{2}} \cdot dL \right) l^{-\frac{5}{2}} \cdot dl \\ \text{or} \quad f\ell \cdot dl &= - \left( \frac{1}{3} b \int_0^{\infty} FL \cdot L^{\frac{3}{2}} \cdot dL \right) l^{-\frac{3}{2}}. \end{aligned}$$

The numbers of stars as bright or brighter than any given apparent light, ought therefore to be inversely proportional to the three-halves power of the light. Now we have found, in Chapter II, that, denoting by  $v(m)$  the number of stars as bright or brighter than magnitude  $m$ , in the Northern heavens, we have

$$m = -\frac{1}{3} + 1.89 \dots \log . v(m)$$

and this would give, according to the formula just obtained, 0.352 for the logarithm of the ratio of light between successive magnitudes. This value is, however, considerably smaller than has ever been found from observation. In fact, a single glance at the heavens is sufficient to show that the stars are not uniformly distributed throughout space, but that there is a great concentration in the plane of the milky way. The effect of such a concentration obviously is to make the number of stars at a given distance proportional to something between  $r$  and  $r^2$ , so that  $v(m)$  will become proportional to something between  $l^{-1}$  and  $l^{-\frac{3}{2}}$ , and the logarithm of the ratio of light between successive magnitudes will be between 0.528 and 0.352, which limits do in fact include all the observed values of this quantity.

The stars are concentrated toward the plane of the milky way; but is the star-density constant within that plane, or subject only to irregular variations? Supposing  $L$  to be equal to unity for all stars, we have

$$\varphi r \cdot r^2 \cdot dr = f\ell \cdot dl,$$

and consequently

$$(3) \quad \varphi r = 2l^{\frac{5}{2}} \cdot f.$$

Now Argelander, in his Preface to the third volume of the *Durchmusterung*, has given the following numbers of stars of different magnitudes contained in a sample of the milky way,

Mags.	Numbers
7	562
8	2068
9	18916

These numbers are undoubtedly somewhat too small for the fainter stars, because, as Argelander has himself explained, there was some tendency to omit stars where they were the thickest. Notwithstanding this, if we take the limiting magnitudes from my table on page 26, and, using Rosén's value for the ratio of light between successive magnitudes, calculate the mean densities of the stars at the distances corresponding to the different magnitudes, on the supposition that  $L = 1$ , we obtain the following values:

Mags.	Densities
7	536
8	542
9	587

If, in place of using my reduced magnitudes, the unreduced magnitudes had been used, the increase in the density would have been still more marked. This increase of the density with the radius is not accidental, but proves that the denser parts of the milky way have really an annular form.

I next propose to consider the distribution of the brighter stars. For this purpose I divide the entire sphere of the heavens into 32 equal regions, as follows. First, a circular region around that pole of the milky way which lies in Coma Berenices, and which I term the Beronicean pole. Secondly, a similar region around the opposite pole of the milky way, which I term the Magellanic pole because the

Magellanic Clouds are upon that side. Between these, five zones, parallel to the milky way, and each divided perpendicularly into six equal parts. These five zones I term respectively the Beronicean Apogalactic, the Beronicean Perigalactic, the Engalactic, the Magellanic Perigalactic, and the Magellanic Apogalactic zones. The different regions of each zone are distinguished by the letters A, B, C, D, E, and F, each of which is appropriated to a line of regions in the same galactic meridian. The numbers of stars of each magnitude within each of these regions, as shown upon Heis's maps down to 20°S. and upon Behrmann's maps south of that parallel, are shown in the following tables. The 6th magnitude includes Heis's 6.7, which seems to have the same limit as Behrmann's 6th.<sup>15</sup>

<i>Region</i>	<i>1<sup>m</sup></i>	<i>2<sup>m</sup></i>	<i>3<sup>m</sup></i>	<i>4<sup>m</sup></i>	<i>5<sup>m</sup></i>	<i>6<sup>m</sup></i>	<i>Cl. Neb.</i>
Beronicean Pole . . . . .	1	—	4	6	41	164	2
<b>Beronicean Apogalactic:</b>							
A Virgo region . . . . .	1	3	6	6	18	132	—
B Leo region . . . . .	1	3	4	11	22	159	—
C Leo minor region . . . . .	—	—	11	9	34	174	—
D Dipper region . . . . .	—	7	8	4	40	190	1
E Hercules region . . . . .	—	3	11	17	34	175	2
F Libra region . . . . .	—	3	7	9	26	143	1
<b>Beronicean Perigalactic:</b>							
A Centaurus region . . . . .	—	—	7	10	38	149	1
B Hydra region . . . . .	—	1	3	9	28	148	2
C Gemini region . . . . .	1	1	3	9	26	202	2
D Polaris region . . . . .	—	1	4	13	44	196	—
E Lyra region . . . . .	1	1	6	17	40	201	1
F Ophiuchus region . . . . .	1	4	8	9	36	96	1
<b>Engalactic:</b>							
A Crux region . . . . .	4	2	11	20	61	182	4
B Argo region . . . . .	—	1	3	11	43	178	2
C Capella and Procyon reg.	2	3	8	6	44	197	2
D Cassiepea region . . . . .	—	4	6	17	53	228	3
E Cygnus region . . . . .	1	2	6	24	54	224	1
F Scutum region . . . . .	—	4	14	10	28	112	4

15. The numbers of stars between 20° and 30°S., according to Heis and Behrmann are as follows:—

Magnitude	1	2	3	4	5	6	6.7
5 . . . . .	1	4	12	31	78	119	68
B . . . . .	1	4	9	32	82	270	—

<i>Region</i>	1 <sup>m</sup>	2 <sup>m</sup>	3 <sup>m</sup>	4 <sup>m</sup>	5 <sup>m</sup>	6 <sup>m</sup>	<i>Cl. Neb.</i>
<b>Magellanic Perigalactic:</b>							
A Octans region . . . . .	—	2	—	16	53	151	—
B Canis major region . . . . .	2	6	6	20	66	243	1
C Orion region . . . . .	3	4	10	28	59	266	—
D Andromeda region . . . . .	—	4	4	26	45	221	1
E Equuleus region . . . . .	—	3	9	15	35	191	1
F Corona Australis region . . . . .	—	1	3	6	32	128	1
<b>Magellanic Apogalactic:</b>							
A Hydrus region . . . . .	1	—	6	11	35	204	1
B Middle Eridanus region . . . . .	—	—	2	14	38	152	—
C North Eridanus region . . . . .	—	1	7	12	19	109	—
D Pisces ribbons region . . . . .	—	—	3	7	8	75	—
E Aquarius region . . . . .	—	—	4	13	32	100	—
F Piscis Austrinus region . . . . .	1	2	2	10	36	123	—
Magellanic Pole . . . . .	—	2	4	5	29	132	—
<b>Summaries:</b>							
Polar regions ( $\times 3$ ) . . . . .	3	6	24	33	210	888	—
Apogalactic (mean) . . . . .	2	11	40½	61½	171	868	—
Perigalactic (mean) . . . . .	4	14	31½	89	251	1096	—
Engalactic . . . . .	7	16	48	88	283	1121	—
<b>All except Engalactic:</b>							
Beronicean side . . . . .	6	27	92	129	427	2129	—
Magellanic side . . . . .	7	25	60	183	487	2095	—
<b>Series:</b>							
A . . . . .	7	7	30	63	205	818	—
B . . . . .	3	11	18	65	197	880	—
C . . . . .	6	9	39	64	182	948	—
D . . . . .	0	16	25	67	190	910	—
E . . . . .	2	9	36	86	195	891	—
F . . . . .	2	14	34	44	158	602	—

The most striking fact in reference to these numbers is that the stars are as thick in the regions of the galactic poles as they are in the apogalactic regions, and that they are very nearly as thick in the perigalactic as in the engalactic regions. It seems to me that an important inference can safely be drawn from this circumstance. Let the stars differ in intrinsic light as much as they may, the number of them must at least depend upon the depth of the concentrated stratum through which we are looking, and the milky way could not be the result of a concentration of stars in parallel planes, without producing a greater concentration in the engalactic than in the perigalactic regions, these latter having a mean elevation above the great circle of the milky way, of 22°. It seems to be an unavoidable

conclusion that between the perigalactic and apogalactic regions the line of sight approaches to being tangential to the surfaces of equal condensation, and that there is therefore a locus of maximum condensation of an annular form, so that the surfaces resemble Cassinian ovals in their sections. To my mind this conclusion, based on this general consideration, is more trustworthy than anything which can be derived from a consideration of the numbers in their details. I have however made some calculations, based upon the supposition that all the stars have equal intrinsic light, adopting at the same time Rosén's photometric ratio between the different magnitudes. It is true that Rosén's ratio is probably too small for bright stars, but owing to the engalactic and perigalactic regions containing each of them parts of the heavens where the stars vary very much in thickness, it is proper to assign a small value to the ratio, because of the effect which has been pointed out above, of a concentration of the stars more or less toward planes; and if a larger ratio were taken for the apogalactic and polar regions, the general conclusions to which the assumption of Rosén's ratio leads would only be strengthened. Assuming the magnitudes of Behrmann and Heis to be the same, and adopting my reduction of Heis's scale, given on page 36, I have calculated the solid contents of the portions of space within which the stars of each magnitude would be contained, and, dividing the numbers of stars by these volumes, I get the following mean star-densities:

#### *Densities of Stars*

	1 <sup>m</sup>	2 <sup>m</sup>	3 <sup>m</sup>	4 <sup>m</sup>	5 <sup>m</sup>	6 <sup>m</sup>
Galactic Poles . .	.52	.23	.30	.18	.39	.30
Apogalactic Zones .	.34	.42	.51	.34	.32	.29
Perigalactic Zones .	.69	.54	.39	.49	.46	.37
Engalactic Zone .	1.21	.62	.60	.49	.52	.37

#### *Radii Vectores*

1 <sup>st</sup> Mag.	2 <sup>nd</sup>	3 <sup>rd</sup>	4 <sup>th</sup>	5 <sup>th</sup>	6 <sup>th</sup>
1.27	2.80	4.06	5.81	8.15	12.9

I have next plotted these densities on a system of rectangular coördinates, taking the radii vectores for abscissas and the densities

for ordinates (see Plate IV). Instead of making a separate construction for each zone, I have plotted them all upon the same construction, merely distinguishing the different zones by differently shaped dots, and have then drawn curves of the densities for the different zones, having regard, in drawing each of them, (especially for the smaller radii vectores) not merely to the position of the points for that curve, but also to the harmony of the different curves. On the curves so drawn, I have read off the radii vectores for the different densities, and have thus obtained the data for plotting the curves of equal condensation. The result is shown upon Plate V, and it will be seen that there appears to be a concentrated ring, lying much within the milky way, and at a distance of a  $2\frac{1}{2}$  mag. star. This is shown not so much by the concentrated part itself, which might result from an accidental distribution of the numbers, as it is by the concave forms of the outer surfaces of equal condensation. The existence of a ring of bright stars, in the approximate plane of the milky way, has already been noticed by more than one astronomer.

In regard to our position with reference to this ring, it is evident from the total numbers of stars on the Beronicean and Magellanic sides, that we are upon the Beronicean side of the most concentrated plane of bright stars, just as we are upon the same side of the milky way. It is true that the 6th magnitude stars do not show this, but this is probably owing to Behrmann's faintest stars not being really so faint as I have assumed them to be. The comparison of the numbers of stars on the different galactic meridians seems to show that we are not far removed from the axis of the inner ring, and the irregularity of the numbers makes it difficult to decide to which side we incline. There are somewhat more stars on the meridian C, in the direction of Capella and Procyon, but nothing in reference to our position can be concluded from this.

Let us now briefly consider the manner in which our conclusions are affected by the variations in the intrinsic brightness of the stars. Reverting to equation (2),

$$f l \cdot dl = dl \int_0^{\infty} F(lr^2) \cdot \varphi r \cdot r^4 \cdot dr,$$

let us suppose that  $\varphi r$  is expressed in the form

$$\varphi r = \alpha r^{2a-5} + \beta r^{2b-5} + \gamma r^{2c-5} + \text{etc.},$$

where the number of terms is finite. Substituting in (2) this value of  $\varphi r$ , and also making L the variable instead of  $r$ , we have

$$\begin{aligned} f\ell &= \frac{1}{2} l^{-1} \int_0^\infty F(lr^2)(\alpha r^{2a-1} + \beta r^{2b-1} + \text{etc.}) r^{-1} d(lr^2) \\ &= \frac{1}{2} \alpha \int_0^\infty L^{a-1} \cdot FL \cdot dL \cdot l^{-a} + \frac{1}{2} \beta \int_0^\infty L^{b-1} \cdot FL \cdot dL \cdot l^{-b} + \text{etc.} \end{aligned}$$

If therefore  $f\ell$  be expressed in the form

$$(4) \quad f\ell = Al^{-a} + Bl^{-b} + Cl^{-c} + \text{etc.},$$

where  $a, b, c$  are such values, integral or fractional, as will enable us to express  $f\ell$  with the requisite degree of accuracy in the fewest terms, we find

$$\begin{aligned} (5) \quad \frac{1}{2} \varphi r &= \frac{A}{[L^{a-1}]} r^{2a-5} + \frac{B}{[L^{b-1}]} r^{2b-5} \\ &\quad + \frac{C}{[L^{c-1}]} r^{2c-5} + \text{etc.}, \end{aligned}$$

where  $[L^x]$  denotes the mean value of  $L^x$ .

Although the reasoning by which equation (5) has been obtained is open to some criticism, I think this equation exhibits the manner in which the variation of the intrinsic light of the stars affects inferences regarding their density at different distances. It shows that although any numerical results for the values of the density may require considerable modification, yet the general relations between the inferred densities in different directions will not be completely changed.

In order to gain some idea of the variations in intrinsic light of the stars, I have compared the photometric measures of Seidel with the proper motions of the same stars, as given by Mädler. This astronomer has himself pointed out that the mean distances of the stars of the different magnitudes, as derived from the mean proper motions of each magnitude, have very different ratios from the distances as calculated from the photometric measures. In fact, the mean dis-

tances of the first six magnitudes, as calculated from the proper motions by Mädler, are nearly as the square roots of the distances photometrically deduced.

Mag.	Dist. from proper motions	$\sqrt{\text{Dist. from}}$ the brightness
1	1.00	1.00
2	1.32	1.35
3	1.62	1.62
4	2.00	1.91
5	2.45	2.29
6	2.56	2.88

There are various ways of accounting for this difference; but, however it is to be accounted for, it would seem that if the light of each star is divided by such a power of the proper motion that the ratio tends neither to a continual increase nor a continual decrease with  $l$ , the mean values of this ratio must be independent of the radius vector, and that its variations can depend only on the variations of the intrinsic light, the variations of the real motions of the stars in space, and the errors of observation. In the case of the stars of Seidel, I find that such a ratio exhibits enormous variations, which may be due in large measure to the errors in those proper motions which are very small. But, on the other hand, it is nearly certain that the intrinsic light of the stars does vary enormously. Supposing that it has some maximum value, never surpassed, the mean value of  $L^x$  would on the supposition of enormous variations depend almost entirely on this maximum value; and consequently the effect upon equation (5) would be nearly the same as if all the stars had the same intrinsic light.

On the whole, therefore, I believe that considerable weight should be attached to general conclusions respecting the shapes of the surfaces of equal condensation, calculated on the supposition of equal intrinsic light; but such conclusions are greatly strengthened, in the case of the bright stars, by the circumstance that at a distance of some  $22^\circ$  from the milky way the apparent density of these stars reaches a value which is nowhere much surpassed.

PLATES

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Plate I

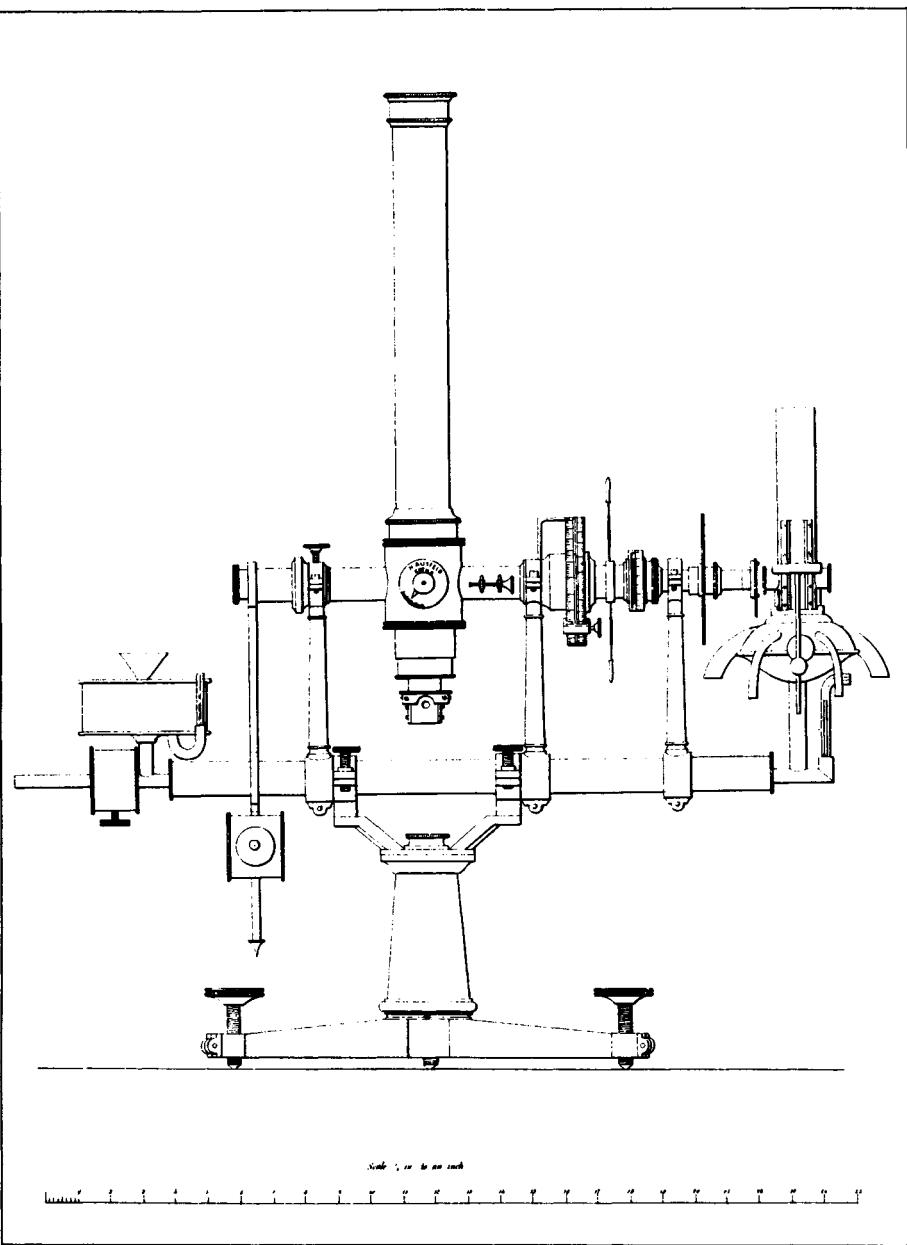
	0	1	2	3	4	5
I	1	12	21	30	39	48
S	2	23	32	41	50	59
U	3	34	43	52	61	70
T (De Nova Tables)	4	45	54	63	72	81
T (Rudolphine Tables)	1	12	23	34	45	56
H	2	23	32	41	50	59
A	3	34	43	52	61	70
S	4	45	54	63	72	81
B	5	55	64	73	82	91
DM	6	66	75	84	93	102
	7	77	86	95	104	113
	8	88	97	106	115	124
	9	99	108	117	126	135
	10	110	119	128	137	146
	11	121	130	139	148	157
	12	132	141	150	159	168
	13	143	152	161	170	179
	14	154	163	172	181	190
	15	165	174	183	192	201
	16	176	185	194	203	212
	17	187	196	205	214	223
	18	198	207	216	225	234
	19	209	218	227	236	245
	20	220	229	238	247	256
	21	231	240	249	258	267
	22	242	251	260	269	278
	23	253	262	271	280	289
	24	264	273	282	291	300
	25	275	284	293	302	311
	26	286	295	304	313	322
	27	297	306	315	324	333
	28	308	317	326	335	344
	29	319	328	337	346	355
	30	330	339	348	357	366
	31	341	350	359	368	377
	32	352	361	370	379	388
	33	363	372	381	390	399
	34	374	383	392	401	410
	35	385	394	403	412	421
	36	396	405	414	423	432
	37	407	416	425	434	443
	38	418	427	436	445	454
	39	429	438	447	456	465
	40	440	449	458	467	476
	41	451	460	469	478	487
	42	462	471	480	489	498
	43	473	482	491	500	509
	44	484	493	502	511	520
	45	495	504	513	522	531
	46	506	515	524	533	542
	47	517	526	535	544	553
	48	528	537	546	555	564
	49	539	548	557	566	575
	50	550	559	568	577	586

Diagram showing the Comparison of the Scales

## of Magnitudes of the different Observers

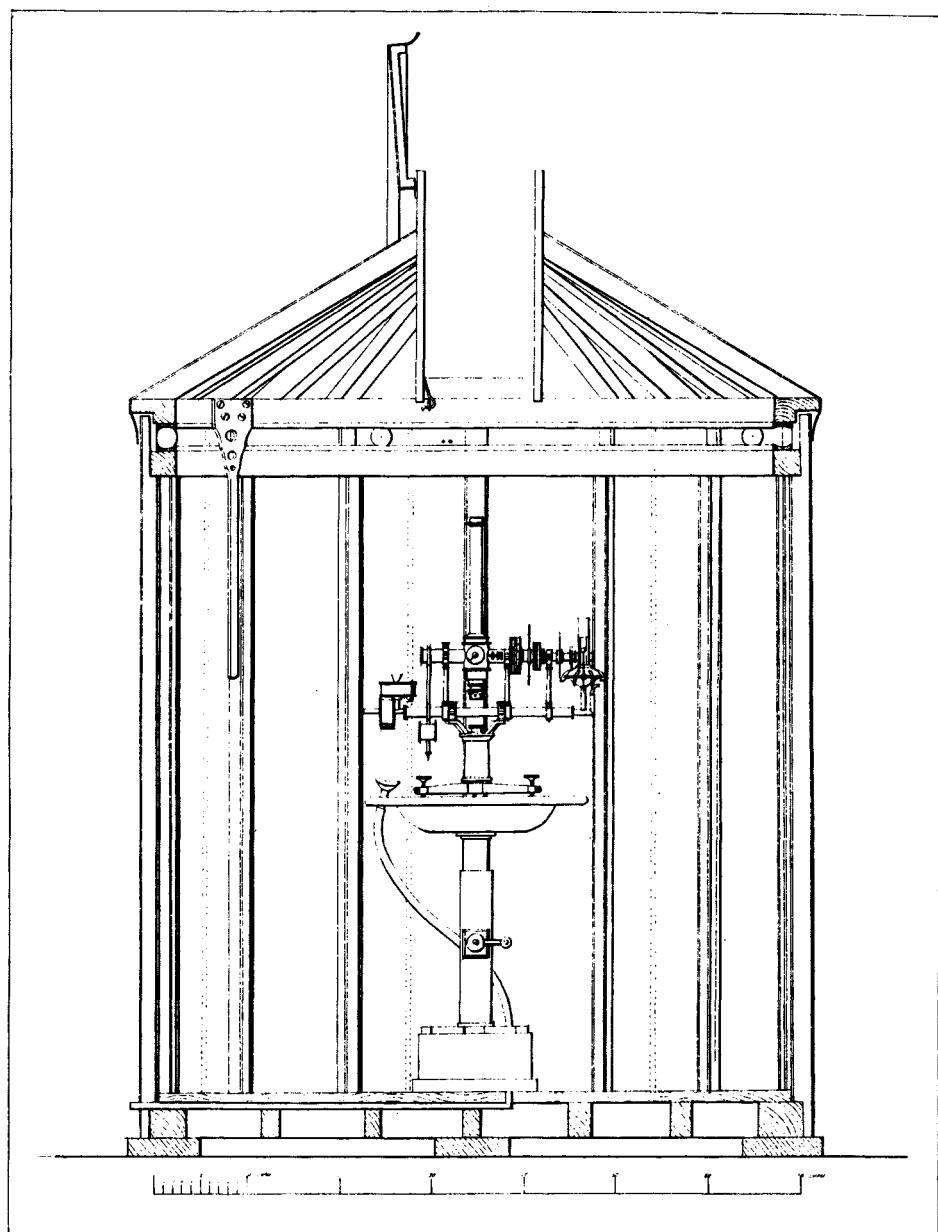
	5	6	7	8	9	10
I	5	5	5	5	5	5
S	5	3	5	5	5	5
U	5	5	5	5	5	5
T (De Novo Stress)	5	5	5	5	5	5
T (Endothelin- 1 Receptor)	5	5	5	5	5	5
H	5	5	5	5	5	5
A	5	5	5	5	5	5
S	5	5	5	5	5	5
B	5	5	5	5	5	5
DM	2	3	3	3	3	3
5	5	5	5	5	5	5
6	6	6	6	6	6	6
7	7	7	7	7	7	7
8	8	8	8	8	8	8
9	9	9	9	9	9	9
10	10	10	10	10	10	10

Plate II



PHOTOMETER

Plate III



PHOTOMETER OBSERVATORY

Plate IV

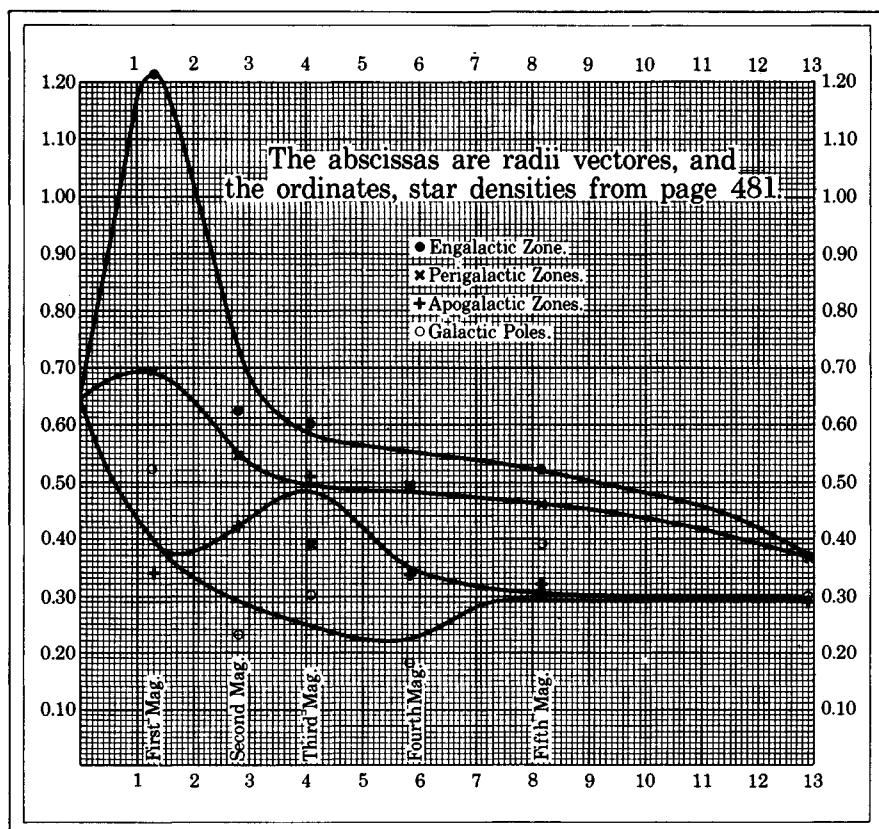
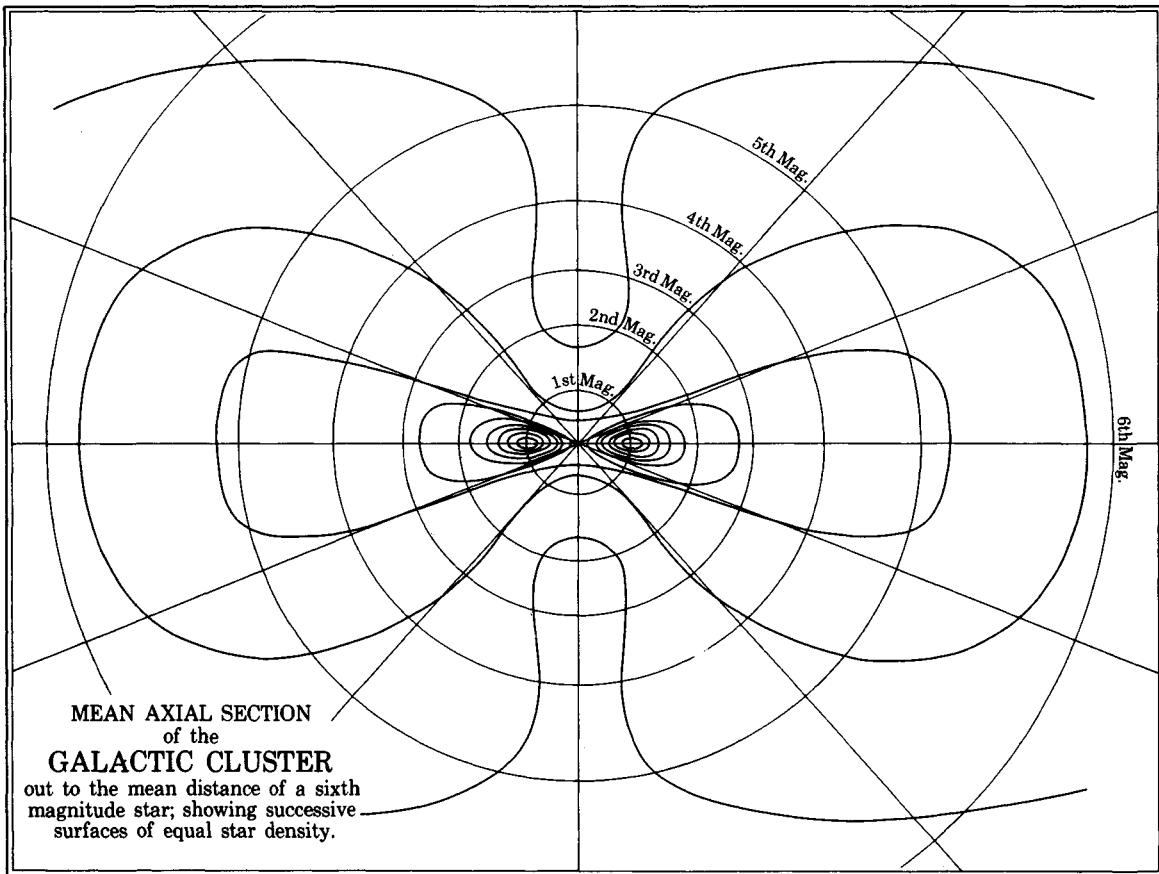


Plate V



## Plate VI

## TABLE OF MAGNITUDES,

where  $m = \log \cdot \sin^2 \varphi / \log 2.25$ ,  $\varphi$  = photometer angle, and 2.25 = ratio of light between successive mags.

m.	φ	m.	φ	m.	φ	m.	φ	m.	φ	m.	φ	m.	φ	m.	φ	m.	φ	m.	φ
0.00	86.3+	0.50	54.5+	1.00	41.7+	1.38	34.9	1.72	29.9	2.13	24.9	2.66	19.9	3.35	14.9	4.34	9.9	6.07	4.9
0.01	83.6+	0.51	54.2+	1.01	41.5+	1.38	34.8	1.72	29.8	2.14	24.8	2.67	19.8	3.37	14.8	4.37	9.8	6.12	4.8
0.02	81.8+	0.52	53.9+	1.02	41.2+	1.39	34.7	1.73	29.7	2.15	24.7	2.68	19.7	3.38	14.7	4.39	9.7	6.17	4.7
0.03	80.3+	0.53	53.6+	1.03	41.0+	1.40	34.6	1.74	29.6	2.16	24.6	2.69	19.6	3.40	14.6	4.42	9.6	6.22	4.6
0.04	79.0+	0.54	53.2+	1.04	40.8+	1.40	34.5	1.75	29.5	2.17	24.5	2.71	19.5	3.42	14.5	4.44	9.5	6.28	4.5
0.05	77.9+	0.55	52.9+	1.05	40.6+	1.41	34.4	1.75	29.4	2.18	24.4	2.72	19.4	3.43	14.4	4.47	9.4	6.33	4.4
0.06	76.9+	0.56	52.6+	1.06	40.4+	1.41	34.3	1.76	29.3	2.19	24.3	2.73	19.3	3.45	14.3	4.50	9.3	6.39	4.3
0.07	75.9+	0.57	52.3+	1.07	40.2+	1.42	34.2	1.77	29.2	2.20	24.2	2.74	19.2	3.47	14.2	4.52	9.2	6.45	4.2
0.08	75.0+	0.58	52.0+	1.08	40.0+	1.43	34.1	1.78	29.1	2.21	24.1	2.76	19.1	3.48	14.1	4.55	9.1	6.51	4.1
0.10	74.1+	0.60	51.7+	1.09	39.9+	1.43	34.0	1.79	29.0	2.22	24.0	2.77	19.0	3.50	14.0	4.58	9.0	6.57	4.0
0.11	73.3+	0.61	51.4+	1.10	39.7+	1.44	33.9	1.79	28.9	2.23	23.9	2.78	18.9	3.52	13.9	4.60	8.9	6.63	3.9
0.12	72.6+	0.62	51.1+	1.11	39.5+	1.45	33.8	1.80	28.8	2.24	23.8	2.79	18.8	3.53	13.8	4.63	8.8	6.69	3.8
0.13	71.9+	0.63	50.9+	1.13	39.3+	1.45	33.7	1.81	28.7	2.25	23.7	2.81	18.7	3.55	13.7	4.66	8.7	6.76	3.7
0.14	71.2+	0.64	50.6+	1.14	39.1+	1.46	33.6	1.82	28.6	2.26	23.6	2.82	18.6	3.57	13.6	4.69	8.6	6.83	3.6
0.15	70.5+	0.65	50.3+	1.15	38.9+	1.47	33.5	1.82	28.5	2.27	23.5	2.83	18.5	3.59	13.5	4.72	8.5	6.90	3.5
0.16	69.9+	0.66	50.0+	1.16	38.7+	1.47	33.4	1.83	28.4	2.28	23.4	2.84	18.4	3.61	13.4	4.74	8.4	6.97	3.4
0.17	69.2+	0.67	49.7+	1.17	38.5+	1.48	33.3	1.84	28.3	2.29	23.3	2.86	18.3	3.62	13.3	4.77	8.3	7.04	3.3
0.18	68.6+	0.68	49.5+	1.18	38.3+	1.49	33.2	1.85	28.2	2.30	23.2	2.87	18.2	3.64	13.2	4.80	8.2	7.12	3.2
0.19	68.0+	0.69	49.2+	1.19	38.2+	1.49	33.1	1.86	28.1	2.31	23.1	2.88	18.1	3.66	13.1	4.83	8.1	7.19	3.1
0.20	67.4+	0.70	48.9+	1.20	38.0+	1.50	33.0	1.86	28.0	2.32	23.0	2.90	18.0	3.68	13.0	4.86	8.0	7.28	3.0
0.21	66.9+	0.71	48.7+	1.21	37.8+	1.51	32.9	1.87	27.9	2.33	22.9	2.91	17.9	3.70	12.9	4.89	7.9	7.36	2.9
0.22	66.4+	0.72	48.4+	1.22	37.6+	1.51	32.8	1.88	27.8	2.34	22.8	2.92	17.8	3.72	12.8	4.93	7.8	7.45	2.8
0.23	65.8+	0.73	48.1+	1.23	37.4+	1.52	32.7	1.89	27.7	2.35	22.7	2.94	17.7	3.74	12.7	4.96	7.7	7.54	2.7
0.24	65.3+	0.74	47.9+	1.24	37.3+	1.53	32.6	1.90	27.6	2.36	22.6	2.95	17.6	3.76	12.6	4.99	7.6	7.63	2.6
0.25	64.8+	0.75	47.6+	1.25	37.1+	1.53	32.5	1.91	27.5	2.37	22.5	2.96	17.5	3.77	12.5	5.02	7.5	7.73	2.5
0.26	64.3+	0.76	47.4+	1.26	36.9+	1.54	32.4	1.91	27.4	2.38	22.4	2.98	17.4	3.79	12.4	5.05	7.4	7.83	2.4
0.27	63.9+	0.77	47.1+	1.27	36.7+	1.55	32.3	1.92	27.3	2.39	22.3	2.99	17.3	3.81	12.3	5.09	7.3	7.93	2.3
0.28	63.4+	0.78	46.9+	1.28	36.6+	1.55	32.2	1.93	27.2	2.40	22.2	3.00	17.2	3.83	12.2	5.12	7.2	8.04	2.2
0.29	62.9+	0.79	46.6+	1.29	36.4+	1.56	32.1	1.94	27.1	2.41	22.1	3.02	17.1	3.85	12.1	5.16	7.1	8.15	2.1

0.30	62.0+	0.80	46.1+	1.30	36.0+	1.57	32.0	1.95	27.0	2.42	22.0	3.03	17.0	3.87	12.0	5.19	7.0	8.28	2.0
0.31	61.6+	0.81	45.9+	1.31	35.9+	1.57	31.9	1.96	26.9	2.43	21.9	3.05	16.9	3.89	11.9	5.23	6.9	8.40	1.9
0.32	61.2+	0.82	45.6+	1.32	35.7+	1.58	31.8	1.96	26.8	2.44	21.8	3.06	16.8	3.91	11.8	5.26	6.8	8.53	1.8
0.33	60.8+	0.83	45.4+	1.33	35.5+	1.59	31.7	1.97	26.7	2.45	21.7	3.08	16.7	3.94	11.7	5.30	6.7	8.67	1.7
0.34	60.3+	0.84	45.2+	1.34	35.4+	1.59	31.6	1.98	26.6	2.46	21.6	3.09	16.6	3.96	11.6	5.34	6.6	8.83	1.6
0.35	59.9+	0.85	44.9+	1.35	35.2+	1.60	31.5	1.99	26.5	2.48	21.5	3.10	16.5	3.98	11.5	5.37	6.5	8.98	1.5
0.36	59.5+	0.86	44.7+	1.36	35.0+	11.61	31.4	2.00	26.4	2.49	21.4	3.12	16.4	4.00	11.4	5.41	6.4	9.15	1.4
0.37	59.1+	0.87	44.7+	1.37	34.9+	1.62	31.3	2.01	26.3	2.50	21.3	3.13	16.3	4.02	11.3	5.45	6.3	9.34	1.3
0.38	58.8+	0.88	44.5+	1.38	34.7+	1.62	31.2	2.02	26.2	2.51	21.2	3.15	16.2	4.04	11.2	5.49	6.2	9.53	1.2
0.39	58.4+	0.89	44.3+	1.39	34.7+	1.63	31.1	2.03	26.1	2.52	21.1	3.16	16.1	4.06	11.1	5.53	6.1	9.75	1.1
0.40	58.0+	0.90	44.0+	1.40	34.6+	1.64	31.0	2.03	26.0	2.53	21.0	3.18	16.0	4.09	11.0	5.57	6.0	9.98	1.0
0.41	57.6+	0.91	43.8+			1.64	30.9	2.04	25.9	2.54	20.9	3.19	15.9	4.11	10.9	5.61	5.9	10.24	0.9
0.42	57.3+	0.92	43.6+			1.65	30.8	2.05	25.8	2.55	20.8	3.21	15.8	4.13	10.8	5.65	5.8	10.53	0.8
0.43	56.9+	0.93	43.4+			1.66	30.7	2.06	25.7	2.56	20.7	3.22	15.7	4.15	10.7	5.70	5.7	10.86	0.7
0.44	56.6+	0.94	43.1+			1.67	30.6	2.07	25.6	2.58	20.6	3.24	15.6	4.18	10.6	5.74	5.6	11.24	0.6
0.45	56.6+	0.95	42.9+			1.67	30.5	2.08	25.5	2.59	20.5	3.25	15.5	4.20	10.5	5.78	5.5	11.69	0.5
0.46	56.2+	0.96	42.7+			1.68	30.4	2.09	25.4	2.60	20.4	3.27	15.4	4.22	10.4	5.83	5.4	12.24	0.4
0.47	55.9+	0.97	42.5+			1.69	30.3	2.10	25.3	2.61	20.3	3.29	15.3	4.25	10.3	5.87	5.3	12.95	0.3
0.48	55.5+	0.98	42.3+			1.69	30.2	2.11	25.2	2.62	20.2	3.30	15.2	4.27	10.2	5.92	5.2	13.95	0.2
0.49	55.2+	0.99	42.1+			1.70	30.1	2.12	25.1	2.63	20.1	3.32	15.1	4.29	10.1	5.97	5.1	15.66	0.1
0.50	54.8+	1.00	41.9+			1.71	30.0	2.12	25.0	2.65	20.0	3.33	15.0	4.32	10.0	6.02	5.0	$\infty$	0.0

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## Editorial Notes

The functions of these notes, which are keyed to page and line numbers, are generally self-explanatory. But three kinds of notes require brief explanation.

The first of these is the initial note to each selection which serves as a headnote and provides a brief physical description of the copy-text, whether manuscript or published article, and, when possible, the reasons for its composition; unless otherwise indicated, all manuscripts are in Peirce's own hand.

The second identifies the names not found in the standard reference works for our edition. These works include the *Dictionary of American Biography*, the *Dictionary of National Biography*, the *Dictionary of Scientific Biography*, the 15th edition of the *Encyclopaedia Britannica*, *The Encyclopedia of Philosophy*, the *National Encyclopedia of American Biography*, and the *New Century Encyclopedia of Names*.

The third kind identifies the source of quotations. Scarcely anything in the present volume that was not published by Peirce was intended for publication in the form in which he left it. He was not consistent in his use of source material. He rarely cited his source fully and often omitted documentation altogether. Sometimes he used quotation marks when he was merely paraphrasing, at other times he omitted them when transcribing verbatim. It was originally our intention to identify only those passages quoted verbatim, but it was soon clear that Peirce paraphrased and interpreted the work of others extensively and that knowing the sources for these materials would be of help to the reader. Consequently, we now identify most paraphrases as well.

Material in quotation marks that was found to be verbatim copy, or very close paraphrase of a specific passage, is identified. When Peirce put in quotation marks what is not a quotation but his own

interpretation of the author referred to or when without using quotation marks he offered a summary statement of the general view of an author on a certain point, we provide a reference to the passage in which the author comes nearest to saying what Peirce reported. In the case of a rhetorical flourish in quotation marks we of course supply no note. Unless otherwise indicated, all translations in the editorial notes are by the editors.

Every effort is made to cite the editions that Peirce owned or that we know he used. When we cannot provide such information or when the edition he used was not available to us, we cite one that was accessible to Peirce. In some cases we add in brackets a more general reference as to book or chapter number or a reference to an edition more readily available to the modern reader. References to the first two volumes of the present edition appear as *W1* or *W2*, followed by a colon and page number.

Citations are generally given in shortened form; full bibliographic information is provided in the bibliography that follows the editorial notes. Works we know Peirce owned are identified in the bibliography by the degree symbol (°).

### *Educational Text-Books*

Haskell's *Index to the Nation* leaves this review essay unassigned. But a letter dated 9 May 1872 and addressed to E. L. Godkin, editor of the *Nation*, shows that Peirce is author of the notice on Wilson; he probably wrote the remainder of the essay as well. He also may have written the brief part on Fowler that appears in "Educational Text-Books I," published in the *Nation* a week earlier, though Haskell's *Index* assigns that review essay to William Francis Allen. (For the brief part on Fowler, a correction to the Wilson notice, and Peirce's letter, see the notes below.)

- 1.4 Proctor's] An English mathematician and astronomer, Richard Anthony Proctor (1837-1888) proposed in 1873 that lunar craters were caused by meteoric bombardment, a theory still held today. He lived in America from 1881 until his death.
- 1.5-8 In a . . .] See his *Handbook of the Stars*.
- 1.25-27 "I believe . . . ,"] Proctor, *Star Atlas*, p. 14.
- 2.15 Elijah Burritt's] Brother of the more famous Elihu Burritt, Elijah Hinsdale Burritt (1794-1838) wrote *The Geography of the Heavens* (revised edition by Hiram Mattison [New York: F. J. Huntington, 1853]), which went through several editions.

- 2.19–20 Tyndall's] The reference is to John Tyndall's *Heat, a Mode of Motion*, 3rd edition (London: Longmans, Green, and Co., 1868).
- 2.34–3.6 But . . .] A correction to this notice on Wilson appeared on p. 274 of the 25 April 1872 *Nation*:

Dr. W. D. Wilson, author of *Lectures on the Psychology of Thought and Action*, reviewed in the *Nation* of April 11, has pointed out to us that we misrepresented him in saying that he thinks instinct and clairvoyance to be due to a direct action of the spinal cord. On the contrary, he thinks them due to the possession of 'senses' other than the familiar five. We much regret the error, though we think the opinion we attributed to Dr. Wilson is not more at variance with first principles than that which he really holds.

And concerning the same notice, Peirce wrote to E. L. Godkin on 9 May 1872:

I suppose you know that I fell into an error in my notice of Dr. W. D. Wilson's psychology which required a correction in a later number. I have felt that I owed you an explanation of the matter and should have written before but have not had time. Dr. Wilson seems to think that the mistake was a very extraordinary one & I can easily perceive that, being capable of holding the doctrine he does, he should think that he had expressed himself with perfect clearness. What he thinks is that instinct in animals as well as clairvoyance etc. may be due to the possession of a *sense* additional to the familiar five. It never occurred to me that a man could seriously and deliberately think that. To suppose for example that Swedenborg perceived that fire in Stockholm when he was in Berlin—I think that was the place—by means of a physical effect upon any organ of sense not discovered by anatomy, that so marked an effect could be produced upon this organ by objects at that distance that he could say that it was caused by a fire, by a fire not in a forest but a city, that the city was not Copenhagen or Königsberg but Stockholm, that he should know further the street and the house, in short that he should have a sense—a common non-supernatural sense—immeasurably more minute than the eye, without the organ of it being prominent or yet detected by naturalists, this I say appeared to me so absurd a thing that it never occurred to me that Dr. Wilson could intend it. So, though he did express himself pretty plainly, I twisted his words to another interpretation and supposed that by a sense he meant an intuitive faculty merely, as when we speak of the inward sense. This was a construction his words would bear & was the only one I thought of. Yours very truly  
C S Peirce (L 248)

- 3.7–11 We said. . .] See p. 222 of the 4 April 1872 *Nation*:

The best logic for instruction in colleges is, in our judgment, Fowler's (*Elements of Deductive Logic*—New York: Macmillan). A young man who has been through it under a teacher of power will have had his mind enlightened and strengthened, and will be better prepared for life. In short, it to some extent fulfils the function of an elementary logic, a thing which most text-books do not begin to do. Mr. Fowler closely follows Mill's work, of which this must be allowed, that it represents the best scientific thought of the age more nearly than any other systematical exposition of the subject. It contains, however, in

our opinion various important errors not only upon its philosophical side, but also in its relation to practice, against which the student ought to be put upon his guard. To these we have not space here to refer; but as they are of interest we shall take an early opportunity to recur to them.

- 3.34 “extralogical”] Peirce gives the following definition in the *Century Dictionary*: “Lying out of or beyond the province of logic, when this is conceived to be restricted to syllogistic and subsidiary doctrines, and to have no further concern with the truth or falsity of reasonings.”
- 4.11 Day’s logics] The reference is to Henry Noble Day’s *Elements of Logic* (New York: C. Scribner and Company, 1867) and *Logical Praxis* (New Haven, CT: Chatfield, 1872 [c1871]).
- 4.39-5.3 A scientific. . . .] See Mill, *System of Logic*, 2:8-9.
- 5.7-9 “that it. . . .”] Ibid., 2:14.
- 7.14-16 “Eine. . . .”] Ueberweg, *System der Logik*, p. 307 (where “gerade” is “grade”).
- 7.19-22 “Dr. Ueberweg. . . .”] Ueberweg, *System of Logic*, p. viii.
- 7.25-27 “contains. . . .”] Larkin, *Rival Collection*, Preface.

### *Lecture on Practical Logic*

This untitled lecture on reality and the settlement of opinion, written very probably in the summer or fall of 1872, consists of three sheets of medium-light, laid, and lined white paper measuring  $7 \frac{3}{4} \times 9 \frac{3}{4}$  in., inscribed in black ink on rectos only. There are numerous corrections and revisions in the two and one-third pages of text. The first two pages are marked “Pract. Log.” in the upper right-hand corner, the third page “Lect. Logic”; the page numbers are not Peirce’s. It is not known what occasioned the lecture or whether it was given.

### *Third Lecture*

Written very probably in the summer or fall of 1872, this brief lecture on thought consists of two sheets of medium-light, laid, and lined white paper measuring  $7 \frac{3}{4} \times 9 \frac{3}{4}$  in., inscribed in black ink on rectos only. Very carefully written, the two pages contain only three minor revisions. As with the preceding item, it is not known whether the lecture was given or what occasioned it.

### *Logic, Truth, and Settlement of Opinion*

In a letter dated 20 April 1872, Peirce tells his mother from Washington, DC, where he is Acting Assistant in Charge of the Office

of the Coast Survey, that "On clear nights I observe with the photometer; on cloudy nights I write my book on logic which the world has been so long & so anxiously expecting." More than 110 years later, the long and anxious waiting period may be over at last. The following thirty-six items—as well as ten others not published in the present volume—all belong to this "book on logic." Written sometime between February or March 1872 and July 1873, with the dated chapters composed between 6 March and 2 July of the latter year, they used to be known as "The Logic of 1873" but are now more properly called "Toward a Logic Book, 1872–73." Eighteen of the forty-six manuscripts were written by two amanuenses, one an ordinary hand here called *amanuensis A* (seven manuscripts) and the other an ornate and flourished hand here called *amanuensis B* (eleven manuscripts); the identity of the amanuenses has not been determined, though they were probably Peirce's aides in the Coast Survey.

"Toward a Logic Book, 1872–73" is a fragmentary and heterogeneous collection of manuscripts; it lacks a definitive plan for the organization of the numerous chapters and almost certainly represents two or three distinctive projects for a book on logic. But fragmentary and unfinished though it may be, the 1872–73 collection is nonetheless interesting in that, by pursuing the shifting arrangements of topics, we are able to glimpse the development of Peirce's conception of logic. The most marked shifts seem to occur at the end of the first half of 1872, in March of 1873, and at the beginning of the summer of the same year. According to an early outline, the following six topics were to be treated: doubt and belief, the ways of settling belief, reality, the categories, signs, and inference in general. The first three chapters—on doubt and belief, on inquiry, and on the four methods of settling opinion—were written during the first six months of 1872, and the fourth on reality shortly thereafter. In March of 1873, the fourth chapter was to deal with the continuity of thought, and the chapter on reality was to come after some intervening chapters following the treatment of the methods of settling opinion. According to a later outline, which omits representations as well as categories, there were to be seventeen chapters. And according to yet another outline, the fifth chapter was to deal with the elements of cognitions in terms of beliefs, the sixth with reality, and the seventh with logic as a study of signs; then follow several continuous chapters of a more technical nature. But on 15 March, Peirce departs from that outline by writing an untitled ninth chapter on the quasi-formal analysis of what is asserted or denied by propositions; accord-

ing to the outline, chapter 9 was to deal with space as being essential in logic. And finally, it is not at all clear where exactly the several chapters on signs or representations, as well as the chapters on time and on thought in reference to the future, fit into the various outlines. Lacking a definitive plan for the arrangement of all the manuscripts belonging to "Toward a Logic Book, 1872-73," we present them in chronological order (as that order could best be determined). A few of the following items have appeared in volume 7 of the *Collected Papers*, but they represent only a very small part of the forty-six manuscripts that can be identified with "Toward a Logic Book."

The first item included here is an untitled manuscript consisting of (1) four and one-half consecutive pages that are published here and (2) three one-page earlier and unfinished versions of the first page as well as an additional page that may represent yet another version of the beginning of this manuscript. The first five sheets are of heavy, wove, white paper, the other four of medium-light, laid, and lined white paper; all measure  $7 \frac{3}{4} \times 9 \frac{3}{4}$  in. and are inscribed in black ink on rectos only. It is possible that the four and one-half consecutive pages were once part of a larger manuscript, although page numbers 5 through 9 in the upper right-hand corners are not Peirce's.

16.20-23 William Cullen Bryant, "The Battle-Field," stanza 9. Peirce mistakenly attributed the lines to Alexander Pope and obviously quoted from memory; properly, the lines read:

Truth crushed to earth shall rise again;  
The eternal years of God are hers;  
But Error wounded, writhes with pain,  
And dies among his worshippers.

The first line reappears in item 61 below, at 274.16.

### *Investigation and Settlement of Opinion*

This obviously unfinished manuscript consists of eight sheets of heavy, wove, white paper measuring  $7 \frac{3}{4} \times 9 \frac{3}{4}$  in., inscribed in black ink on rectos only. They are arranged as (1) four and one-half pages that are published here, (2) an unfinished page of an earlier version of the second page that follows from page 1, and (3) two separate sheets, each with nearly half a page of text, that represent

earlier versions of parts of the published text. It is possible that these pages once belonged to some other manuscript and that the beginning of the published text is not the beginning of the original manuscript. Though generally well written, the manuscript contains numerous corrections and revisions, and two paragraphs have been deleted and then rewritten. Page numbers 1 through 4 (and 2 on the discarded second page) in the upper right-hand corners are not Peirce's.

- 17.9 tends] Here ends the first page of the manuscript. The second, discarded page reads as follows:

to develope a particular body of doctrine in each community, but also by this very development to make the ideas of different communities diverge more and more as long as they are removed from mutual influence, and even in the same community gives rise to all sorts of sporting of opinion. This it is easy to see must be the case, because this is the effect of all growth. Historically it is impossible to find a case in which this method has been exclusively pursued, but the different customs and ideas of morality of different nations and all those facts upon which Montaigne loves so much to dwell strongly confirm this judgment of the results of this method.

### *Chapter 1 (MS 181)*

This three-page manuscript, which turns into a mere outline toward the end, consists of three sheets of medium-light, laid, and lined white paper measuring  $7 \frac{3}{4} \times 9 \frac{3}{4}$  in., with a “Congress/P&P” embossing in the upper left-hand corner, and inscribed in black ink on rectos only. It is generally well written but contains a number of revisions and interlinear additions. Peirce's page numbers appear in the upper right-hand corners. The first chapter of this manuscript is an earlier version of item 11 below (pp. 24–28), which in turn is an earlier version of section V in item 60 (pp. 248–57).

- 18.21 [Reading *Advertiser*]] Peirce's reminder to himself to give the example of “a certain newspaper . . . [he was] entreated not to read”; see item 11 below (24.16–20) as well as section V of item 60 (249.4–12). The certain newspaper is probably the *Advertiser's Gazette*, which was published in Boston and New York between November 1866 and April 1876.

- 18.22 [Ostrich]] Peirce's reminder to himself to give the example of the ostrich that “buries its head in the sand as danger approaches”; see 25.4–8 and 249.34–37 below.

*Chapter 1 (MS 182)*

This manuscript, which seems to have been written shortly before the very next item, consists of two sheets of medium-light, laid, and lined white paper measuring  $7 \frac{3}{4} \times 9 \frac{3}{4}$  in., with a “Congress/P&P” embossing in the upper left-hand corner, and inscribed in black ink on rectos only. Its one and one-fourth pages are generally well written and contain only a few minor and immediate revisions. It is noteworthy that the first instance of “belief” replaces the crossed-out “a judgment.”

*Chapter 1 (MS 183)*

This manuscript was probably written the same day as the immediately preceding item. It consists of one sheet of medium-light, laid, and lined white paper measuring  $7 \frac{3}{4} \times 9 \frac{3}{4}$  in., with a “Congress/P&P” embossing in the upper left-hand corner. Aside from a few minor revisions, the single page of text is carefully inscribed.

*Chapter 1. Doubt and Belief*

This flawlessly written manuscript is taken from L 248, the 249-page Coast Survey letterpress copybook that Peirce used between 4 May 1872 and 10 September 1873 and that also contains items 40, 41, and 43 in the present volume. It is bound in blackish cloth covers (with the front cover now missing), has remnants of a red leather spine, and measures  $8 \frac{5}{8} \times 11 \frac{3}{8}$  in.; the 249 sheets, inscribed on rectos only, are of exceedingly light, wove onion-skin paper, and page numbers are printed in the upper right-hand corners of the rectos. Written between 11 and 14 May 1872, the dates of the two letters surrounding it, the present manuscript appears on pages 10-12 of the copybook and is two and one-third pages long. “Chapter 1” is preceded by “Logic” in the main title. For a slightly different version of the present manuscript, see section III in item 60 below, p. 247. (Was this part of the “admirable chapter” that Peirce read before the Metaphysical Club in Cambridge?)

22.10-13 The assassins . . .] A Syrian religious and military order founded in 1090 in Persia and finally subdued in Lebanon in 1272. The Holy Spirit was said to reside in Sheik al-Jebal (Old Man of the Mountain),

whose will was followed in blind obedience. Notorious for their widespread acts of terror, the Assassins numbered over 50,000 during the time of the Crusades.

### *Chapter 2. Of Inquiry*

This manuscript consists of three sheets of light, laid, and unlined paper measuring  $7 \frac{3}{4} \times 9 \frac{3}{4}$  in., watermarked "J. Whatman/1868" and inscribed in black ink on rectos only. It contains a few minor revisions but is otherwise carefully written and, except for the missing second page, it is complete. "Chapter 2" is preceded by "Logic" in the main title. For a slightly different version of the present manuscript, see section IV in "The Fixation of Belief" below, item 60, pp. 247–48.

23.16 [ . . . ] For the missing text, or something close to it, see 248.4–16 below.

### *Chapter 3. Four Methods of Settling Opinion*

This manuscript consists of twelve sheets of light, laid, and unlined paper measuring  $7 \frac{3}{4} \times 9 \frac{3}{4}$  in., watermarked "J. Whatman/1868" and inscribed in black ink on rectos only. At least eight sheets or pages are missing, namely pages 2, 10–15, and 20. Though it is generally well written, it contains a number of deletions and revisions in black ink, lead pencil, and blue pencil, all in Peirce's hand. "Chapter 3" is preceded by "Logic" in the main title, and "Opinion" in the subtitle replaces the crossed-out "Belief." For a somewhat different and more complete version of the present manuscript (which version contains all the lead pencil changes), see section V in "The Fixation of Belief," item 60 below (pp. 248–57).

24.16–20 I remember . . . special] See 18.21 and 249.4–12.

24.20 [ . . . ] For the missing text, or something close to it, see 249.8–21.

25.4–8 When . . . ?] See 18.22 and 249.34–37.

26.10 separate] In the manuscript, the misspelled word *segragate* is written in pencil above the word *separate*, though it is not in Peirce's hand. The properly spelled *segregate*, however, appears in the later printed version; see 250.38.

26.15–16 from the days. . . . ] See editorial note 251.3–4 below.

27.8 [ . . . ] For the missing text, or something close to it, see 251.36–253.39.

28.26 [ . . . ] For the remaining text of this manuscript, or something

resembling it, see the last three paragraphs of section V in item 60 below, 255.25-257.23.

### *On Reality (MS 194)*

Written by amanuensis B, this untitled manuscript consists of eleven sheets of light, laid, and unlined paper measuring  $7 \frac{3}{4} \times 9 \frac{3}{4}$  in., watermarked "J. Whatman/1868" and inscribed in black ink on rectos only. There are neither corrections nor revisions. At the top of the first page, Knight McMahan (an early cataloguer of the Peirce Papers) has written, "Ch. IV 'On Reality'" followed by his initials; to the right, Arthur Burks has written "Logic 1873" as well as the box number that once contained this particular manuscript. (Peirce manuscripts used to be identified by the number of the box containing them.) Page numbers in the upper right-hand corners are by McMahan, those in the upper left-hand corners by the amanuensis. This and the following eight items should be read together, as all nine represent versions of the chapter on reality.

29.19-23 It is. . . . ] See Locke's *Essay*, bk. 2, chap. 1, or Hume's *Enquiry*, sect. 2, or *Treatise*, bk. 1, pt. 1.

### *Chapt. 4 (MS 195)*

Written by amanuensis B, this manuscript on thought and external reality consists of seven sheets of light, laid, and unlined paper measuring  $7 \frac{3}{4} \times 9 \frac{3}{4}$  in., watermarked "J. Whatman/1868" and inscribed in black ink on rectos only. Page numbers for pages 3, 5, and 7 appear in the upper left-hand corners. There are a few pencil corrections and revisions in Peirce's hand.

### *Chap. 4 (MS 196)*

Written by amanuensis B, this manuscript on the fourth method of inquiry consists of six sheets of light, laid, and unlined paper measuring  $7 \frac{3}{4} \times 9 \frac{3}{4}$  in., watermarked "J. Whatman/1868" and inscribed in black ink on rectos only. Pages are numbered in the upper right-hand corners. There are a few pencil corrections and revisions in Peirce's hand, and "*Logic*" precedes "Chap. 4" in the title. In the upper right-hand corner of the first page, Arthur Burks has written

“Logic, 1873” as well as the box number that once contained this particular manuscript.

### *On Reality (MS 197)*

This brief manuscript on nothing, absurdity, and unreality consists of one sheet of medium-heavy, laid, and unlined white paper measuring  $7 \frac{3}{4} \times 9 \frac{3}{4}$  in., inscribed in black ink on the recto only. Containing several deletions and immediate revisions, the page seems to have been written rather hurriedly.

### *On Reality (MS 198)*

This manuscript on reality and on meaning and feeling consists of five sheets of medium-heavy, laid, and unlined white paper measuring  $7 \frac{3}{4} \times 9 \frac{3}{4}$  in., inscribed in black ink on rectos only. The five pages of text were probably written the same day as item 15; they contain similar deletions and immediate revisions as well as a few additional corrections. Pages 2 through 4 are numbered in the upper right-hand corner by a later editor. The fifth page has been recovered from a fragment folder.

### *Chap. 4. Of Reality*

Written by amanuensis B, this manuscript consists of twenty-three sheets of light, laid, and unlined paper measuring  $7 \frac{3}{4} \times 9 \frac{3}{4}$  in., watermarked “J. Whatman/1868” and inscribed in black ink on rectos only. Pages are numbered in the upper left-hand corners; page numbers on the right are by Arthur Burks who has also written, in the upper right-hand corner of the first page, “Logic 1873” and the box number that once contained this particular manuscript. The main title of the manuscript is “Logic” underlined three times; written to the right in pencil, perhaps by Peirce, is “(1st draft).” There are numerous corrections and revisions, some by Peirce and others by the amanuensis, on the first seventeen pages; on the last six there are neither revisions nor other markings.

41.6–7 no man . . . ,] Hobbes, *Leviathan*, p. 85 [chap. 11]. See also the footnote in W2:261.

41.14 modern necessitarians] Peirce is no doubt thinking of Mill; see the footnote in W2:261.

*Of Reality*

This manuscript on investigation and the final opinion consists of twenty sheets of light, laid, and unlined paper measuring  $7 \frac{3}{4} \times 9 \frac{3}{4}$  in., watermarked "J. Whatman/1868" and inscribed in black ink on rectos only; a single sentence on the second verso replaces one crossed out on the preceding recto. There are numerous deletions and revisions, especially in the first half of the manuscript. In the upper right-hand corners of the second and eighth sheets, Arthur Burks has written "1873 Logic" as well as the box number that once contained this particular manuscript. Page numbers 1 through 20 are not Peirce's.

50.11-12 To know. . . ,] Hamilton, *Discussions*, p. 53.

50.12-13 The . . . ,] Kant, *Werke*, 2:732 [*Critique*, B132].

*Chapter IV. Of Reality (MS 204)*

This manuscript consists of (1) fourteen numbered pages of consecutive text that are published here and (2) three unnumbered and discarded pages of an earlier version of pages 5 through 7 that follow from page 4. All seventeen pages are inscribed in black ink on seventeen sheets of light, laid, and unlined paper measuring  $7 \frac{3}{4} \times 9 \frac{3}{4}$  in., watermarked "J. Whatman/1868." It is likely that the original seven pages once represented a complete version. But Peirce then rewrote the three pages and added seven more. Though there are a number of deletions, revisions, and corrections, the manuscript is on the whole carefully written. Page numbers in the upper right-hand corners are not Peirce's.

54.27-29 Yet. . . ,] In the right-hand margin of this sentence in the manuscript, there is a brace and to the right of that, written sideways, is the word "Obscure." Neither is in Peirce's hand.

55.14 Bradley] James Bradley (1693-1762), English astronomer.

55.15 Fizeau] Armand Hippolyte Louis Fizeau (1819-1896), French physicist.

56.8ff. ultimate . . . ,] Here begins the rewritten fifth page. The discarded three pages read as follows:

true opinion to which investigation will finally lead us. This is the realistic view of reality.

To reconcile these two theories it may be supposed that entirely indepen-

dent of all thought there exist such things as we shall think in the final opinion; that these things affect our senses and that the nature of the mind is such that these sensations will ultimately lead us to the true opinion.

This I take to be the metaphysics most commonly adopted. But the idealists have brought against it a fatal objection. To say that a conception exists means obviously that it may be thought. The object of the final opinion exists in that sense. But there is no meaning in your words if you say that this object exists altogether independently of being thought. To obviate this objection a meaning must be assigned to this expression. What must be said, is that that which will be thought in the final opinion exists now in this sense that the influence of observation on the mind is such that we shall ultimately be led to that opinion.

The conception that a real existence at this present moment consists entirely in the future occurrence of an event, is nothing new or strange. The most uncultured mind possesses it. We hold for instance that a diamond is hard and that its hardness is a real quality of it existing all the time; and yet nobody thinks that this hardness consists in anything but the fact that when we come to try we find we can't scratch it. We do not think it exists in any other sense. So we say that a fixed force exists, say gravitation, for example. But we do not imagine that the existence of force (or what mathematicians call *potential energy*) consists in anything but the certainty that bodies in certain situations will be sure to experience certain accelerations of velocity. This is another example of the conception of a real existence which consists only in the fact that certain phenomena will be manifested. Boscovich held (and many physicists hold with him) that matter exists only in the sense that there are certain centres of force, that is only in that certain phenomena will be manifested. I extend this conception to realities generally and say that as our thoughts are the only things which appear to us, the only way in which anything exists is by exerting an influence on thoughts, and to say that this influence exists is only to say that as opinions, through observation and reasoning change and develop, they tend at last to certain determinate results.

#### *Chapter IV. Of Reality (MS 205)*

This carefully written manuscript, with only a few very minor revisions, consists of three sheets of light, laid, and unlined paper measuring  $7 \frac{3}{4} \times 9 \frac{3}{4}$  in., watermarked "J. Whatman/1868" and inscribed in black ink on rectos only. The three pages (the third recovered from a fragment folder) seem to be the last member of the series begun above in items 9–11. Page numbers 1 and 2 are not Peirce's.

#### *The list of Categories*

This manuscript, which ends when Peirce reaches the first of the three categories, consists of two sheets of light, laid, and unlined paper measuring  $7 \frac{3}{4} \times 9 \frac{3}{4}$  in., watermarked "J. Whatman/1868" and inscribed in black ink on rectos only. Though there are several

deletions, insertions, and other revisions, the manuscript is on the whole carefully written. At the end of the second paragraph, Peirce has crossed out the following sentence: “A griffin, however, possesses only a partial being because it is not completely definite, as no object of actual thought is completely.”

*On Representations (MS 212)*

Written by amanuensis A, this manuscript consists of ten sheets of medium-light, laid, and lined white paper measuring  $7 \frac{3}{4} \times 9 \frac{3}{4}$  in., with an “E H Owen” embossing in the upper left-hand corner, and inscribed in black ink on the ten rectos and two of the versos. The ten unnumbered rectos of consecutive text are published here; the versos of the third and fourth pages, which contain text that alternates between shorthand and longhand, represent an earlier beginning of the manuscript but are entirely crossed out. The consecutive text contains numerous abbreviations, which have been emended in our text, and the first five pages are heavily revised and corrected by Peirce—because his amanuensis seems to have had a difficult time understanding what was dictated.

*On Representations (MS 213)*

This manuscript, which defines representation as “an object which stands for another, so that an experience of the former affords us a knowledge of the latter,” consists of three sheets of light, laid, and unlined paper measuring  $7 \frac{3}{4} \times 9 \frac{3}{4}$  in., watermarked “J. Whatman/1868” and inscribed in black ink on rectos only. Aside from a few immediate revisions, it is well written. Unfortunately, the manuscript breaks off just as Peirce is ready to begin discussing the third property of representations.

*On the nature of signs*

Written by amanuensis A, this manuscript on the three properties of signs, earlier called representations, consists of six sheets of medium-light, laid, and lined white paper measuring  $7 \frac{3}{4} \times 9 \frac{3}{4}$  in., with an “E H Owen” embossing in the upper left-hand corner, and inscribed in black ink on rectos only. There are only a few very minor

revisions in these five and one-third pages of text. Page numbers in the upper right-hand corners are almost certainly by a later editor.

### *Time and Thought (MS 215)*

Written by amanuensis B, this untitled manuscript consists of nine sheets (the last folded) of light, laid, and unlined paper measuring  $7\frac{3}{4} \times 9\frac{3}{4}$  in., watermarked "J. Whatman/1868" and inscribed in black ink on rectos only (only the first recto of the folded sheet). There are a few pencil corrections and revisions by Peirce. At the top of the first page, Knight McMahan (the early cataloguer of the Peirce Papers) has written, "Temporal succession of/ideas as continuous/43," followed by his initials. Page numbers in the upper right-hand corners are probably also McMahan's.

### *Time and Thought (MS 216)*

Written by amanuensis B, this untitled manuscript seems to be a new version of the immediately preceding item. It consists of nine sheets of light, laid, and unlined paper measuring  $7\frac{3}{4} \times 9\frac{3}{4}$  in., watermarked "J. Whatman/1868" and inscribed in black ink on rectos only. The eight and one-half pages of text contain two immediate revisions by the amanuensis; Peirce may have crossed out one "the." At the top of the first page, Knight McMahan has written, "Continuity &/mental life/45," followed by his initials. Pages 3 through 5 are numbered in the upper left-hand corner by the amanuensis; the other page numbers are by a later editor, probably McMahan.

### *Chap. 5th*

Written by amanuensis B, this manuscript consists of seven sheets of light, laid, and unlined paper measuring  $7\frac{3}{4} \times 9\frac{3}{4}$  in., watermarked "J. Whatman/1868" and inscribed in black ink on rectos only. The main title of the manuscript is "*Logic*," followed by the date; "Chap. 5th," which appears at the top right of the first page, seems to have been added later. All seven pages, which liken the continuous process of thinking to the continuous process of semiosis, are carefully inscribed, though the first two contain several ink revisions by Peirce. Only the third page is numbered by the amanuensis in the upper left-hand corner.

*Chap. 6th*

Written by amanuensis B, this manuscript consists of ten sheets of light, laid, and unlined paper measuring  $7 \frac{3}{4} \times 9 \frac{3}{4}$  in., watermarked "J. Whatman/1868" and inscribed in black ink on rectos only. Pages are carefully inscribed, with only a few pencil revisions by Peirce. As with the preceding item, the main title is "*Logic*," followed by the date; "Chap. 6th" appears above the main title. MS 219, which is not published here, is an eight-page copy of the present item by amanuensis A.

*Memorandum*

This one-page list of what seem to be the last eleven chapters of a book on logic was written by amanuensis B, probably between 11 and 14 March 1873; it consists of one sheet of medium-heavy, laid, and lined white paper measuring  $7 \frac{3}{4} \times 9 \frac{3}{4}$  in., with an "E H Owen" embossing in the upper left-hand corner, and inscribed in black ink. Some of the items following in the present volume obviously fit into the list; some other chapters have not been found and may never have been written.

*Chap. 7. Logic as a Study of Signs*

Written by amanuensis B, this manuscript consists of two sheets, originally one folded sheet, of medium-light, laid, and lined white paper measuring  $7 \frac{3}{4} \times 9 \frac{3}{4}$  in., with an "E H Owen" embossing in the upper left-hand corner, and inscribed in black ink on both rectos and versos. There are several pencil revisions by Peirce, primarily the insertion of left-out words, as well as several inserted paragraph signs. The heading "Chap. 7." is preceded by "*Logic*"; the date is in the upper right-hand corner.

84.2-5 certain logicians. . . . ] The reference is almost certainly to W. Stanley Jevons and especially his *Pure Logic*, pp. 75ff. [chap. 15]. See also W2:445-47.

*Chap. 9th*

Written by amanuensis B, this manuscript on the principles of formal logic consists of three sheets of medium-light, laid, and lined

white paper measuring  $7 \frac{3}{4} \times 9 \frac{3}{4}$  in., with an "E H Owen" embossing in the upper left-hand corner, and inscribed in black ink on all rectos and versos. The eleven and one-half pages of text are very carefully written. The main title of the manuscript is "*Logic*"; the date appears in the upper left-hand corner.

### *Chap. VIII. The Copula*

This manuscript on the various properties of the copula consists of five sheets of medium-light, laid, and lined white paper measuring  $7 \frac{3}{4} \times 9 \frac{3}{4}$  in., with an "E H Owen" embossing in the upper left-hand corner, and inscribed in black ink on rectos only. There are numerous deletions and insertions, as well as corrections and revisions, and page numbers 1 through 3 are not Peirce's. It is not clear whether the five pages represent Peirce's complete manuscript. A much shorter and rougher version of the present manuscript is contained in MS 228.

### *Chap. IX. Relative Terms*

This obviously incomplete manuscript consists of four sheets of medium-light, laid, and lined white paper measuring  $7 \frac{3}{4} \times 9 \frac{3}{4}$  in., with an "E H Owen" embossing in the upper left-hand corner, and inscribed in black ink on rectos only. As in the preceding item, there are numerous deletions and insertions as well as revisions and corrections. There are two additional but separate pages on the same kind of paper, which are not published in the present volume; the first, which may be an untitled earlier version of the first page, reads as follows:

A very important class of relatives are those every individual case of which is of the form A:A. Such are "man that is \_\_\_\_", "animal that is \_\_\_\_", etc. It will occasion no ambiguity to denote such relatives by the same letters which denote the absolute terms (man, animal, etc) contained in their definitions.

Such relatives may be termed scalars. Their properties are three 1st That ab — a, a coward that is a soldier is a soldier 2nd That ab — b 3rd That is a — b and a — c then a — bc. "If John is a coward and John is a soldier then John is a coward that is a soldier."

A noticeable corollary is that a — aa.

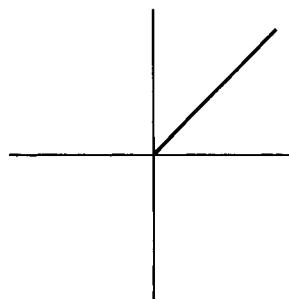
Another is that ab — ba.

It is to be remarked that in regard to relatives in general we cannot write  $l - 1$ . We can only do so in the case of scalars.

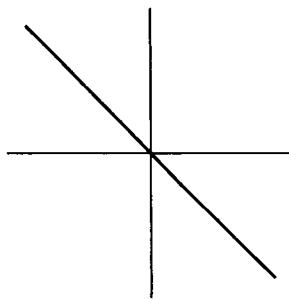
But in general it is easy to prove that [ . . . ]

The second page, which may not belong here, reads as follows:

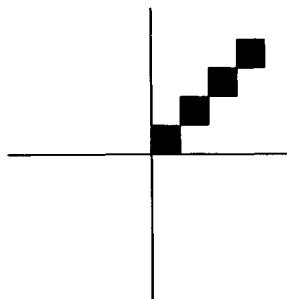
We must therefore start from the corner of the square space and blacken a line every point of which shall be as much above the lower boundary as it is to the right of the left hand boundary. Thus:—



If we wish to represent spouse, we may imagine all the married men ranged off on one side and all the married women on the other side of a centre, and the following figure will represent the relative



Such a relative as fellowcountryman will be



Further pages have not been found.

***Chap. X. Copula and Simple Syllogism***

Written by amanuensis A, this carefully inscribed manuscript consists of three folded sheets of medium-light, laid, and lined white paper measuring  $7 \frac{3}{4} \times 9 \frac{3}{4}$  in., inscribed in black ink on the six rectos. There are four one-word revisions and a single word insertion by the amanuensis. The first recto of each sheet is numbered in red pencil in the upper right-hand corner, though probably not by Peirce.

96.29 Alfred] Alfred the Great (849–901).

98.6 This. . . . ] See De Morgan, *Formal Logic*, pp. 76–106 [chap. 5].

***Chap. XI. Logical Breadth and Depth***

Written by amanuensis A, this carefully inscribed manuscript consists of (1) seven sheets of medium-light, laid, and lined white paper measuring  $7 \frac{3}{4} \times 9 \frac{3}{4}$  in., inscribed in black ink on rectos only, and (2) two folded sheets of medium-light, laid, and lined white paper measuring  $7 \frac{3}{4} \times 9 \frac{3}{4}$  in., with an “E H Owen” embossing in the upper left-hand corner, and inscribed in black ink on the two rectos only. As the last of the nine pages is written on the first recto of the second folded sheet, the manuscript appears to be unfinished rather than incomplete. There are only a few minor corrections and revisions, all by the amanuensis.

***Chapter IV. Conception of Time (MS 237)***

Both this and the following item were written on 1 July 1873, with Copy A added a day later. The present manuscript consists of eight sheets of light, laid, and unlined paper measuring  $7 \frac{3}{4} \times 9 \frac{3}{4}$  in., watermarked ‘J. Whatman/1868’ and inscribed in black ink on rectos only. The nearly eight pages of text are carefully written and contain only a few minor revisions. Though Copy A seems to represent a rewriting of the concluding section of the first six pages of the manuscript, we have decided to print that section as it appears and then to append the nearly two pages of Copy A. Page numbers 1 through 6 in the upper right-hand corners are not Peirce’s.

*Chapter IV. Conception of Time (MS 238)*

This manuscript was written immediately after Peirce had finished the first six pages of the preceding item. It consists of four sheets of light, laid, and unlined paper measuring  $7 \frac{3}{4} \times 9 \frac{3}{4}$  in., watermarked "J. Whatman/1868" and inscribed in black ink on rectos only. The three and one-half pages of text contain several revisions and corrections; one sentence toward the end has been deleted and then rewritten in the three concluding sentences. Page numbers 1 through 4 are not Peirce's.

*Chapter V. Reference to the Future*

Written immediately or very shortly after the two preceding items, the present manuscript consists of five sheets of light, laid, and unlined paper measuring  $7 \frac{3}{4} \times 9 \frac{3}{4}$  in., watermarked "J. Whatman/1868" and inscribed in black ink on rectos only. The five sheets can be divided into (1) four numbered pages that are published here and (2) a single unnumbered and discarded page of an earlier version of the second page that follows from page 1. But Peirce then rewrote the second page and added two more. Though there are a number of corrections and revisions, the manuscript is generally well written. Page numbers 1 through 4 in the upper right-hand corners are not Peirce's.

*Notes on Logic Book*

These brief but highly interesting notes on the Logic Book are carefully written on the first recto of a folded sheet of medium-heavy, laid, and unlined white paper measuring  $8 \times 10$ , watermarked "Marcus Ward" and inscribed in black ink. Almost certainly they are retrospective rather than prospective and were written, very probably, in the summer of 1873.

*Peirce to Conger*

Written by amanuensis B and signed by Peirce, this three-page letter—or copy of the letter whose original does not survive—appears on pages 73-75 of L 248, the Coast Survey letterpress copy-

book, which also contains items 9, 41, and 43 in the present volume. (See the headnote to item 9 for a physical description of the book and paper.) Peirce's letter responds to Conger's letter of 27 December 1872 (which is contained in L 93). Little is known about Abraham Bogart Conger, except that he was once president of the New York State Agricultural Society.

- 109.7 I am . . .] Peirce was Acting Assistant in Charge of the Office (of the Coast Survey) from 15 April to 23 August 1872, while J. E. Hilgard was in Europe to pursue the possibilities of setting up an International Bureau of Weights and Measures.
- 109.8 [the charts] Conger had asked Peirce for copies of "Charts Nos. 3 &c as far as completed" of the Hudson River Survey, the results of which were published in several Coast Survey Reports.
- 110.15–17 The only paper. . . .] The reference is to P 52. See item 39 in W2:359–429.

### *Errors of Observation*

This untitled but carefully written three-page manuscript is taken from L 248, the Coast Survey letterpress copybook, which also contains items 9, 40, and 43 in the present volume. (See the headnote to item 9 for a physical description of the book and paper.) Written between 28 March and 2 April 1873, the dates of the two letters surrounding it, it appears on pages 125–27 of the copybook. (Compare this with P 77, the next item in the present volume.)

### *Theory of Errors of Observations*

Written early in the last third of 1872, this important paper was published as Appendix 21 in the *Coast Survey Report 1870*, which appeared in 1873. Robin MS 1600 contains an offprint whose first page was annotated by Peirce in 1909; the two annotations are reproduced below. In his paper, Peirce applies his logic of relations to probability, and he demonstrates that the principles of the theory of errors are in fact conditional probabilities.

- 114.25 indeterminacy] Peirce underlines the word in the offprint in Robin MS 1600 and writes in the left-hand margin: "*indefiniteness* not *indeterminacy*. Note 1909 Nov 17 PM 3:05." The specific difference between the two words is not clear in the present paper, especially as Peirce uses them interchangeably in several other places.

115.20  $1x = 1$ ] Peirce underlines the equation in the offprint in Robin MS 1600 and writes in the left-hand margin: “There must be some misprint here—probably  $1^x = 1$ . No there is no misprint. It is an *error*”; but the first sentence, excluding perhaps the equation, is crossed out. But if  $x$  is an absolute term, as the following sentence suggests it is in the present paper, the equation is not an error; it would be an error only if  $x$  were a relative term.

115.32–33 As De Morgan. . . .] De Morgan, *Formal Logic*, pp. 40–41 and 55; or “On the Structure of the Syllogism,” 380.

121.11 Crofton’s method] As delineated in Morgan William Crofton’s article on probability in the *Encyclopaedia Britannica*, 9th edition, 19:780.

125.1–4 I copy . . . :] See Chauvenet, *A Manual of Astronomy*, pp. 489–90.

125.20–26 Fechner. . . .] See Fechner, *Elemente*, I:120–28. For Peirce’s *Century Dictionary* definition of Fechner’s psychophysical law, see editorial note 294.24.

132.11–12 Mr. Glaisher . . . ,] Glaisher, “Discordant Observations,” 397.

132.26–27 The criterion . . . Peirce] Benjamin Peirce’s criterion, as quoted and discussed by Chauvenet, *A Manual of Astronomy*, 2:558, reads as follows:

*It is proposed to determine in a series of  $m$  observations the limit of error, beyond which all observations involving so great an error may be rejected, provided there are as many as  $n$  such observations. The principle upon which it is proposed to solve this problem is, that the proposed observations should be rejected when the probability of the system of errors obtained by retaining them is less than that of the system of errors obtained by their rejection multiplied by the probability of making so many, and no more, abnormal observations.*

133.5 Hipp] A Swiss scientist, Matthaeus Hipp (1813–1893) published two works on electrical chronographs.

133.5–30 Hipp. . . .] Peirce gives a somewhat shorter definition of the chronoscope in the *Century Dictionary*:

a time-measuring instrument consisting of a train of wheels, moved by a weight, with two dials having hands the wheelwork moving which is thrown in and out of gear with the main train by the action of a clutch worked by an electromagnet. The hands, at first stationary, are thrown into gear by the initial event of the period to be measured, and move until, at the final event, they are thrown out of gear and arrested by the clutch. The distance which they have traveled over the dials measures the interval between the two events.

135.10–11 accompanying figure] There is no such figure here. (But MS 210, which is not published here, contains a figure that more or less fits the description.)

136.11 Hirsch, Daumbusch] Adolph Hirsch (1830–1901), Swiss astronomer and geodesist. Daumbusch has not been identified.

### *Classification of Vids*

This carefully written four-page manuscript is taken from L 248, the Coast Survey letterpress copybook, which also contains items 9, 40, and 41 in the present volume. (See the headnote to item 9 for a physical description of the book and paper.) Written between 3 and 7 April 1873, the dates of the two letters surrounding it, it appears on pages 130–33 of the copybook. It contains a few deletions and minor revisions. The main title of the manuscript refers to Benjamin Peirce's *Linear Associative Algebra*, a memoir first published in 1870 but reissued with addenda and notes by Charles Peirce in *The American Journal of Mathematics* in 1881 and separately in 1882. See especially sections 38–70 in the 1870 work.

- 161.8 §§54–55] See Benjamin Peirce, *Linear Associative Algebra*, pp. 33–34.

### *Lazelle's One Law in Nature*

Haskell's *Index to the Nation* lists Peirce as the author of this unsigned review. Furthermore, there is an earlier draft in MS 236, missing the first page. The remaining six pages, numbered 2 through 7 in the upper right-hand corners, serve as copy-text for what is published in the present volume; they consist of six sheets of medium-light, wove, and lined white paper measuring  $7 \frac{3}{4} \times 9 \frac{3}{4}$  in., inscribed in black ink on rectos only. There are corrections, deletions, and revisions, though the manuscript is generally well written. Henry Martyn Lazelle (1832–1917), an officer in the U.S. Army, wrote several works on cosmology.

- 165.1 [gravitation]] As the word appears in parentheses in the *Nation* but not at all in Lazelle's work, it is an editorial insertion; consequently, we have put it in brackets rather than parentheses.

### *Rainfall*

Published late in 1873, the present article is a different version of a paper—with somewhat different focus and emphasis—which Peirce presented before the Philosophical Society of Washington

[DC] on 21 December 1872 under the title "On the Coincidence of the Geographical Distribution of Rainfall and of Illiteracy, as shown by the Statistical Maps of the Ninth Census Reports." (For copies of the presentation, see MSS 206 and 225. See also the final section of "A Theory of Probable Inference" in P 268, which will appear in volume 4 of the present edition.) The following abstract of the presentation was published in the Society's *Bulletin*:

The author called attention to the striking resemblance between the map showing the distribution of illiteracy (the percentage of population unable to read or write) in the United States, given in the Report of the Census of 1870, and the map showing the distribution of rainfall during the three winter months published in Mr. Schott's reduction and discussion of the Smithsonian observations of that element. Mr. Peirce suggested as a possible explanation for the resemblance that the copious winter rains would produce agricultural plenty, which in its turn would favor indolence. (P 68)

167.24 lowered.] In MSS 206 and 225, Peirce includes at this point a brief, helpful list of some of the characteristics of cold and warm spots:

*Cooler place*: top of atmosphere is lower, air flows in at the top, more air over that place, greater pressure at the bottom, wind blows away from the place;  
*Warmer place*: top of atmosphere is higher, air flows off at the top, less air over that place, less pressure at the bottom, wind blows toward the place.

170.10 General Myer's weather-maps] The reference is to Albert James Myer's *The Practical Use of Meteorological Reports and Weather Maps* (1871).

170.29-33 Mr. Schott. . . .] The reference is to Charles Anthony Schott's *Tables and Results of the Precipitation, in Rain and Snow, in the United States* (1872), especially to the five plates.

171.1 state.] In MSS 206 and 225, Peirce discusses the relationship between rainfall and illiteracy in greater detail. He indicates that the illiterate population is situated mainly in the South around the borders of rivers such as the Mississippi, Alabama, Red, and Tallahassee where the rainfall is much greater, and he emphasizes that the correspondence holds for winter but not for summer rainfall. (There is no such river as "the Tallahassee"; Peirce was probably thinking of the St. Marks area in Florida mentioned elsewhere in the manuscripts.)

171.34 10.6] In MS 225, the rainfall for Fort Brooke, Florida, is listed as 11.6 rather than 10.6.

### *Political Economy*

Peirce's interest in political economy and in mathematical economics, which appears in several letters in earlier years, finds its first

formal expression in this untitled manuscript. Torn from a notebook, it consists of eight sheets of exceedingly brittle and rather light, laid, and unlined blue paper measuring  $6 \frac{1}{2} \times 8 \frac{1}{4}$  in., inscribed in black ink on rectos only. It seems to fall into three sections of three, two, and three pages: (1) the beginning through the long paragraph on page 174; (2) what follows to the end of page 175; and (3) the last part following the date. There are only a few minor revisions in these carefully written pages.

### ***Multiple Algebra***

Published in the *Proceedings of the American Academy of Arts and Sciences* 10 (May 1874–May 1875): 392–94, the paper was “only read by title” at the 680th meeting of the Academy on 11 May 1875 because Peirce was in Europe from the spring of 1875 until the fall of 1876. MS 273 contains a four-page earlier draft, of which the third page is missing. In 1883 Peirce said the following about the paper:

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. (P 245)

Two of the pages of the manuscript that were lost may well be those contained in MS 274 (*/ On the Algebra of Quaternions /*). Perhaps an immediate continuation of the published paper, they read as follows:

This manner of considering algebras will, I believe, prove useful. I will proceed to show some curious results of applying it to quaternions. Hamilton's form of the algebra of quaternions is resolved, thus—

$$\begin{aligned} l &= X:X + Y:Y + Z:Z + W:W \\ i &= X:Y - Y:X + Z:W - W:Z \\ j &= Y:W - W:Y + Z:X - X:Z \\ k &= Y:Z - Z:Y + X:W - W:X. \end{aligned}$$

Now, an operation of the form  $X:Y - Y:X$  is obviously a quadrant rotation in the plane of  $X$  and  $Y$ . The quaternion algebra obviously therefore represents a space of four dimensions  $x, y, z$ , and  $w$ ; a rotation in the plane of any two dimensions not being discriminated from a rotation in the plane of the other two. I have no doubt that the

great secret of the power of quaternions consists in this implicit introduction of a fictitious fourth dimension. May it not be that the same fiction would assist the integration of such equations as Laplace's?

In Professor Peirce's form quaternions appears thus:—

$$i = A:A \quad j = A:B \quad k = B:A \quad l = B:B.$$

The geometrical interpretation of this is as follows. The four dimensions are to be divided into two pairs X,Y and Z,W. One dimension of each pair is to be regarded as the imaginary of the other. Thus A consists of X as its real part and y as its imaginary part and B of z as its real part and w as its imaginary part. Then the operations i and l are such that either reduces one pair of coordinates to zero and leaves those of the other unchanged; while the operations j and k are such that either substitutes for one pair of coordinates the values of the other pair and reduces this other pair to zero.

All these results were communicated some two years ago to the Philosophical Society of Washington but they have not all been printed.

For these results see P 82.

### *Abstract of Photometric Researches*

This untitled manuscript consists of twelve numbered sheets of medium-heavy, laid, and unlined white paper measuring  $7\frac{3}{4} \times 9\frac{3}{4}$  in., inscribed in black ink on rectos only. They represent an abstract of Peirce's major published scientific work, *Photometric Researches* (item 69 in the present volume), or rather an abstract of how that work was envisioned in 1875. (A brief examination of item 69 will show that the plan and organization of the work had considerably changed when it was finally published.) Although Peirce arranged for its publication in Germany in the summer of 1875, several problems intervened, and the work did not appear in print until August 1878. The present abstract, which aside from a few revisions is carefully inscribed, was probably written in the summer or fall of 1875. It is not known whether Peirce intended to include it in the published volume or whether he planned to publish it in a scientific journal or in a Coast Survey Report. For all references, see the editorial notes for P 118 below.

### *Fundamentals of Algebra*

This flawless manuscript consists of two first pages on two sheets of medium-light, laid, and unlined white paper measuring  $8\frac{1}{8} \times$

10 5/8 in. and inscribed in black ink. They were probably written in Paris in the late winter or early spring of 1876. Almost certainly there were further pages, but these have not been found.

### *Axioms of Geometry*

This manuscript consists of (1) one and one-half pages of finished text with Peirce's signature at the end and (2) four additional first pages that represent earlier unfinished and rough versions; only the finished text is published here. There are five sheets of medium-light, laid, and unlined white paper measuring 8 1/8 × 10 1/8 in., watermarked "Lacroix Frères" and inscribed in black ink on rectos only; a sixth sheet measures 8 1/8 × 10 5/8. "30 Rue de Victoire" is written sideways in the upper right-hand margin of one of the additional sheets, indicating probably that Peirce wrote the manuscript while he was in Paris, in the early spring or in June of 1876.

### *Logical Contraposition and Conversion*

Peirce was in Europe from April 1875 until August 1876, and it was probably while he was in England that he read the first issue of *Mind*. The editor's review of *Syllabus of Plane Geometry*, entitled "Logic and the Elements of Geometry" (pp. 147–50), inspired him to write what is published in the present volume. Peirce wrote a four-page manuscript, of which only the last three pages, numbered 2 through 4, survive in MS 291 (the fourth page bearing Peirce's signature); the missing first page is what was published in the second issue of *Mind*. MS 291 consists of three sheets of medium-light, laid, and unlined white paper measuring 8 1/8 × 10 5/8 in., watermarked "Lacroix Frères" and inscribed in black ink on rectos only. We do not know whether Peirce sent only the first page of his manuscript to George Croom Robertson, or whether the editor decided to publish only the first and to return the remaining three to Peirce. Whatever the case may be, P 99 and MS 291 constitute one coherent and finished piece, and it is published as such in the present volume. Peirce's note as published in *Mind* is followed by a three-paragraph response from the editor, the first two paragraphs of which are reproduced below. See also the immediately following item.

191.1 Contraposition] Usually understood to mean “the obverse of the converted obverse,” contraposition here means simply “the converted obverse.”

191.4-5 See the Bibliography of Peirce’s References under “Robertson.”

192.11 This is the last line of P 99, followed in *Mind* by the editor’s response. As the third paragraph is just a further comment upon *Syllabus of Plane Geometry*, we reproduce only the first two paragraphs:

Mr. Peirce gets the contrapositive of All S is P without the double process of obversion and conversion, but does not, as far as I can see, impugn the validity of the double process. What he asserts I am far from denying, though I doubt whether his mode of treating the proposition is one that would in all cases be easily applied. The double process is always perfectly sure and simple. To obvert a proposition is to express it as negative if it is affirmative, as affirmative if it is negative: convert it in this obverse form, and then you have its contrapositive. It is interesting to note the consequence as regards the four typical propositions known to logicians as *A*, *E*, *I*, *O*. As every tyro knows, *A* is degraded in quantity when converted as it stands, and *O* cannot be converted at all; *E* and *I* alone get full justice in conversion. The scale is exactly redressed in contraposition: *E* becomes degraded in quantity, when converted in the obverse form, and *I* cannot be contraposed at all; on the contrary, *A* and *O* get full justice.

Mr. Peirce’s objection, if objection it should be called, seems to be sufficiently met by saying that, since the word *not*, treated as a relative term, is its own correlative, one is at liberty to take account of that fact in dealing with the logic of affirmation and negation. The case would be different if one were setting up a logic of relation in general.

### *Addition to the note for Mind*

This carefully written manuscript, which has only five minor revisions and corrections, consists of three sheets of medium-light, laid, and unlined white paper measuring  $8 \frac{1}{8} \times 10 \frac{5}{8}$  in., watermarked “Lacroix Frères” and inscribed in black ink on rectos only. The three pages are numbered in the upper right-hand corner and signed at the end by Peirce. It is not clear whether it was intended to go along with P 99 or with the four pages made up of P 99 and MS 291; whether Peirce wrote it before or after the appearance of P 99; and whether or not he sent it to the editor of *Mind*. There is no relevant correspondence in L 293 that sheds light on these questions.

### *Non-Associative Multiplication*

This well-written manuscript, which contains only a few corrections, consists of six sheets of medium-light, laid, and unlined white

paper measuring  $8 \frac{1}{8} \times 10 \frac{5}{8}$  in., watermarked "Lacroix Frères" and inscribed in black ink on rectos only. The last one-half page of text has been found in one of the fragment folders. Page numbers 1 through 6 appear in the upper right-hand corners. "Sketch of the Theory of" replaces the crossed-out "Note on."

200.27–28 Taylor's Theorem] For a modern version of the theorem, see Richard Stevens Burington, *Handbook of Mathematical Tables and Formulas*, 5th edition (New York: McGraw-Hill, 1973), pp. 56, 245. See also W2:406.

201.10–11 This is. . . . ] See De Morgan, *Formal Logic*, p. 7.

### *Principles of Mechanics*

This carefully written manuscript, which contains several corrections and revisions only on the first page and on the last four pages, consists of four discrete texts: (1) a single page whose continuation has not been found, (2) two and one-fourth pages, (3) two and one-half pages, and (4) nearly four pages, entitled "Problem I," which may or may not be a continuation of one of the first three texts. All eleven sheets are of medium-light, laid, and unlined white paper measuring  $8 \frac{7}{8} \times 10 \frac{5}{8}$  in., watermarked "Lacroix Frères" and inscribed in black ink on rectos only. The first three texts are entitled "The Principles of Mechanics." The exact order in which all four were written cannot be determined.

### *Nicholas St. John Green*

This obituary notice of Nicholas St. John Green consists of two parts, only the second of which is by Peirce; the author of the first part is Henry W. Paine, a lawyer and member of the Cambridge Scientific Club. Peirce knew Green especially as a member of the Cambridge Metaphysical Club and later referred to him as "a profound lawyer, a most genial man, and a great admirer of Alexander Bain." Green was born 30 March 1830 and died of an overdose of laudanum, perhaps taken accidentally.

208.9 Chauncey Wright] Wright died 12 September 1875 and, according to Green's son Frederick, had been his father's "closest" and "most intimate" friend.

- 209.4 Judge Curtis] Benjamin Robbins Curtis (1809-1874).  
 209.18 Here begins the part of the obituary written by Peirce.

### *Sensation of Color*

On 25 October 1875 Peirce received two grants from the Bache Fund of the National Academy of Sciences (of which he was elected a member in April 1877), one to study color (\$1200) and the other to compare sensations (\$500); the present note is probably the first published result of these studies—and the first of several later contributions on the subjects of color and sensations (such as P 282, 327, 379, and 997). After its initial publication in the *American Journal of Science and Arts*, the note was republished with minor changes in the Supplement to the British *Philosophical Magazine and Journal of Science*, 5th ser. 3 (1877): 543-47. (For some very favorable remarks on the present article, see Ogden N. Rood's famous *Modern Chromatics* [1879].)

- 211.19 Fechner's law] For Peirce's definition in the *Century Dictionary*, see editorial note 294.24.  
 212.11 Now....] Peirce relates the hue-shift, or Bezold-Brücke phenomenon, to practical problems in astronomy at a number of places, as in P 179 where he refers to "a certain yellow element" that is added as brightness increases and that causes observers to estimate the brighter of two double-stars to be different in color from the other. For a further discussion of this hue-shift see P 149, a review of Rood's *Modern Chromatics*, which will be published in volume 4 of the present edition.  
 213.11-14 In my book....] See the first chapter of item 69 in the present volume, pp. 382-89.  
 215n.1 No such note has been found.  
 216.18-19 My....] See the headnote above.  
 216.22 Professor Rood] Ogden Nicholas Rood.

### *Flexibility of the Pendulum Support*

From 27 September to 2 October 1877, Peirce represented the United States at the Conference of the International Geodetic Association in Stuttgart, Germany, where he gave the paper, in French, whose English version is published here. The paper was written in June-July of 1877; sent to Emile Plantamour in Geneva on 13 July (who in turn sent a lithograph copy of the paper to General Ibañez, the president of the Association); circulated among participants be-

fore the conference; and published the following year, in French, in the conference proceedings. Peirce's paper was very well received—and favorably commented upon by Oppolzer, Plantamour, and Cellérier ("my three former opponents, one after another, rose and fully acknowledged that I was in the right," Peirce later said)—and he thus became the world authority on the flexure of pendulum supports and a full-fledged member of the international scientific community. Peirce first measured the flexure of the tripod for the Repsold-Bessel pendulum at the Geneva Observatory in August of 1875. By taking into consideration the flexure of the stand, Peirce was able to account for the discrepancy that existed between the values of the seconds-pendulum as determined by the reversible pendulum and as determined by the method of Bessel introduced in 1826. What is published here is the English version that appeared as part of Appendix 14 in the *Coast Survey Report 1881*, published in 1883. "Translated from French into English by the author" appears in parentheses beneath the title. (See also MSS 308, 309, 312, and 313, as well as P 102 and 131. For a related paper see P 126, presented by title before the American Academy of Arts and Sciences on 13 March 1878, and published in the Academy's *Proceedings* as "On the Influence of Internal Friction upon the Correction of the Length of the Seconds' Pendulum for the Flexibility of the Support.")

217.7 Messrs. Repsold] A. & G. Repsold was a manufacturing firm in Hamburg.

217.12 Baeyer] Johann Jakob Baeyer.

219n.5 Peirce refers almost certainly to criticisms that were made at the conference itself, some of which appear in the minutes of the conference meetings and in published statements made by Oppolzer and Plantamour (see the proceedings in P 131), and criticisms that may have been made by colleagues in the Coast Survey.

220.1–222.14 Peirce's substitution is probably the result of such criticisms as outlined in the preceding editorial note.

222.23 Let us . . . ,] By one oscillation Peirce means one swing from, say, left to right; that is, he means  $\frac{1}{2} T$  in the modern sense.

223.13 Atwood's] The reference is to George Atwood.

225.6 13th of September] The full date, namely 13th of September 1875, is given in the two French versions of this paper, as well as in MSS 309 and 313.

225.20 Paalzow] Carl Adolph Paalzow.

226.2 Morton] Henry Morton.

227.3 Rogers] William Augustus Rogers.

- 227.13 Following Appendix 17 (P 256) in the present *Coast Survey Report*, p. 463, we read as follows: "Correction.—On page 432 above, the heading 'Dynamical Flexure' is not in the original. This heading correctly describes the experiments, but this phrase was first used later by Professor Plantamour.—[C.S.P.]" The "original" refers to the original French version, for which see P 102 and 131.
- 232.18 There is an obvious error in this line, for the numbers do not yield the proper results. If the "chronometer interval" were changed to 299.9171 (which is the number appearing in P 102 and MS 313) we get the correct "corrected time" of 299.9136, though the final three digits of the "period" should be 421 (which number appears in MS 313); if, on the other hand, the "chronometer interval" is correct, the "corrected time" should be 299.9139, which divided by the "number of oscillations" yields 422 as the final three digits of the "period". Our evidence is insufficient to suggest exactly what changes should be made to correct the obvious error; consequently, we give the line as published in the *Coast Survey Report*.
- 234.7 Rather than the two dots indicating a 15, there is a 16 in P 102 and 131, the original French versions.

### *New Class of Observations*

This manuscript consists of five sheets of heavy, wove, unlined white paper measuring  $8\frac{1}{2} \times 11$  in., inscribed in black ink on rectos only, as well as a dark grey envelope measuring  $4\frac{1}{2} \times 7\frac{1}{2}$  in. It was written almost certainly in the summer of 1877. The envelope contains, as indicated on its outside in black ink, "74 Scraps of Ribbon —Numbered in order of apparent brightness of light by CSP on a dark day." Though the manuscript is generally well written, there are quite a number of deletions, corrections, and revisions.

- 236.30-33 Here I. . . .] The reference is to the envelope mentioned in the headnote above.
- 237.15-16 as Aristotle. . . .] Reported by Alexander Aphrodisiensis, in his commentary upon *Topics* 149.9-17, as having been said in Aristotle's *Protrepticus*. See *The Works of Aristotle*, edited by David Ross (Oxford: Clarendon, 1952), vol. 12, p. 27: "since even to inquire whether we ought to philosophize or not is to philosophize."

### *Grassmann's Calculus of Extension*

Published in the *Proceedings of the American Academy of Arts and Sciences* 13(1877-78): 115-16, the paper was "read Oct. 10, 1877" before the Academy. It must have been read by someone else, perhaps his father, for Peirce did not return to the United States until

early November, after having attended the international geodetic conference held in Stuttgart, Germany, from 27 September to 2 October. Peirce's paper was probably written before 26 September 1877, the date of Hermann Gunther Grassmann's death.

238.4–5 For full bibliographic information concerning Grassmann's paper, see the Bibliography of Peirce's References.

### *Fixation of Belief*

For a detailed introduction to the genesis and significance of the following six items, published in the *Popular Science Monthly* under the collective title "Illustrations of the Logic of Science," see the introduction to the present volume. The first of the six papers, or several parts thereof, was initially written in 1872 (see items 9–11 above), as Peirce later recalled, and it contained "the earliest formulation of a method of logical analysis that [I] had had the habit of alluding to as [my] 'pragmatism,'" and it "was the tiny seed that under the culture of richer minds, grew into the goodly tree of that same appellation that already begins to afford a comfortable and wholesome lodge for many a soul" (Robin MS 633). Peirce later intended this paper as essay 8 of the "Search for a Method" (1893), as chapter 5 of "How to Reason" (1894), and as the leading item in a collection of his essays on pragmatism, variously titled "Studies in Meaning" or "Essays Toward the Interpretation of our Thoughts: My Pragmatism" (1909). See also the French version, item 66 in the present volume.

243.11 "He wrote . . . ,"] William Harvey's comment to John Aubrey; see the latter's *Brief Lives*.

243.31–32 "Lege. . . ."] "Read, read, read, work, pray, and read again" is a sentiment frequently expressed in the old alchemical texts, but an exact source has not been located.

247.1–29 With this third section compare item 9 above, pp. 22–23.

247.7–9 The Assassins. . . . ] See editorial note 22.10 above.

247.30–248.35 With this fourth section compare item 10 above, pp. 23–24.

248.36–257.23 With this fifth section compare item 11 above, pp. 24–28.

251.3–4 from the days. . . . ] From the beginning to the present, in other words. Numa Pompilius was the legendary second king of Rome (715–672 B.C.), and Pius IX, or Giovanni Mastai Ferretti, was pope from 1846 until his death in 1878. He was the prime mover in the acceptance, on 18 July 1870, of the doctrine of papal infallibility, an event that deeply impressed Peirce.

- 252.28-31 Plato. . . .] See Plato, *Timaeus* 35-39 or *Epinomis* 990-92.  
 253.9-10 And so. . . .] See Bacon, *Novum Organum*, bk. 1, aphs. 19 and 21, for examples.

### *How to Make our Ideas Clear*

The second of the six “Illustrations” is, like the first, one of Peirce’s best known and most frequently reprinted papers. In a letter to his mother dated 2 November 1877, Peirce says that he “wrote the best part of [his] second paper” between 13 and 24 September while sailing to Plymouth, England. Though he says elsewhere that the second paper was first composed in French, there is little reason to trust the statement. Peirce later intended this paper, which was first published in the January 1878 issue of the *Popular Science Monthly*, as essay 9 of the “Search for a Method” (1893) and as chapter 16 of “How to Reason” (1894). See also the French version, item 67 in the present volume.

- 259.20 Such . . . .] See Descartes, *Principia*, Part I, §45 et seq.; see also editorial note 71n.16 in W2:505.  
 260.1-3 Accordingly . . . .] See Leibniz, in his Eighth Letter to Burnet; see also editorial note 71n.17 in W2:505-6.  
 261.14 Melusina] An apparent (prophetic) allusion to his wife Zina, Harriet Melusina Fay Peirce, who was soon to disappear from Peirce’s life.  
 261.20 The . . . papers] See the immediately preceding item in the present volume, pp. 242-57.  
 270.29-32 In a recent. . . .] See Kirchhoff, *Mechanik*, Preface.  
 272.10-16 When Scotus. . . .] Not located, though no doubt in Erigena’s *De divisione naturae*.  
 272.30-31 “Historia Calamitatum”] A record of Abelard’s life from 1119 until 1132, composed in or soon after the latter year, this letter “written to a friend” appears at the beginning of the first volume of Cousin’s *Abaelardi Opera*; for a modern translation, see Betty Radice’s 1974 Penguin edition, *The Letters of Abelard and Heloise*.  
 274.16 “Truth . . . ,”] This is the first line of stanza 9 of William Cullen Bryant’s “The Battle-Field.” For the full stanza, see item 4 in the present volume, p. 16.  
 274.24-27 Thomas Gray, *Poetical Works*, p. 56 [“Elegy Written in a Country Churchyard,” stanza 14]. See W2:104.

### *Doctrine of Chances*

In a letter dated 4 February 1878, Peirce told his mother that the “March number of the Pop. Sci. will contain my article on the Theory of Chances which I fear however they will have to divide into two.”

Peirce's fears came true, for "The Doctrine of Chances" appeared in the March issue of the *Popular Science Monthly*, and "The Probability of Induction" in the April issue. (For a single outline of the two articles, see the unpublished MS 316.) The two parts of the article were written sometime between December 1877 and February 1878. Peirce later intended "The Doctrine of Chances" (and "The Probability of Induction"?") as essay 10 of the "Search for a Method" (1893) and as chapter 18 of "How to Reason" (1894).

- 277.5–6 Of those . . . resemblances,] Bacon, *Novum Organum*, bk. 2, aph. 27.
- 278.17 Quêtelet, Galton] See Adolphe Quêtelet's *Théorie des probabilités* (1853) and Francis Galton's *Hereditary Genius* (1869).
- 279.25–27 See the immediately preceding item, pp. 257–76.
- 279.37–280.4 Locke, *Essay*, p. 150 [bk. 4, chap. 15, sec. 1].
- 280.17–18 As. . . . "] Ibid. The *such as* is not italicized in the original.
- 280.28 "Being . . . ;] Peirce found the statement in Hegel's *Wissenschaft der Logik*, pp. 79–80. See W2:317 and the accompanying editorial note for 317:10–12.
- 280.28–30 and this. . . . ] See the immediately preceding item, pp. 257–76.
- 281n.1–2 The. . . . ] For Peirce's review of Venn's *Logic of Chance*, see W2:98–102.
- 285.26–28 that famous. . . . ] See 1 Cor. 13.

### *Probability of Induction*

Written with the preceding item between December 1877 and February 1878, the fourth paper of the "Illustrations" is a continuation of the third. Peirce later intended it as essay 11 of the "Search for a Method" (1893) and, with "The Doctrine of Chances," as chapter 18 of "How to Reason" (1894).

- 291.29–32 The conception. . . . ] See the preface to Venn's *Logic of Chance*. See also W2:98–102.
- 294.24 Fechner's psycho-physical law] Peirce gives the following definition in the *Century Dictionary*:

the law that as the physical force of excitation of a nerve increases geometrically the sensation increases arithmetically, so that the sensation is proportional to the logarithm of the excitation. Thus, if with a given degree of attention we just perceive the difference between the sensations of pressure produced by 1 pound and 1.1 pounds, we shall also just perceive the difference produced by 2 pounds and 2.2 pounds. The differences of sensation are thus the same in the two cases, and so are the differences of the logarithms of the pressures. According to Fechner, the total sensation varies directly with the logarithm of

the stimulus divided by the stimulus just sufficient to give an appreciable sensation, or  $s = k \log_{x_1} x$ . This is Fechner's formula.

296n.1-2 The quotation is from De Morgan's *Formal Logic*.

298.9-11 In a . . . ] See Quetelet, *Théorie des probabilités*, pp. 43-47 [part 2, chap. 1].

304.11-12 the Abbé . . . ] See the introduction to Gratry's *Logique*. See also bk. 1, chap. 1; bk. 3, chap. 4; and bk. 4, chap. 7.

304n.1-3 See especially the introduction to Gratry's *Logique*.

### *Order of Nature*

Written in February and March of 1878, the fifth paper of the "Illustrations" was published in the June issue of the *Popular Science Monthly*. It raises some interesting questions regarding the logical as distinguished from the theological considerations that go into discussions of nature. Peirce later intended this paper as essay 12 in the "Search for a Method" (1893).

306.30-307.3 He . . . perfect.] See Etienne Vacherot, *La religion* (1869), bk. 2, chap. 5.

307.31-34 The greatest. . . . ] The reference is to Pierre Simon de Laplace's *Système du monde*. The anecdote of Laplace's answer to Napoleon is told in James R. Newman, *The World of Mathematics* (New York: Simon and Schuster, 1956), 4:2376-77.

308.4-9 "How often . . . ?"] Tillotson, *Works*, 1:346.

310.9-14 "How long. . . . "] Ibid., 1:347.

310.22-24 any plurality. . . . ] See De Morgan, *Formal Logic*, p. 39 and "On the Syllogism, No. V," 456 and 461-62.

312.31-32 In the. . . . ] See the immediately preceding item in the present volume, pp. 290-305.

314.22-25 induction depends. . . . ] See Mill, *Logic*, 1:343ff. [bk. 3, chaps. 3-4].

315.14-18 Mr. Mill. . . . ] Ibid., 1:351 [bk. 3, chap. 3].

316.9-10 The explanation . . . papers.] See the immediately preceding item in the present volume, pp. 290-305.

316.9-13 The explanation. . . . ] It might be said that this statement is the key to interpreting the entire series of six papers of the "Illustrations."

317.18-22 With reference . . . surface.] Berkeley, *Works*, 1:35-36 [*New Theory of Vision*, secs. 2-3].

### *Deduction, Induction, and Hypothesis*

The sixth, and last, of the "Illustrations" was written in April and May of 1878, and it was published in the August issue of the *Popular*

*Science Monthly.* Peirce later intended this paper as essay 13 of the “Search for a Method” (1893).

- 332.3 Mill’s four methods] For the four methods, that is the methods of agreement, of difference, of residues, and of concomitant variations, see Mill’s *Logic*, vol. 1, bk. 3, chap. 8.
- 334.11–14 “The subtlety . . . .”] Bacon, *Novum Organum*, p. 158 [bk. 1, aph. 10].
- 334.18 the law of Boyle] Peirce gives the following definition in the *Century Dictionary*:

in *physics*, the law that at any given temperature the volume of a given mass of gas varies inversely as the pressure which it bears. It was discovered by Robert Boyle, and published by him about 1662; but Edme Mariotte having published a book concerning it (about 1679), the law was for a long time called *Mariotte’s law*.

- 335.7–8 When . . . .] See Bernoulli’s *Hydrodynamica*.

336n.1–3 For the 1866 Lowell Lectures (including the “Memoranda Concerning the Aristotelean Syllogism”), see W1:358–514 (especially pp. 505–14); for the 1867 “Natural Classification of Arguments,” see W2:23–48.

### *Comment se fixe la croyance*

The first two of the six “Illustrations” were also published in French in 1878–79, in the *Revue Philosophique*, where their main title is simply “La logique de la science.” The French of the first paper is, according to at least one expert, “franchement mauvais,” while that of the second paper is considerably better. Still, both papers are better in English, notwithstanding Peirce’s claim that he preferred the French versions. For the genesis of the six papers, as well as the French and English versions of the first two, see the detailed account in the introduction to the present volume. For some of the references in the present paper, see item 60 above. (See also Gérard Deledalle’s detailed comparison of the French and English versions in the *Transactions of the Charles S. Peirce Society* 17 [1981]: 141–52.)

- 339.22 transmutation] The word “transsubstantiation” would have been better.

339.32–33 acceptant . . . cela;] “Posant les autres hypothèses possibles” would have been better for “setting down the alternatives” (243.9).

- 339.34–35 <Il a. . . . >] See editorial note 243.11.

- 340.9 *vingt et une*] The English “twenty-two” is “twenty-one” here, perhaps because it was thought that as Kepler’s twenty-second hypothesis was the right one, it could not be “irrational.”
- 340.19 *<Lege. . . .>*] See editorial note 243.31-32.
- 340.37 *en moyenne*] For “in the long run,” “à la longue” would have been better.
- 341.30 *contredire*] The proper translation of “contract” would have been “limiter.”
- 342.5 *décevante*] The proper translation of “fallacious” would have been “fallacieux.”
- 342.18 *cuivre*] The French for “copper” is “cuivre rouge.”
- 342.24 *bronze*] The French for “brass” is “cuivre jaune.”
- 342.39 *sommaire*] The proper translation of “general” is “générale.”
- 343.3 *en deux classes*] This represents a significant and inaccurate change from the division into “one class” and “the others” in the English version.
- 343.10-11 *une certaine. . . .*] This is a mistranslation of “a variety of facts are already assumed when the logical question is first asked” (246.14-15).
- 343.23-24 *l’importance . . . déduire*] “L’importance de ce qu’on peut déduire” would have been better for “importance of what may be deduced” (246.27).
- 343.28 *opération de logique*] “Réflexion logique” would have been better for “logical reflection.”
- 343.31 *le concept de qualité*] “La conception de la qualité” would have been better.
- 343.35-40 *La vérité . . . logique.*] The two sentences are poor translations of the English version; see 246.37-41.
- 344.1-32 With this third section compare item 9 above, pp. 22-23.
- 344.7 *règlent*] The word “façonnent” would have been better.
- 344.7-10 *Les Assassins. . . .*] See editorial note 22.10. The parenthetical “Hatchichins” does not appear in the English versions.
- 344.16-19 *Le doute . . . croyance.*] The two sentences should have been translated as one, with the following changes: “Le doute est . . . de se libérer . . . tandis que celui-ci . . . qu’on ne cherche pas à éviter ni à échanger contre une autre croyance.”
- 344.29 *quelque . . . croyance.*] “L’analogue de la croyance” would have been better for “analogue of belief” (247.26).
- 344.33-345.41 With this fourth section compare item 10 above, pp. 23-24.
- 344n.2 *mobiles*] Either “impulsions” or “propensions” would have been better for “impulses.”
- 345.6 *résultat.*] For some reason, the sentence that follows in the English version has been left out here: “But it will only do so by creating a doubt in the place of that belief” (248.1-2).
- 345.16 *pensons*] The word should have been “penserons.”
- 346.1-355.16 With this fifth section compare item 11 above, pp. 24-28.
- 346.14 *déçu*] The proper translation of “deceived” is “trompé.”

- 346.21 idées. . . . ] The proper translation of “views they already take” (249.15) is “conceptions qu’ils soutiennent déjà.”
- 346.26 avec quelques fous] The phrase “with some fools” does not appear in the English version.
- 346.34 décevant] The proper translation of “deceptive” is “trompeur.”
- 347.2 damné] The proper translation of “wretched” is “malheureux.”
- 348.20 Léon XIII] The English version has Pius IX. The change was made because Pius had died on 7 February 1878, and Leo had become pope three days later. See editorial note 251.3–4.
- 348.21 sacerdoce,] The proper translation of “priesthood” is “clergé.” Furthermore, the English “and no religion is without [a priesthood]” (251.5) has not been translated into French, perhaps because Peirce (or someone else) realized that Islam in fact has no priesthood.
- 349.6–7 si . . . imperceptible] This phrase does not convey the meaning of the English “so slow as to be imperceptible” (251.28–29).
- 349.16 longtemps que] The English “men are in such a state of culture that” (251.36–37) has been left out here.
- 350.10–13 See editorial note 252.28–31.
- 350.21 l’homme seul agit] The (admittedly) ambiguous English “man only acts” (252.38) should have been translated as “l’homme n’agit que.”
- 350.34–35 Aussi. . . . ] For some reason, “in Lord Bacon’s phrase” (253.10) is left out here. See editorial note 253.9–10.
- 352.18 intelligence,] For some reason, the sentence that follows in the English version has been left out here: “So that the social impulse does not cause me to doubt it” (254.24–25).
- 353.19–20 ainsi soient. . . . ] This does not convey the meaning of the English “be such as investigation would approve” (255.19–20).
- 355.5 sexes,] The second part of this sentence in the English version—“or to a reformed Catholic who should still shrink from reading the Bible” (256.40–41)—has been left out.
- 355.16 esprit,] In the English version, there is an additional paragraph that concludes the first paper; see 257.11–23.

### *Comment rendre nos idées claires*

Though Peirce insists that the second of the “Illustrations” was first written in French and then translated into English, there is no convincing evidence to support the claim. What is clear, however, is that the French of the second paper is much superior to that of the first; but the English version (item 61 in the present volume) seems nonetheless clearer and more precise. As with the preceding item, Peirce said that he preferred the French version—the result, perhaps, of his love of France and all things Gallic, and of his dislike of and impatience with E. L. Youmans, the editor of the *Popular Science Monthly*.

- 356.38 notre première partie] See the immediately preceding item in the present volume, pp. 338-55; see also pp. 242-57.
- 357.2 certitude] The word “infaillibilité” would have been better.
- 357.18 Telle . . . ,] See editorial note 259.20.
- 357.38-40 C'est. . . ,] See editorial note 260.1-3.
- 359.18 Mélusine, la belle fée] The English “the beautiful Melusina of the fable” (261.14-15) lacks the obvious pun on Harriet Melusina *Fay (fée)* Peirce. See also editorial note 261.14.
- 362.27-29 Cependant. . . ,] The English “yet it is conceivable that a man should assert one proposition and deny the other” (264.10-11) is much simpler and clearer.
- 363.diagram The two figures are somewhat different in the English version (see p. 264), but they prove the same thing.
- 363.17-18 S'il . . . sensations] The English “If there be a unity among our sensations” (265.12-13) is much clearer.
- 365.3 effets pratiques] Unlike the English “effects that have practical bearings,” the French phrase is ambiguous.
- 365.32-33 arrangements d'idées] Perhaps “idées” should have been “faits” (as it is a few lines later), but the English “arrangements of facts” seem to be conceptual.
- 367.13-15 Si le lecteur. . . ,] The English sentence contains an additional part: “but if mathematics are insupportable to him, pray let him skip three paragraphs rather than that we should part company here” (268.24-26).
- 368.14-15 La règle. . . ,] The last, and necessary, part of the English sentence has been left out here: “and then to geometrically add the paths” (269.17).
- 368.diagram The two figures are slightly different in the English version (see p. 269), and the arrows in figure 4 have been left out entirely.
- 369.5-6 au moment actuel] The accompanying footnote in the English version has been left out here: “Possibly the velocities also have to be taken into account” (270n.1).
- 369.21-23 Un livre. . . ,] See editorial note 270.29-32.
- 369.32 certains philosophes] This obviously lacks the pejorative quality of the English “some hair-splitting philosophers” (271.1-2).
- 369.37-38 tout esprit. . . ,] This does not convey the meaning of the English “every mind accustomed to real thinking” (271.8).
- 369n.1 Obviously, this note is absent in the English version.
- 370.36 comme on l'a vu] The English reference, “as we have seen in the former paper” (272.3), is clearer.
- 371.6-10 Commentant. . . ,] See editorial note 272.10-16.
- 371.27 *Historia calamitatum*] See editorial note 272.30-31.
- 373.1-3 L'opinion. . . ,] This sentence is much clearer in the English version, which also has an important footnote that is missing in the French; see 273.33-36 and 273n.1-4.
- 373.19-20 «La vérité . . . ,»] See editorial note 274.16.
- 373.22 la réalité . . . fait] This does not have the same meaning as the

English “the reality of that which is real does depend on the real fact” (274.18–19).

373.27–30 See editorial note 274.24–27.

374.31 In the English version, this paragraph is continued and there is, in addition, another concluding paragraph; see 275.27–276.7.

### *Ferrero's Esposizione*

This untitled review in the first issue of the *American Journal of Mathematics* is the first of several important contributions Peirce made to that journal. An incomplete and very early two-page version of the review is contained in MS 315. An Italian mathematician, geographer, and geodesist, Annibale Ferrero (1839–1902) was also an army officer and a diplomat.

379.2 Laplace . . . Crofton] Pierre Simon de Laplace; Siméon Denis Poisson; Johann Georg Hagen, who anglicized his Christian names after he moved to the United States; and Morgan William Crofton (1826–1900), who taught mathematics and mechanics in Ireland.

### *Photometric Researches*

Peirce had been made an assistant in the Harvard Observatory on 30 October 1869 and, though his position became unsalaried in 1872, his connection with the Observatory continued until the publication of his *Photometric Researches* in August 1878. When Peirce went to Europe in April 1875, he arranged to have his researches published in Germany. But Joseph Winlock died suddenly on 11 June 1875; Peirce did not get along well with either Arthur Searle, the interim director, or Edward Charles Pickering, who became director of the Observatory on 1 February 1877; more historical research needed to be done—and the book, Peirce’s only published book, did not appear for another three years. His photometric researches had begun in February 1872 and ended in January 1875. Early in 1875 he wrote his father that this would be *the* book on the subject, without a rival or second. But however pioneering and sound, Peirce’s major published scientific work is not definitive, and it was soon superseded by the *Harvard Photometry* (1884) and the *Revised Harvard Photometry* (1908), both of which incorporate earlier findings, including Peirce’s three published star lists (see P 95, 96, and 159). Printed in

a run of 600 copies, of which only 200 were sent to the United States, and published as volume 9 of the Annals of the Astronomical Observatory of Harvard College, *Photometric Researches* consists of a preface, five chapters, and five appended plates. For various reasons, we have reproduced here only chapters 1, 3, and 5, as well as the five plates, and we have added a sixth plate. See item 48 above (pp. 180-85) for the 1875 plan and organization of the work. (For a full description of the content and significance of the book, see Victor F. Lenzen's "Charles S. Peirce as Astronomer," in Moore and Robin's *Studies in the Philosophy of Charles Sanders Peirce* [1964], pp. 33-50; see also B. Z. Jones and L. G. Boyd's *The Harvard College Observatory* [1971], pp. 184-87.)

- 384n.1 For full bibliographic information, see the Bibliography of Peirce's References.
- 385n.1-3 For full bibliographic information regarding the two sources, see the Bibliography of Peirce's References.
- 388.6-7 *Fechner's . . . law*] For Peirce's definition in the *Century Dictionary*, see editorial note 294.24.
- 389.4 brighter.] Here follows the 82-page second chapter entitled "On the Numbers of Stars of Different Degrees of Brightness," which is not published here (but see the following note). Though Fechner's law is not exact, it does seem to be approximately true that sensations vary as the log of the corresponding variation in the exciting force; accordingly, scales of observed relative stellar magnitudes are arranged so that equal increments in magnitude correspond (nearly) to equal increments in the logarithm of the relative amount of light as measured with the photometer. Peirce opts for supposing that the errors made by previous observers are distributed normally, so that the remaining major reason for differences in their estimates of stellar magnitudes is that each uses a different scale. He therefore (in the second chapter) reduces the scales of a number of other observers to one scale that he calls his "scale of equitable distribution." In most cases the reductions are effected by letting the numbers of stars an observer has assigned to a given magnitude or higher on the observer's original scale determine the reduced magnitude to be assigned on Peirce's scale. The limiting magnitudes obtained by counting the number of stars estimated to be a certain magnitude or higher are large numbers relative to the standard range of values for stellar magnitudes which is between 1 and 10. Peirce reduces these large numbers to within the standard range with the function

$$l_m = 1.892958 \log n(m) - \frac{1}{3}$$

where  $l_m$  designates the limit of the magnitude on Peirce's scale of equitable distribution and  $n(m)$  the number of stars on the original scale listed as being of a certain magnitude or higher. To calculate the mean magnitude, Peirce uses the function

$$M_m = 1.892958 \log \frac{n(m_1) + n(m_2)}{2} - \frac{1}{3}$$

He then proceeds to reduce the scales of ten catalogues to his scale of equitable distribution. In some cases he first increases the number of stars (that is, those left out) in the original catalogue before reduction to his scale. The ten catalogues so reduced are (1) the catalogue in Ptolemy's *Almagest* (540 stars), (2) the catalogue of Abd-al-Rahman al-Sûfi as translated by Schjellerup (380 stars), (3) Hyde's edition of the catalogue of Ulugh Beg (539 stars), (4) the catalogue in Tycho Brahe's *De nova stella* (559 stars), (5) Tycho Brahe's catalogue in Kepler's *Rudolphine Tables* (661 stars), (6) the catalogue of Hevelius (1097 stars), (7) Argelander's *Uranometria nova* (2340 stars), (8) Heis's *Catalogus Stellarum* (3904 stars), (9) Behrmann's catalogue (2324 stars) and (10) the *Durchmusterung* (314925 stars). Plate I in the *Photometric Researches*, which is reproduced in the present volume, displays the result of these reductions, the grid representing the magnitudes on Peirce's scale of equitable distribution. The single whole numbers after each observer represent where the mean of that observer's magnitude falls upon Peirce's scale. The vertical lines represent limiting magnitudes. DM and H are expressed in decimals. In other cases, numbers such as 2.1 or 2.3 are not decimals but represent sets of stars that are brighter or less bright than the average magnitude (in this case magnitude 2).

In the course of the ten reductions Peirce gives much information concerning the catalogues of Beg, Heis, Ptolemy, Sûfi, and Tycho Brahe, and the *Durchmusterung*. Most notable is his observation that Halma and Baily did not transcribe Ptolemy's magnitudes (which are probably those of Hipparchus) to thirds as they are presented in the original.

In addition to the ten catalogues, Peirce describes Sir William Herschel's results concerning stellar magnitudes (in the *Philosophical Transactions* of 1796, 1797, and 1799) and using an empirical linear equation, reduces these results to his scale. These results, along with others not represented in Plate I, are listed in Peirce's catalogue at the end of chapter 3.

Though Peirce later indicated that his scale of equitable distribution was not very equitable, it did have the advantage of permitting some comparisons, of smoothing out some improbable previous values, and of yielding a catalogue in which the probable error of a single magnitude does not exceed  $\frac{1}{10}$  of a magnitude.

The log of the light-ratio for Peirce's scale is 0.4402 (see chapter 2, p. 14, and chapter 4, p. 158 in the book). Pogson's ratio is 2.512 and

Peirce's 2.756. The *Revised Harvard Photometry* used Pogson's ratio and did not decrease this ratio as the brightness increased, an improvement suggested by Peirce in chapter 2 (p. 15).

389.5-18 This list of abbreviations actually occurs at the end of the second chapter of the book (p. 88); we have included it here because the abbreviations are used in what follows.

390.30-31 In . . . memoir,] A letter from Peirce to Joseph Winlock dated 16 March 1875 indicates that there was such a comparative color-catalogue; but it does not appear in the book, nor has it been found among the Peirce Papers at Harvard or elsewhere.

391.11 Rosén] A Swedish astronomer and geodesist, Per Gustaf Rosén (1838-1914) published a variety of scientific studies.

418.17-18 *Durchmusterung*] For bibliographic information, see the Bibliography of Peirce's References under "Argelander."

453.1 The magnitudes given for these 494 stars are those of Sir William Herschel (H), Argelander (A), Heis (H), *Durchmusterung* (DM), Sir John Herschel (h), Seidel (S), Zöllner (Z), and Peirce (P). Peirce's values require the following corrections (as indicated by Peirce on p. 171 in the fourth chapter of his book.) (See p. 539).

475.32 mine.] Here follows the 32-page fourth chapter entitled "Comparisons of the Different Observers," which is not published here. In it Peirce makes statistical comparisons of the reduced magnitudes of Ptolemy, Beg, Brahe, Argelander, Hevelius, Sir William Herschel, Sir John Herschel, the *Durchmusterung*, Seidel, Zöllner, Heis, and Peirce, and he discusses the difficulty of calculating the probable errors of observers using different scales and of observers having variable sensitivity to lights of different colors. There is a two-page section on the variability of stars, and Peirce concludes that "the general variability of stars is a barely appreciable quantity" of a cyclical character with a relatively short mean period. He further indicates that though there are many long cycles, most cycles are short, and that the mean variability has never before been calculated; in L 472 he gives the mean variability as equal to 0.0011 mag.

475.35-38 Sir . . . ] Under the assumption of homogeneous density, the length of the visual line to the edge of the galaxy is a function of the number of stars observed on that line, and this constitutes Herschel's "gauge."

475.35-476.2 Sir . . . plane.] Unlike Herschel or Struve, Peirce assumes equal distribution of magnitudes over any given section of galactic stars. For a brief reference to the relationship between Peirce's and Struve's view of the galaxy, see A. M. Clerke's *The System of the Stars*, 2nd ed. (1905), p. 341.

477.8 Chapter II] This chapter is not published here; but see editorial note 389.4 above.

478.13-14 page 26] The reference is to the second chapter. See the preceding note.

479.9 Heis's] The reference is to Eduard Heis (1806-1877), a German

Nos. of Catalogue	Cor.						
1-4	+0 <sup>m</sup> 04	140	+0 <sup>m</sup> 11	211-216	-0 <sup>m</sup> 13	389-393	-0 <sup>m</sup> 20
5	+0.05	141	+0.10	217	-0.14	394-403	-0.19
6-8	+0.06	142	+0.09	218-223	-0.15	404-407	-0.18
9-15	+0.07	143-148	+0.08	224-228	-0.16	408-415	-0.17
16	+0.08	149, 150	+0.07	229-232	-0.17	416-424	-0.16
17-21	+0.09	151, 152	+0.06	233-238	-0.18	425-427	-0.15
22, 23	+0.10	153-157	+0.05	239-244	-0.19	428-433	-0.14
24-27	+0.11	158, 159	+0.04	245-254	-0.20	434-437	-0.13
28-34	+0.12	160, 161	+0.03	255, 256	-0.21	438-444	-0.12
35-37	+0.13	162	+0.02	257-261	-0.22	445-447	-0.11
38-40	+0.14	163-168	0.00	262-269	-0.23	448-450	-0.09
41-45	+0.15	169-176	-0.01	270-273	-0.24	451, 452	-0.08
46-48	+0.16	177, 178	-0.02	274-278	-0.25	454-456	-0.07
49-53	+0.17	179, 180	-0.03	279-282	-0.26	457	-0.06
54-67	+0.18	181-187	-0.04	283-289	-0.27	458, 459	-0.05
68-81	+0.19	188-190	-0.05	290-328	-0.28	460-464	-0.04
82-107	+0.20	191	-0.06	329-346	-0.27	465-469	-0.03
108-118	+0.19	192-194	-0.07	347-349	-0.26	470-478	-0.02
119-123	+0.18	195-198	-0.08	350-361	-0.25	479-482	-0.01
124-127	+0.17	199	-0.09	362-368	-0.24	483-486	0.00
128-131	+0.16	200-206	-0.10	369-372	-0.23	487-490	+0.01
132-136	+0.15	207-209	-0.11	373-380	-0.22	491	+0.02
137-139	+0.14	210	-0.12	381-388	-0.21	492-494	+0.03

mathematician and astronomer who published an important star atlas in 1872.

479.10 Behrmann's] The reference is to Carl Behrmann, a German astronomer born in 1843, who published a work on meteors and another entitled *Atlas des südlichen gestirnten Himmels* (1874).

481.20 page 36] See note 478.13-14 above.

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- Wilson, William Dexter. *Lectures on the Psychology of Thought and Action, Comparative and Human*. Ithaca, NY: Andrus, McChain & Lyons, 1871.

## Chronological List 1872–1878

Three kinds of materials are included here:

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 and Index of the Published Works of Charles Sanders Peirce with a Bibliography of Secondary Studies*, ed. Kenneth L. Ketner et al. (Greenwich, CT: Johnson Associates, 1977), the letterpress companion volume to the 149-microfiche edition of Peirce's published works.
2. All of Peirce's known manuscripts and annotated offprints, identified by MS followed by a number. These numbers reflect the Kloesel 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. (Copies of several thousand pages of manuscripts and letters have recently been acquired from the National Archives in Washington, DC) Parentheses after the MS number give either the name or location of those collections, or they identify the Robin manuscript number. 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. Selected letters and letter drafts, identified by L followed by a (Robin) number. Parentheses give the location of letters not contained in the Peirce Papers.

Not included here, or in future lists, are (1) those items in *A Comprehensive Bibliography and Index* that merely make mention of Peirce's Coast Survey duties and observations (in the annual *Re-*

port of the Superintendent of the United States Coast Survey) or give brief descriptive notes of papers which were presented at professional meetings or on other occasions, but for which there are no manuscripts, and (2) those manuscript items that are of purely biographical interest, such as address books or diaries (like MS 299).

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*, NE to *The New Elements of Mathematics*, and Contributions to *Charles Sanders Peirce: Contributions to THE NATION* (3 vols., edited by Kenneth L. Ketner and James E. Cook [Lubbock: Texas Tech Press, 1975–79]).

Dates of publication or composition appear to the right; those in italics are Peirce's own. Descriptive or supplied titles appear in italic brackets. Journal titles are abbreviated. Items marked with an asterisk are published in the present volume.

- |  |                            |
|--|----------------------------|
| P 64–65  | 1872                       |
| [“ <i>Atlantic Almanac Calendars and Notes</i> ”]. Boston: James R. Osgood and Company, 1872.                      |                            |
| *MS 179 (364, 721)   | winter–spring 1872         |
| [Logic, Truth, and the Settlement of Opinion]. [CP 7.321–325.]   |                            |
| *MS 180 (364)  | winter–spring 1872         |
| [Investigation and the Settlement of Opinion]. [CP 7.317–320, with last two paragraphs of the manuscript omitted.] |                            |
| *MS 181 (360)  | winter–spring 1872         |
| Chapter 1. [CP 7.315–316; only the first and sixth paragraph of the manuscript.]                                   |                            |
| *MS 182 (361)  | winter–spring 1872         |
| Chapter 1 (Enlarged abstract). [CP 7.313–314.]   |                            |
| *MS 183 (362)  | winter–spring 1872         |
| Chapter 1 (Enlarged abstract).   |                            |
| MS 184 (363)   | winter–spring 1872         |
| [On Doubt and Belief].   |                            |
| *P 66  | 11 April 1872              |
| “Educational Text-Books, II.” <i>Nation</i> 14, 244–46. [ <i>Contributions</i> , 1:46–51.]                         |                            |
| MS 185 (1055)  | 4 May–10 June 1872         |
| Record of C. S. Peirce's Photometric Observations.   |                            |
| MS 186 (National Archives)   | 10–19 May 1872             |
| [Coast Survey Office Time Observations].   |                            |
| *MS 187 (L 248)  | between 11 and 14 May 1872 |
| Logic—Chapter 1: Of the Difference between Doubt & Belief.<br>[Cf. P 107 and 129.]                                 |                            |

*MS 188 (364, 333)	May-June 1872
Logic—Chapter 2. Of Inquiry. [Cf. P 107 and 129.]	
*MS 189 (366, 371, 333)	May-June 1872
Logic—Chapter 3. Four Methods of Settling Opinion. [Cf. P 107 and 129.]	
MS 190 (333)	summer 1872
<i>[On Logic and the History of Science].</i> [Cf. P 107.]	
*MS 191 (393)	summer-fall 1872
<i>[Lecture on Practical Logic].</i>	
*MS 192 (395)	summer-fall 1872
Third Lecture.	
MS 193 (396)	summer-fall 1872
<i>[On Kinds of Inference].</i>	
*MS 194 (370)	fall 1872
<i>[On Reality. Chapter 4].</i> [CP 7.336-345.]	
*MS 195 (368)	fall 1872
Chapt 4 (2nd draft).	
*MS 196 (369)	fall 1872
Logic. Chap 4 (— draft). [CP 7.326, with the second half of the manuscript omitted.]	
*MS 197 (375)	fall 1872
On Reality.	
*MS 198 (374, 396)	fall 1872
On Reality.	
MS 199 (371)	fall 1872
<i>[On Reality].</i>	
*MS 200 (367)	fall 1872
Logic. Chap. 4. Of Reality. [CP 7.327-335, with last two pages of the manuscript omitted.]	
MS 201 (935)	fall 1872
<i>[On Reality].</i>	
MS 202 (371)	fall 1872
<i>[On Reality].</i>	
*MS 203 (373)	fall 1872
Of Reality.	
*MS 204 (372, 371, 333)	fall 1872
Logic—Chapter IV. Of Reality.	
*MS 205 (371, 333)	fall 1872
Logic—Chapter IV. Of Reality.	
MS 206 (1131)	December 1872
<i>[Rainfall and Illiteracy].</i> [Cf. MS 225 and P 81.]	
*MS 207 (385)	winter 1872-73
Logic Chapter—. The list of Categories.	
MS 208 (1573)	1872
<i>[On the Radiation of Heat].</i>	
MS 209 (531, 810, 839, 278, 741)	1872
Brief Account of the Principles of the Logic of Relative Terms.	

MS 210 (1363)	1872
<i>/On an Electrical Chronometric Device].</i>	
P 71-72	1873
<i>[“Atlantic Almanac Calendars and Notes”]. Boston: James R. Osgood and Company, 1873.</i>	
*L 248	3 or 4 January 1873
<i>[Letter, Peirce to A. B. Conger].</i>	
MS 211 (381)	winter-spring 1873
<i>/On Representations].</i>	
*MS 212 (389)	winter-spring 1873
<i>On Representations.</i>	
*MS 213 (388)	winter-spring 1873
<i>On Representations.</i>	
*MS 214 (381)	winter-spring 1873
<i>On the nature of signs.</i>	
*MS 215 (377)	8 March 1873
<i>/On Time and Thought].</i>	
*MS 216 (376)	8 March 1873
<i>/On Time and Thought]. [CP 7.346–353.]</i>	
*MS 217 (378)	10 March 1873
<i>Logic. Chap 5th. [CP 7.354–357.]</i>	
*MS 218 (379)	10 March 1873
<i>Logic. Chap. 6th.</i>	
MS 219 (379)	10 March 1873
<i>Logic. Chap. 6th.</i>	
*MS 220 (394)	between 11 and 14 March 1873
<i>Memorandum: Probable Subjects to be treated of.</i>	
*MS 221 (380)	14 March 1873
<i>Logic Chap. 7. Of Logic as a Study of Signs.</i>	
MS 222 (National Archives)	14 March–25 June 1873
<i>/Coast Survey Office Time Observations].</i>	
*MS 223 (382)	15 March 1873
<i>Logic Chap 9th.</i>	
*MS 224 (L 248)	between 28 March and 2 April 1873
<i>/On Errors of Observation].</i>	
MS 225 (1131)	March 1873
<i>/Rainfall and Illiteracy. [Cf. MS 206 and P 81.]</i>	
*P 77	spring 1873
<i>“On the Theory of Errors of Observations.” <i>Coast Survey Report</i> 1870, 200–224. [NE 3:639–76.]</i>	
MS 226 (National Archives)	31 March–17 July 1873
<i>/Coast Survey Office Time Observations].</i>	
*MS 227 (L 248)	between 3 and 7 April 1873
<i>Linear Associative Algebra: Improvement in the Classification of Vids.</i>	
MS 228 (386, S 64)	spring 1873
<i>Of the Copula.</i>	

- \*MS 229 (386, 387) spring 1873  
 Chap VIII. Of the Copula.
- \*MS 230 (387) spring 1873  
 Chap IX. Of relative terms.
- MS 231 (396) spring 1873  
 /On Relative Terms.
- \*MS 232 (383) spring 1873  
 Chap. X. The Copula and Simple Syllogism.
- \*MS 233 (384) spring 1873  
 Chap. XI. On Logical Breadth and Depth.
- MS 234 (National Archives) 3 April-10 June 1873  
 /Hoosac Pendulum Observations and Chronometer Comparisons.
- MS 235 (National Archives) 15 May-18 July 1873  
 /Hoosac Pendulum Observations.
- P 76 30 June 1873  
 /“The Total Solar Eclipse of 21 December 1870”. *Coast Survey Report 1870*, 125. [See also Zina Fay Peirce’s report, pp. 125-27.]
- MS 236 (772) June 1873  
 /Draft of Lazelle Review (P 73).
- \*MS 237 (391) 1 and 2 July 1873  
 Chapter IV. The Conception of Time essential in Logic.
- \*MS 238 (390) 1 July 1873  
 Chapter IV. The Conception of Time essential in Logic.
- \*P 73 10 July 1873  
 “Lazelle’s *One Law in Nature.*” *Nation* 17, 28-29. [*Contributions*, 1:52-54. Cf. MS 231.]
- \*MS 239 (392, 371) summer 1873  
 Chapter V. That the significance of thought lies in its reference to the future. [CP 7.358-361.]
- \*MS 240 (396) summer 1873  
 Notes on Logic Book.
- MS 241 (National Archives) 19 August-22 October 1873  
 /Hoosac Time Observations and Computations.
- MS 242 (National Archives) 2 October 1873-2 August 1874  
 /Hoosac Pendulum Observations and Computations.
- MS 243 (National Archives) 19 November 1873-7 May 1874  
 /Harvard College Observatory Time Computations.
- MS 244 (National Archives) November 1873  
 /On the Determination of Local Variations in Gravitation.
- MS 245 (1024) 1873  
 Theory of Compound Colors.
- MS 246 (1020) 1873  
 Calculation of wave lengths of Maxwell’s primary colours.
- MS 247 (1058) 1873  
 Tracings showing our Groups of Stars 40°-50° N as they appear in Argelander.

MS 248 (1571)	1873
Moving Ball.	
MS 249 (1095, 194, 278)	1873
Two pendulums vibrating parallel act upon each other.	
P 79-80	1874
/“ <i>Atlantic Almanac</i> Calendars and Notes”]. Boston: James R. Osgood and Company, 1874. [See especially “The Course of the Planets” and the “Transit of Venus,” p. 4.]	
*P 81	1874
“Rainfall.” <i>Atlantic Almanac</i> 1874, 65. [Cf. MSS 206 and 225.]	
MS 250 (National Archives)	24 March 1874
/On Pendulum Experiments].	
MS 251 (National Archives)	7 May 1874
/Cambridge Observatory Pendulum Observations].	
MS 252 (National Archives)	10 June-6 November 1874
/Hoosac and Northampton Time Observations].	
MS 253 (National Archives)	14 June 1874
/Hoosac Pendulum Observations].	
MS 254 (National Archives)	14 June-21 August 1874
/Hoosac Pendulum Observations].	
MS 255 (National Archives)	24 June-2 August 1874
/Hoosac Pendulum Observations and Chronograph Readings].	
MS 256 (National Archives)	3 July-5 September 1874
/Hoosac Tunnel Pendulum Observations].	
MS 257 (1022)	summer-fall 1874
Memorandum of Studies to be made on Color.	
MS 258 (1049)	summer-fall 1874
Homogeneous light.	
MS 259 (1083, 1573, 1059, 194)	summer-fall 1874
Simple Pendulum hung by an elastic string.	
MS 260 (1573)	summer-fall 1874
Battery—Formulae.	
MS 261 (National Archives)	21 August-23 September 1874
/Hoosac Pendulum Observations].	
MS 262 (National Archives)	25 August-9 September 1874
/Hoosac Time Computations].	
MS 263 (National Archives)	28-29 August 1874
/Hoosac Pendulum Observations].	
MS 264 (National Archives)	29-30 August 1874
/Hoosac Pendulum Observations].	
MS 265 (National Archives)	5-26 September 1874
/Hoosac Mountain and Hoosac Tunnel Pendulum Observations].	
MS 266 (National Archives)	8-26 September 1874
/Hoosac Pendulum Observations].	
*MS 267 (1569)	21 September 1874
/On Political Economy]. [NE 3:547-50, with the dated section of the manuscript at the beginning rather than at the end.]	

MS 268 (National Archives)	26 September-6 November 1874
<i>/ Hoosac and Northampton Time Computations.</i>	
MS 269 (1059)	1874
<i>/ Star Catalogue.</i>	
MS 270 (1056)	1874
<i>Phyllotactic Numbers.</i>	
MS 271 (National Archives)	30 April 1875
<i>/ On Kew Observatory Pendulum Experiments.</i>	
MS 272 (1095)	spring 1875
<i>/ Hoosac Mountain Pendulum Research.</i>	
MS 273 (82)	April-May 1875
<i>On the Application of Logical Analysis to Multiple Algebra. [Cf. P 90.]</i>	
MS 274 (90)	April-May 1875
<i>[On the Algebra of Quaternions]. [A continuation, perhaps, of MS 273.]</i>	
*P 90	11 May 1875
<i>"On the Application of Logical Analysis to Multiple Algebra."</i>	
<i>Proc of the Am Acad of Arts and Sciences</i> 10, 392-94.	
<i>[CP 3.150-151. Cf. MSS 250 and 251.]</i>	
MS 275 (1059, 839, 1095, 1573, 1306)	spring-summer 1875
<i>/ Draft Pages of Photometric Researches (P 118).</i>	
MS 276 (National Archives)	31 May 1875
<i>/ On Kew Observatory Pendulum Experiments.</i>	
MS 277 (National Archives)	30 June 1875
<i>/ On Berlin Pendulum Experiments.</i>	
MS 278 (554, L 227)	summer 1875
<i>[Algebraic Notations and Relative Terms].</i>	
*MS 279 (1050)	summer-fall 1875
<i>/ Early Abstract of Photometric Researches (P 118).</i>	
MS 280 (National Archives)	23 September 1875
<i>/ On Geneva Pendulum Experiments.</i>	
MS 281 (National Archives)	9 December 1875
<i>Errata in the C. S. Star List of 1875.</i>	
MS 282 (1063)	1875
<i>/ Pendulum Research Notes.</i>	
MS 283 (1063)	1875
<i>/ Pendulum Calculations.</i>	
MS 284 (100)	1875
<i>First Attempt at a Geometry logically correct.</i>	
MS 285 (1059)	1875
<i>/ Descriptive Star List.</i>	
P 95	1875
<i>"A List of Stars for Observations of Latitude." Coast Survey Report 1873, 138-74 (Appendix 14).</i>	
P 96	1875
<i>"Errata in the Heis Catalogue of Stars." Coast Survey Report 1873, 175-80 (Appendix 15).</i>	

P 89		1875
	“A Plan and an Illustration.” In <i>The Democratic Party: A Political Study</i> , by A Political Zero [Melusina Fay Peirce] (Cambridge, MA: John Wilson and Son, 1875), pp. 36–37.	
MS 286 (839)		winter–spring 1876
	/On Inference as Substitution].	
*MS 287 (81)		winter–spring 1876
	Notes on the Fundamentals of Algebra.	
MS 288 (National Archives)		2 March 1876
	/On Geneva and Paris Pendulum Experiments].	
MS 289 (1018)		6 March 1876–15 February 1877
	/Color Experiments].	
MS 290 (National Archives)		19 April–4 June 1876
	/ Berlin Pendulum Observations].	
*MS 291 (278, 783)		spring 1876
	/Logical Contraposition and Conversion]. [Continuation of P 99.]	
*MS 292 (L 293, 783)		spring 1876
	Addition to the note for <i>Mind</i> . [Cf. P 99.]	
*MS 293 (98, 99)		spring 1876
	The Axioms of Geometry.	
*MS 294 (570, 278)		spring–summer 1876
	Sketch of the Theory of Non-Associative Multiplication.	
MS 295 (1098)		24 June–12 July 1876
	Barometer Readings.	
MS 296 (National Archives)		27 June–12 July 1876
	/Kew Observatory Time Observations].	
MS 297 (National Archives)		6 July 1876
	/On Pendulum Experiments at European Stations].	
*P 99		July 1876
	“Logical Contraposition and Conversion.” <i>Mind</i> 1, 424–25. [CP 2.550. Continued by MS 261; see also MS 262.]	
*MS 298 (1026)		summer–fall 1876
	The Principles of Mechanics.	
MS 300 (National Archives)		17 December 1876–22 November 1877
	/Coast Survey Office Comparisons of Thermometers].	
MS 301 (1059)		1876
	Coordinates of Relative Parallax.	
MS 302 (1059, 1050)		1876
	/Tables, Plates, and 1876 Title Page of <i>Photometric Researches</i> (P 118)].	
MS 303 (National Archives)		1 March–29 June 1877
	/Stevens Institute Time Computations].	
MS 304 (National Archives)		8 April 1877
	/On Pendulum Observations].	
*P 108		spring 1877
	“Nicholas St. John Green.” <i>Proc of the Am Acad of Arts and Sciences</i> , n.s. 4, 289–91.	

- \*P 100 April 1877  
 "Note on the Sensation of Color." *Am J of Science and Arts* 3d ser.  
 13, 247-51. [Cf. P 105.]
- MS 305 (National Archives) 18 May 1877  
*[On Pendulum Micrometer Experiments].*
- MS 306 (National Archives) May 1877  
*[On Pendulum Experiments at European Stations].*
- MS 307 (1093) May-June 1877  
 Note on the Theory of the Economy of Research. [Cf. P 160,  
 which will be published in W4.]
- MS 308 (1061) June 1877  
*[On the Flexure of Pendulum Stands].*
- MS 309 (1961) June-July 1877  
 Sur la Flexion des pieds des pendules. [Cf. P 102 and 131.]
- MS 310 (National Archives) 4 July-24 December 1877  
*[Stevens Institute Time Computations].*
- \*P 253 13 July 1877  
 "On the Influence of the Flexibility of the Support on the Oscillation of a Pendulum." *Coast Survey Report 1881*, 427-36. [Translation of P 102, 131; see also MSS 308 and 309.]
- \*MS 311 (1104, 1023) summer 1877  
 On a New Class of Observations, suggested by the principles of logic.
- P 102 August 1877  
 De l'influence de la flexibilité du trépied sur l'oscillation du pendule à reversion. [Unmarked copy in MS 312; cf. P 131.]
- MS 313 (National Archives) 6-8 September 1877  
*[Summary of Flexibility of Pendulum Stands Memoir (P 102)].*
- MS 314 (National Archives) 18 September 1877-27 May 1878  
*[Hoosac Pendulum Observations and Computations].*
- MS 315 (1366a) fall 1877  
*[Draft of Ferrero Review (P 111)].*
- MS 316 (762) fall-winter 1877  
*/Outline of 3rd and 4th "Illustrations" Paper (P 120 and 121)].*
- \*P 125 10 October 1877  
 "Note on Grassmann's Calculus of Extension." *Proc of the Am Acad of Arts and Sciences* 13, 115-16. [CP 3.152-153.]
- MS 317 (National Archives) 15 October 1877-5 January 1878  
*[Berlin Pendulum Observations and Comparisons].*
- MS 318 (1095) 21 November 1877  
 Measurement on Rutherford Micrometer of Berlin.
- MS 319 (National Archives) 21-23 November 1877  
*[Berlin Pendulum Observations: Measurement of Scale].*
- \*P 107 November 1877  
 "Illustrations of the Logic of Science. First Paper—The Fixation of Belief." *Popular Science Monthly* 12, 1-15. [CP 5.358-387. Cf. MSS 186-189 and P 129.]

- MS 320 (National Archives)                            17 and 24 December 1877  
     /On the Prussian and U.S. Pendulum-Meters].
- MS 321 (863, 892)                                    winter 1877-78  
     /Scientific Thought and Our Faith in Immortality, God, and Morality].
- MS 322 (1065, 1085)                                8 January 1878 - 11 April 1881  
     / Pendulum Observations and Comparisons].
- \*P 119    January 1878  
     "Illustrations of the Logic of Science. Second Paper—How to Make Our Ideas Clear." *Popular Science Monthly* 12, 286-302.  
     [CP 5.388-410. Cf. P 162.]
- MS 323 (National Archives)                            1 March 1878  
     /On the Quincuncial Projection: Table of Rectangular Coördinates].
- P 126    13 March 1878  
     "On the Influence of Internal Friction upon the Correction of the Length of the Seconds' Pendulum for the Flexibility of the Support." *Proc of the Am Acad of Arts and Sciences* 13, 396-401.  
     [Reprinted in P 253, pp. 437-39.]
- \*P 120    March 1878  
     "Illustrations of the Logic of Science. Third Paper—The Doctrine of Chances." *Popular Science Monthly* 12, 604-15. [CP 2.645-660.]
- \*P 111    spring 1878  
     /“Ferrero's *Esposizione del metodo dei minimi quadrati*”]. *Am J of Math* 1, 59-63. [NE 3:993-98.]
- \*P 121    April 1878  
     "Illustrations of the Logic of Science. Fourth Paper—The Probability of Induction." *Popular Science Monthly* 12, 705-18. [CP 2.669-693.]
- \*P 122    June 1878  
     "Illustrations of the Logic of Science. Fifth Paper—The Order of Nature." *Popular Science Monthly* 13, 203-17. [CP 6.395-427.]
- P 114    1 August 1878  
     /“Newcomb's *Popular Astronomy*”]. *Nation* 27, 74. [Contributions, 1:55.]
- P 117    8 August 1878  
     “Floating Magnets.” *Nature* 18, 381.
- \*P 123    August 1878  
     "Illustrations of the Logic of Science. Sixth Paper—Deduction, Induction, and Hypothesis." *Popular Science Monthly* 13, 470-82. [CP 2.619-344.]
- \*P 118    August 1878  
     *Photometric Researches: Made in the Years 1872-1875*. Annals of the Astronomical Observatory of Harvard College, 9. Leipzig: Wilhelm Engelmann, 1878.

P 161		13 December 1878
	“Measurements of Gravity at Initial Stations in America and Europe.” <i>Coast Survey Report</i> 1876, 202-337 and 410-16 (Appendix 15).	
MS 324 (National Archives)	<i>19 December 1878-2 March 1879</i>	
	/Allegheny Observatory Pendulum Observations].	
*P 129		December 1878
	“La Logique de la Science. Première Partie—Comment se fixe la croyance.” <i>Revue Philosophique de la France et de l’Étranger</i> 6, 553-69. [Cf. P 107 and MSS 186-189.]	
P 128		1878
	“Description of an Apparatus for Recording the Mean of the Times of a Set of Observations.” <i>Coast Survey Report</i> 1875, 249-53.	
P 131		1878
	“De l’influence de la flexibilité du trépied sur l’oscillation du pendule à réversion.” In <i>Verhandlungen der . . . fünften allgemeinen Conferenz der Europäischen Gradmessung</i> (Berlin: Georg Reimer), pp. 171-87. [Cf. P 102 and 253, and MSS 308 and 309.]	
MS 325 (1059)		1878
	Comparison of [Herschel and Seidel] magnitudes.	
MS 326 (1059)		1878
	/Star Catalogue].	
MS 327 (1059)		1878
	/Star Lists].	
MS 328 (1098)		1878
	/Pendulum Observations and Computations].	
P 159		1878
	“A Catalogue of Stars for Observations of Latitude.” <i>Coast Survey Report</i> 1876, 83-129 (Appendix 7).	
MS 329 (1600)		1878
	/Annotated Offprints of P 128].	
MS 330 (1600)		1878
	/Annotated Offprints of P 118].	
MS 331 (1572)		1878
	Projection of the whole sphere on a finite surface. [Cf. P 135, 151, which will be published in W4.]	
MS 332 (1355)		1878
	Quincuncial Projection [Tables].	
MS 333 (1353)		winter 1878-79
	/On the Quincuncial Projection].	
*P 162		January 1879
	“La Logique de la Science. Deuxième Partie—Comment rendre nos idées claires.” <i>Revue Philosophique de la France et de l’Étranger</i> 7, 39-57. [Cf. P 119.]	

## Essay on Editorial Method

Like the two previous volumes in this series, this volume represents a broad variety of materials—both in content and form—and consequently, editing techniques must differ at least for the two major categories of materials: previously published items and those which remain only in manuscript form. Moreover, there is an added dimension to the manuscripts contained in this volume, for some of them are inscribed by two different amanuenses. Of the sixty-nine items included here, all by Peirce, twenty were published in ten different publication sources, which thus present us with ten different house publication styles. Of the twenty published items, only one article was republished in English in a second source, and six others have either corresponding manuscript drafts or related manuscript material that pose textual complications. These seven are discussed at length below.

The remaining forty-nine items exist in manuscript form; sixteen of them are in the hand of the amanuenses. The problems these manuscripts pose derive chiefly from their physical state and from Peirce's working methods. Though widely scattered runs of manuscript pages had first to be reassembled, and in six instances pages appear to be irretrievably lost,<sup>1</sup> all but one of the manuscripts included in this volume are in good physical condition. In a few cases, portions of words are obliterated by frayed or torn edges, inkstains, or water damage, but these are easily restored.

Peirce's working methods, on the other hand, present a different set of problems. Most of the manuscripts included in this volume are either unfinished or, at the very least, unpolished. Though a few are fair copy, others are carelessly inscribed or hurriedly written, and still others are mere outlines. Many contain what at first glance seem

<sup>1</sup>These lost pages occur in MSS 188, 189, 230, 236, 273, and 298.

to be different "drafts" of the same piece. Some are even labeled as such. But close scrutiny reveals that this is not the case. Peirce seldom created "drafts" by adding revisions to an earlier version. Rather, he was more likely to begin anew, repeating perhaps the same or a similar first sentence but then going in a different direction and thus creating several versions of a single composition. More often than not, these versions cannot be confidently arranged in the order in which they were written, nor can they be collated; consequently, all versions become justifiable candidates for publication.<sup>2</sup>

Most of the manuscripts published here must fairly be called "working copies," and they display Peirce's usual pattern of simultaneous composition and self-editing (deletions, insertions, transpositions, false starts, incomplete revisions, notes to himself, and alternative words left undecided). As with his earlier manuscripts, he seems to be revising mainly for clarity of meaning. But in addition, he is now concerned with stylistic improvement as well. His characteristic earlier misspellings have now all but disappeared, and we find, rather, occasional inattentive slips of the pen or his life-long habit of leaving letters out of words. In his writings of the W2 period, he demonstrated his ability to distinguish accurate punctuation practices from the more cavalier sort found in his very early manuscripts. Although we are beginning to discern some patterns, like his habit of using commas before conjunctions in series, even in the W3 period he tends to be inattentive to such matters in "working copies." Moreover, he now delegates some of these matters to others. In a letter to the Coast Survey Office in Washington dated 23 October 1873, Peirce comments on an 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."<sup>3</sup> It matters little whether these "proof-readers" were employees of the public printer or the Coast Survey. We know it was the usual procedure at the Coast Survey office that an incoming report was copied out at least once upon receipt, circulated for comment and some substantive editing, and if necessary recopied before being sent

<sup>2</sup>MS 298, "The Principles of Mechanics," is a good example of Peirce's working methods. Also, the multiple versions of various chapters of the logic book series serve well to demonstrate the same technique over a larger range of materials and a more extended period of time.

<sup>3</sup>National Archives, Record Group 23.

to the printer. We know further that copyists usually imposed their own system of capitalization, punctuation, and spelling on the document being recopied. Since only two of the sixty-nine items in this volume are taken from *Coast Survey Reports*, we might be inclined to drop the matter here. But we cannot. Though we have not been able to identify the names of the two amanuenses represented in sixteen of the manuscripts published here, we know that both were Coast Survey employees whose duty it was to make fair copies of reports and correspondence in a neat, legible hand. For convenience of identification we have labeled the two amanuensis A and amanuensis B, and we can make certain generalizations about each.

Amanuensis A took dictation. One of his manuscripts included in this volume has on its verso pages an unidentified system of short-hand sprinkled with occasional longhand words or phrases, and in more than one of his manuscripts there are clauses attached to the wrong sentences. The problem of punctuation is compounded by the fact that he used a very fine pen point, and quite often periods are altogether missing though spacing and capitalization indicate the need for them. In contrast to B, who seemed to suffer from Peirce's habit of leaving letters out of words and who tended to use antiquated spellings, A seemed to have a better eye for such details. But B had the bolder editorial spirit, correcting Peirce's grammar, making minor (though not always appropriate) adjustments in sentence structure, and generally paying more attention to content and meaning than did A.

The first step in editing and preparing manuscripts for subsequent publication is the transcription process. To assure readers that they have before them what Peirce actually wrote, we have established certain transcription guidelines based on editorial theory and on the available working materials.

The Peirce Edition Project owns two sets of photocopies of the Peirce manuscripts deposited in the Houghton Library, Harvard University. These are third-generation copies made from an electroprint copy which, in turn, was printed from the negative microfilm copy at Harvard. 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 lead pencil or different colors of ink and pencil for revisions and annotations, the legibility of our copies falls away sharply. For this reason, after the initial transcriptions are made, they are read at least two times, by two different editors,

against the photocopies; passages that are difficult to read are marked. One of the editors then rereads the transcripts a third time, with the help of a local person, against the originals in the Houghton Library, paying particular attention to the "difficult to read" passages. At that time such features are noted as holes, ink blots, or colored pencil contained in the originals but not distinguishable on the copies. The transcripts are then revised to coincide with the Harvard originals.

During the process of transcription, Peirce's incomplete revisions, misspellings, misplaced or omitted punctuation, etc., are typed exactly as they appear on the manuscript page. The underlinings (single, double, even triple) that Peirce used for emphasis are included also, but are later interpreted as copy-editing instructions and are printed in the volume as italic, small capital letters, and regular capital letters respectively. As noted above, Peirce made internal revisions in his manuscripts, usually during the process of composition; some of the amanuensis manuscripts also contain substantive revisions in Peirce's hand. All such revisions are interpreted as Peirce's final intention and are incorporated into the initial transcriptions. Material that Peirce crossed out is omitted, as is accompanying punctuation that he failed to cross out; his caret-edited revisions are inserted, passages he marked for transposition are transposed, and his instructions for moving large blocks of material are followed.<sup>4</sup> For the most part, Peirce clearly marked such revisions and marginal annotations. When he did not, we use the following established guidelines for resolving difficulties.

Whenever the context permits, Peirce's unclear revisions and marginal annotations are inserted into the text. As both are instances of editorial intervention, we record them in the Emendations list with an accompanying textual note indicating the original placement of the material on the manuscript page. When the revision or mar-

<sup>4</sup>A listing of Peirce's alterations in the manuscripts is not included in this volume. We believe the reader is better served and can gain a better understanding of Peirce's techniques of composition if we devote the space available in the volume to presenting as many of his variant texts as possible and supply information on his alterations only when his final intentions in a particular manuscript remain unclear. Moreover, in view of the accessibility of the microfilm edition and the availability of inspecting sets of photocopies of the original papers at three research centers in the United States and Canada, we do not feel justified in increasing the size and cost of our volumes by adding information that could be better understood by consulting the original or a photocopy of the original.

ginal annotation cannot be incorporated into the text without creating a nonsensical reading, it is reproduced in the Editorial Notes, again with an explanation of its placement on the manuscript page. Amanuensis manuscripts aside, the only material on the manuscript page that the reader will not find reproduced in the text are Peirce's own deletions, page numbers, meaningless doodles, or later annotations by other persons. In the more than three hundred manuscript pages published in this volume, there are only two instances in which Peirce's intended revisions are unclear. The first appears in MS 181 at 18.21 and has been incorporated into the text. The second instance occurs in MS 237 and is somewhat more problematical. At 104.7, Peirce has caredt in the notation "copy A" in pencil. Two additional pages of the manuscript are clearly dated and labeled "A." This is his usual method for adding large blocks of material to an already inscribed manuscript. But a reading of the "A" material indicates that it is actually a rewrite of the final portion of the manuscript beyond the "copy A" insert, material he failed to delete. Since Peirce's usual method is not applicable here, we have chosen to reproduce the manuscript exactly as it stands. The "copy A" notation has been inserted at the designated place, but the material following it is the original inscription. And following that are the two additional pages dated and labeled "A."

One final point of editorial policy regarding the transcription and interpretation of manuscripts must be noted. As Peirce's placement of quotation marks (both single and double) when used in combination with a period, comma, colon, or semi-colon is rarely precise enough to make his intention clear, we interpret all such combinations by modern standard punctuation practices. We have applied this procedure to the amanuensis manuscripts as well.

Three sets of symbols have been adopted and incorporated into the text to reflect physical limitations of the manuscripts. Where pages are lost, three ellipsis points within italic brackets mark the absence. Where manuscripts are partially destroyed and words or portions of them are editorially reconstructed, the reconstructions appear in italic brackets also; to give the reader a hint as to manuscript content, we have supplied titles within italic brackets to papers that Peirce left untitled. The third symbol combination incorporated into the text is that of double and single slashes, which is used when Peirce left choices of words or phrases undecided. The double slashes

signal the beginning and ending of the undecided choice, and the single slash divides the two alternatives; the reading to the left is the original inscription, that to the right the interlined alternative. The reader will find yet another symbol, of sorts. Five blank spaces occur within lines of text in MSS 196, 200, and 215, all by amanuensis B, and the blanks in the printed text correspond to the blanks within the manuscripts, left there because the amanuensis either missed words in dictation or could not read Peirce's writing when making the fair copy. The manuscript blanks vary in length, and our printed text reflects and approximates the variation. Similar blanks occur in other amanuensis manuscripts, but Peirce filled those in or rewrote the entire passage during the process of revision.

Determining copy-text for Peirce's unpublished holograph manuscripts did not prove problematical. Because his working methods created variant texts which cannot be collated, each variant text theoretically stands as a separate item as would any other single-text item. On the advice of at least one of our contributing editors, that text was chosen for publication that seemed most carefully written and most fully developed and best argued. But we impose no limit on the number of variant texts we are willing to publish. The quality of the content is the only determining factor, and when a contributing editor thought more than one version of a given paper worth publishing, more than one is published. Though we may refer to other versions for helpful information, each version serves as its own copy-text, and no other version of the same paper is considered as having any textual authority for that one.

The same principle applies to the sixteen amanuensis manuscripts included in this volume. Whether Peirce dictated the content or handed a draft over for recopying, the amanuensis manuscripts reflect the same tendency to begin anew and to create variant texts. But we must here address the issue of authority. Theoretically, a document contains two levels of authority, that of the substantives—the words an author chooses to express his ideas—and that of the accidentals—the punctuation, capitalization, spelling, and paragraphing in which the words are embedded. Choice of copy-text is a function of the authority of the accidentals, for it rests on the editor's judgment about which document displays or lies closest to the author's intent in the structure of its accidentals. Study of all the amanuenses A and B manuscripts dated for the period of this volume, including copies made from extant holograph manuscripts, demon-

strates that neither amanuensis can be relied upon as an authority for Peirce's accidentals.<sup>5</sup> In only one case of the sixteen amanuensis manuscripts published here are there two drafts of a single text and thus a choice of copy-text. That choice is discussed below. The other fifteen are either single or variant texts, and the only document we have of each is the manuscript designated as copy-text. We apply the same editorial procedures to these amanuensis manuscripts that we use for Peirce's holograph manuscripts. But any discernible idiosyncratic punctuation or spelling practices will be those of amanuensis A or B rather than Peirce.

Peirce's published articles included in this volume pose few problems in determining copy-text. Excluding multiple-language publications, only the "Note on the Sensation of Color" was published more than once, and in six other publications there is related manuscript material. All others were printed only once, and the original publication serves as copy-text.

We now turn to those items for which there is more than one form of the text.

#### CHAP. 6TH (MS 218)

MS 218 is a ten-page manuscript written in the hand of amanuensis B. Although there are no blanks in the text, the last line contains the curious "]?[" notation that might be interpreted as a substitute. There is no evidence to indicate whether the source for this copy was an earlier Peirce draft or a dictation. But Peirce did look it over, and he made twenty-three pencil revisions; only two of these are changes in punctuation that relate directly to substantive changes. The revised copy was then handed to amanuensis A who made a new copy incorporating Peirce's revisions. The result is now designated as the eight-page MS 219. Even though it was meant to be a fairer copy, a collation of the two manuscripts indicates that MS 219 contains serious flaws. Amanuensis A could not always read Peirce's handwriting and in one instance left a blank where Peirce had made a revision. At 80.12–13, A's eye skipped in the 218 copy and a whole sentence was accidentally deleted. And he simply dropped the "]?[" notation

<sup>5</sup>At times even the authority of the substantives must be called into question. In MS 194 (at 30.31) we find amanuensis B writing "report" for "support," and in MS 211, not published here, we find amanuensis A very clearly inscribing "impidical" which we must assume is a misreading or, more likely, a mishearing of "empirical." In the same vein, we find amanuensis A writing "equipalent" for "equiparant" in MS 232 (97.19).

that Peirce apparently overlooked as well. There are thirty-three differences in accidentals between the two manuscripts, though one at 78.22 is the correction by A of a spelling error which neither Peirce nor amanuensis B caught. Of the eleven substantive differences (including the omitted sentence and the blank in MS 219), again two are A's corrections of Peirce's and B's oversights. Since neither manuscript can be said to be authoritative in its accidentals, it matters little which is used as copy-text. But we have chosen MS 218 on the grounds that it alone bears proof of Peirce's attention, and, if copied from a draft rather than dictated, it is the more likely to contain at best some of Peirce's original punctuation, capitalization, and spelling. The three corrections by amanuensis A are accepted as emendations to the text, though they too lack Peirce authority. A special listing of all variants between the two manuscripts is given following the full listing of emendations.

#### **"LAZELLE'S *ONE LAW IN NATURE*"**

Peirce's review of Lazelle's *One Law in Nature*, in *Nation* 17 (10 July 1873): 28-29, was published only once. MS 236 is a six-page holograph manuscript draft of the review, with the first page missing. Pages 2-7 are numbered by Peirce, and the second page has approximately one-quarter page blank, a space left for filling in the second of two quotes from Lazelle. The manuscript seems to be one draft prior to printer's copy. In fact, one explanation for the missing first page may be that Peirce considered it good enough to be sent along with a recopied set of pages 2-7. Collation of the manuscript against the *Nation* printing reveals twenty-one substantive and forty-one accidentals changes introduced into the printing. Of the forty-one accidentals changes, twelve can be attributed to alterations within the manuscript itself or to substantive variant readings between manuscript and printed version. Five are the expansion of ampersands to *ands*, changes Peirce himself would have made in writing out a final copy; four others can be identified as probable additional Peirce punctuation on the basis of similar punctuation practices in the manuscript. Of the remaining twenty, two are differences in paragraphing, a change Peirce is unlikely to have made, and the punctuation inversion at 165.35 is a change that we would normally have emended. Though MS 236 was not used as printer's copy for the *Nation* printing, Peirce did inscribe it with attention to detail and it does display his characteristic punctuation practices. We decided,

therefore, to use MS 236 as copy-text and to emend into the manuscript copy-text those twenty-one accidentals changes introduced into the *Nation* printing which can be determined with some accuracy to be authorial in nature. The *Nation* publication serves as copy-text for the material covered by the missing first manuscript page. No separate historical collation is given because all substantive changes introduced into the *Nation* publication have been accepted and emended into the manuscript copy-text with the exception of one at 166.31, and that is noted in the emendations list with a *stet* entry; it is a mistake that must be attributed to either typographical or editorial error.

#### “RAINFALL”

From 1868 until 1874, Peirce regularly supplied the publishers of the *Atlantic Almanac* with annual calendars and accompanying astronomical information. In a 15 July 1872 letter, the publishers informed Peirce that “We shall go to press with the *Almanac* as soon as we can get it ready & shall be pleased to have you furnish the same matter as last year, & can give you an additional page if you have anything noteworthy you would like to put in it.” Peirce accepted the offer, including the “additional page,” and on 17 September he responded, “I enclose herewith the calendars for the 1st 3 months of 1874. The others will be sent early next week & the introductory matter & paper on rainfall at an early date thereafter.” Peirce’s article on rainfall, bearing that simple title, appeared on page 65 of the 1874 *Almanac*. Putting the article together was probably not much of a chore for him, because he had already covered much of the information in a paper delivered before the Philosophical Society of Washington [DC] in December 1872, “On the Coincidence of the Geographical Distribution of Rainfall and of Illiteracy, as shown by the Statistical Maps of the Ninth Census Reports.”

There are no extant drafts of the “Rainfall” article as published in the *Almanac*, but there are two untitled manuscripts (MSS 206 and 225) identified as the paper read before the Philosophical Society of Washington that have an interesting relation to the article.

The first, MS 206, is a holograph thirteen-page manuscript, and the page numbers in the upper right-hand corners are Peirce’s. But it is fragmentary, as the page numbers show, and consists of pages 3–7 and 14–21, and there are nineteen revisions by someone other than Peirce. The missing pages pose no real mystery, however, be-

cause MS 225 is a seventeen-page fair copy of MS 206 in the hand of amanuensis B which incorporates all internal revisions appearing in MS 206 and includes text for the missing pages 8-13. The first two pages of MS 206 must have been discarded at an earlier date, because the third page equals page 1 in MS 225. Amanuensis B displays his usual helpfulness by regularizing Peirce's spelling and repunctuating the text and thereby incorporating over forty accidentals changes into the fair-copy portion of MS 225 for which we have matching Peirce material.

All of this would be of little interest or relevance but for the fact that the MS 206 missing pages 8-13 as reconstructed by MS 225 (pp. 6-10) appear practically verbatim as the last two paragraphs of "Rainfall," and the article up to that point is a rewritten version of MS 206, pp. 16-21 (MS 225, pp. 12-17). The rewritten portion, although containing the same basic information, is so thoroughly revised as to render collation useless, but a collation of pages 6-10 of MS 225 against the final two paragraphs of the article reveal fifty-three accidentals variants and twelve substantive variants, not counting the inclusion of thirty ditto marks in the published article which appear as blanks in the manuscript. The published article cannot be proven to be entirely authoritative in its accidentals, but evidence indicates that the amanuensis manuscript carries with it little or no authority at all. And it is possible that the missing pages of the holograph manuscript were sent to the publishers. We have, therefore, rejected the idea of using a split copy-text here, though we have not been able to ignore the manuscript completely. It is the source for a single substantive emendation at 172.38 that corrects a nonsensical reading in the copy-text. A special list is included in the apparatus giving all variants between pages 6-10 of MS 225 and the matching two paragraphs of the copy-text.

#### "ON THE APPLICATION OF LOGICAL ANALYSIS TO MULTIPLE ALGEBRA"

Peirce was in Europe from the spring of 1875 to the fall of 1876. According to the 11 May 1875 minutes of the American Academy, his paper "On the Application of Logical Analysis to Multiple Algebra" was read "by title" only; it was subsequently published in the Academy's *Proceedings* 10 (May 1874-May 1875): 392-94. As indicated in the first Editorial Note for this item (p. 519), this single publication may not contain the entire paper as originally written,

nor did Peirce see the results of it until after it was in print. But MS 273, three pages of a four-page holograph manuscript (page 3 is missing) is folded in thirds as if for mailing, and corresponds to the *Proceedings* publication. Collation of the extant portion of the manuscript indicates the introduction into the *Proceedings* printing of only two substantive alterations (the deletion of two zeros in the  $bh_6$  diagram) and thirty-six alterations in accidentals (excluding the systematic italicization of variables in the printing). Of these thirty-six variants, one is the removal of Peirce's initial paragraph indentation to correspond to publication house styling and eighteen are the addition of terminal points of punctuation, all of which are unnecessary and two of which result in misreadings. Since Peirce's claim not to have seen the paper till after publication must be taken at face value, there is no reason to believe he is responsible for the alterations in either substantives or accidentals which were incorporated into the *Proceedings* printing. On the other hand, it is probable that MS 273 was the copy sent to his father and that either it or a copy made from it by someone other than Peirce was passed on to the printer. The first two printed diagrams (for which we have no corresponding manuscript) contain all the appropriate zeros, the third contains two in the manuscript and none in the printing, and the fourth contains only blanks in both. There are, of course, any number of possible explanations for this shift from complete to incomplete diagrams, but the most likely solution is suggested by a 20 March 1873 letter to his father in which Peirce discusses a very similar issue and uses similar diagrams. The first few diagrams in the letter are complete, but eventually he begins to leave blanks where zeros would normally appear, explaining that "All blanks are zero's." Even though the lack of attention to details shows that the manuscript was not fully prepared for publication, the close substantives correspondence between the manuscript and printing, as well as the large number of accidentals variants, warrants the use of the manuscript as copy-text. The *Proceedings* printing is copy-text in place of the missing manuscript page. Knowing that Peirce did not see the final paper till after publication, we have refused to emend into the manuscript copy-text the two substantive changes which occur in the *Proceedings* publication. But the *Proceedings* is cited as the source of seven emendations of accidentals which we would normally have made and, in addition, we have removed twelve points of terminal punctuation from the *Proceedings* portion of copy-text in order to make it conform to

Peirce's practice as demonstrated in the manuscript. Since Peirce did not fully prepare the manuscript for publication, we have accepted the italicization of all variables as they appear in the printed version, a practice of which Peirce was aware but did not follow with consistency in the manuscript. We do not list each instance of italicization. A Historical Collation is not included here since the two rejected substantive variants appear in the Emendations with the notation *stet*.

#### "NOTE ON THE SENSATION OF COLOR"

This article was first published in *American Journal of Science and Arts*, 3d ser. 13 (April 1877): 247-51. A few months later it was republished in the supplement to *London, Edinburgh, and Dublin Philosophical Magazine and Journal of Science*, 5th ser. 3 (1877): 543-47. At the end of the PM publication, the source note given is "Silliman's *American Journal*, April 1877." Collation indicates that the PM publication is derived from a printed copy of AJ, after having gone through the hands of a PM house editor. Of the ten substantive variants which are introduced into the PM printing, one (212.12) corrects an AJ nonsensical reading, three others (215.18, 216.17, 216.18-22) are made as accommodations to the PM republication, two are errors (one by the printer at 214.31; the other at 212.12, possibly by the editor), and the final four can best be classified as typical editorial "improvements." In contrast to just ten substantive alterations, the PM publication contains eighty-one changes in accidentals. Forty-three of these are punctuation changes, and in the majority of the remainder British spelling forms are substituted for American. Given the fact that we lack historical evidence for and that the internal evidence speaks against Peirce's intervention in the republication of his article, the AJ publication must be used as copy-text, and only one substantive correction from the PM printing is accepted as an emendation to the copy-text. The PM printing is also cited as the source of four emendations of accidentals which we would normally have made. A Historical Collation list is included in the apparatus giving all variants between the two publications.

#### "THE FIXATION OF BELIEF"

The first of the six papers of the "Illustrations of the Logic of Science" series appeared in print for the first and only time in English in the November 1877 issue of the *Popular Science*

*Monthly.* There are two fragmentary, holograph manuscripts directly related to two separate sections of this article. The first is MS 189, dated May–June 1872, and bears Peirce's own title, "Chapter 3, Four Methods of Settling Opinion." Though it is unquestionably a manuscript written for the 1872–73 projected logic book, it must also be considered a draft of section V of the 1877 publication. A collation of the extant portions of the manuscript against the publication reveals a total of seventeen substantive variants, and eighty-eight variants in accidentals. And even though the manuscript contains at least two layers of revision, one in the original ink in which the entire manuscript is inscribed and another in pencil, we decided to publish both items in their respective chronological places and give each copy-text status.

A second, MS 190, is a two-page fragment, beginning and ending in mid-sentence, corresponding to approximately three paragraphs of section I of the publication. It too is a part of the 1872–73 logic book series, but because of its very fragmentary nature we have declined to publish it. But like MS 189, it must also be considered a draft of a section of the published article. It is a carefully written manuscript with ten internal revisions in the original inscription medium, and a collation of manuscript against publication reveals eleven substantive and twenty-five accidentals variants, only one of which can be attributed to a substantive alteration in the manuscript. Because of its fragmentariness and its five-year predating of the published piece, we have designated it a pre-copy-text form. The reader may recover the manuscript readings in the Historical Collation.

#### "FERRERO'S *ESPOSIZIONE DEL METODO DEI MINIMI QUADRATI*"

The only publication of this review is in *American Journal of Mathematics* 1 (1878): 59–63. MS 315, dated fall 1877, is Peirce's two-page, untitled draft of not quite the first two full paragraphs of the printed version. The two pages are complete in themselves and, in fact, there is enough space at the bottom of the second page for three more lines of text. Collation of the manuscript against the corresponding portion of the AJM printing indicates twenty-two substantive variants and only ten variants in accidentals which are neither contained within nor directly attributable to any substantive alteration. The draft is rough, with many internal revisions, and the substantive variants between it and the printed version are dramatic

compared to the few accidentals variants. It is probably at least two stages prior to printer's copy. It is, therefore, designated a pre-copy-text form, the AJM printing serving as copy-text. The variants between the manuscript and the corresponding portion of the AJM copy-text are listed in the Historical Collation.

**"ON THE INFLUENCE OF THE FLEXIBILITY OF THE SUPPORT ON THE OSCILLATION OF A PENDULUM"**

Though there is no question of using the 1881 *Coast Survey Report* publication as copy-text for this item, there are several related documents in both printed and manuscript form which deserve discussion here. P 102 is a handwritten lithograph of Peirce's report, in French, distributed in advance of the 1877 conference of the International Geodetical Association. A lithographed letter of transmission from Emile Plantamour to General Ibañez, dated 27 August 1877, precedes the report which itself appears in letter form, Peirce to Plantamour, 13 July 1877. The Peirce Papers contain Peirce's own copy of this lithograph (MS 312), without markings or annotations, as well as two holograph manuscripts, MSS 308 and 309. MS 309 contains forty pages of fragmentary drafts, in French, for the original report. MS 308 consists of two fragmentary pages in English, though neither page coincides with text in the 1882 English translation nor do these pages appear in translation anywhere in the French version. It is of some interest to note that in these holograph manuscripts Peirce has consistently used the symbol  $\eth$  rather than its more usual counterpart  $\pi$ , and this same symbol appears throughout the lithograph.

Apparently some confusion arose, either in Peirce's memory or in the files of the Coast Survey Office in Washington, because we find Peirce writing to C. S. Patterson on 6 September 1877, "Yours of the 29th went astray so that I did not receive it until this morning. I will send you tomorrow a memorandum of the reasons for sending me to Stuttgart and Berlin. Meantime permit me to explain that the full account which you desire of the details of my flexure investigation has already passed through your hands & has been sent out for publication." And in a postscript to the same letter, "I have a long letter to you now in the hands of a clerk & shall write another tomorrow in order to supply you with all you ask." There is indeed a nine-page amanuensis letter (neither amanuensis A nor B) of the same date giving justifications for Peirce's attendance at the conference. A third

letter, thirteen pages holograph, also dated 6 September, begins, "You ask me to send you 'the whole paper proposed to be submitted' to the International Geodetical Union. You have already had the paper submitted to you, and I think I shall answer your desire now best by sending a full abstract of it, as I cannot get the whole thing copied inside of several days." The rest of the letter contains an elaborate abstract of the first part of the French paper, and Peirce near the end states, "I can write no more of this today but will send the rest so you can have it by Monday morning." Two days later, 8 September, in another letter to Patterson, this one a three-page cover letter for a fifteen-page amanuensis copy (the same hand of two days before) of the last half of the report, Peirce says, "I send you herewith the remainder of my account of the paper which has already been forwarded to the International Geodetical Union. I have made it somewhat more detailed than in the original memoir." Of the entire two-part report, Peirce's "more detailed" abstract, only the last half follows the organization of the original French version. Though much of the data and many of the mathematical formulae are the same, and the  $\eth$  symbol appears throughout both parts, it cannot be said to be a true English translation.<sup>6</sup>

The proceedings of the 1877 conference were published in Berlin the following year. Peirce's report appears, again in French, on pp. 172–87 preceded by the same Plantamour to Ibañez letter of transmission that was also in the lithograph. A collation of the printed proceedings against the lithograph indicates very few changes between the two, none of which can be attributed to authorial intervention, and the most noticeable of which is the systematic substitution of  $\pi$  for  $\eth$ .

Given the technical nature of this report, we decided to publish only Peirce's own English translation ("Translated into English by the Author" appears directly beneath the title in the *Coast Survey* printing). And it is indeed a close translation of the French version, except for the 1882 substitution of formulae clearly noted in the body of the *Coast Survey* printing and the appended footnotes.

True collations cannot, of course, be performed between the French and English versions, but a comparison of the data and mathematical formulae indicates that the *Coast Survey* printing is a poor

<sup>6</sup>The entire Peirce-Patterson exchange is deposited in the National Archives, Record Group 23.

one. By comparing the two French versions, the two-part abstract sent to Patterson, and MS 309, we were able to locate poor typesetting of math and, in some cases, to trace corruptions in the successive printings of the data. We have emended the *Coast Survey* copy-text printing when we could determine that errors were typographical errors, but we do not identify which related document(s) led to such a determination. On the other hand, we have allowed one inconsistency to stand as it appears in the copy-text, namely the use of both  $\pi$  and  $\odot$ , which may be due to a lapse of attention on Peirce's part in writing out his translation or to the fact that he had both the lithograph and the published proceedings before him at the time.

We mentioned earlier that because of the two types of materials included in this volume, namely previously published and hitherto unpublished manuscript items, a variety of editorial approaches are required. In the case of published items, the editing focuses on the effort to locate and eliminate editorial or printing corruptions and thereby to restore Peirce's original intentions. In the case of holograph manuscript items, the focus is on reproducing Peirce's written texts as faithfully 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 quite confusing or vague. In such cases we intervene by emendation only when we are certain of his intent. When such certainty is lacking, his eccentricities and even anomalies are allowed to stand as he has written them. The following is a summary of the guidelines used in emending Peirce's texts.

We correct only misspellings, slips of the pen, and typographical errors, but do not attempt to regularize Peirce's variant spelling. We retain acceptable nineteenth-century spelling forms as well as acceptable variant spellings of proper names. Our standard reference for determining acceptability of spelling variants is the *Oxford English Dictionary*.

We try to verify all of Peirce's quotations, and to identify and verify quotations that are not marked as such in the manuscripts, in editions he is known to have owned or had available. A quotation is allowed to stand as he gives it, even if it differs from the original. In those cases where Peirce's own oversight or a printer's error result in nonsensical readings, the text is emended, and the entry in the list

of emendations includes a reference to the Editorial Notes, where all information on his quotations and their sources may be found.

No attempt is made to modernize or regularize Peirce's bibliographic citations, but they are corrected when erroneous or unintelligible, and appropriate punctuation is added. When, in referring to his own published writings, Peirce gives specific page or volume numbers, we replace these 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.

To reduce the great variety of different journals' house publishing styles and to make understandable Peirce's obscure references, we emend all title notations to conform to the modern practice of italicizing book titles, placing chapter titles in quotation marks, and so on; we also restyle by emendation lengthy published quotations to conform to our own editorial style for printing extracts.

Obscure or ambiguous abbreviations are expanded by emendation. All other abbreviations are left as Peirce gives them with periods added where appropriate. Periods are also added at the end of sentences where Peirce's (or the amanuenses') pen skipped or he inadvertently omitted them. A dash, parenthesis, or quotation mark is added when it is the missing half of a pair, and apostrophes are inserted in possessives and contractions where Peirce carelessly omitted them.

Peirce often wrote notes to himself as reminders to include or expand on certain points, and he usually enclosed such notes in brackets. In the few cases where he failed to do this, we do it for him.

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 we return to his original word or phrase, citing each occurrence of it in the Emendations. And when he added introductory clauses to already inscribed sentences but failed to lowercase the first word of the original sentence, we emend his misplaced capitalization.

Peirce meticulously collected offprints of his published papers and, as he tended to correct and annotate them, they provide an important source of emendation. His corrections of typographical

errors are emended into the text, and annotations are quoted in diplomatic form in the editorial notes.<sup>7</sup> Further sources for emendation of published papers include published errata lists as well as letters or reports in which Peirce mentions miscalculations, oversights, or printer's errors.

Mathematical recalculations revealed that many of the published data in Peirce's technical scientific papers are inaccurate. Only in cases where we have documentary evidence to indicate which numbers are in error (as in P 253) do we emend the text.

All emendations to the texts described above are listed, and the sources for each are cited in the Emendations. Though we try to avoid silent changes of the text, a few of these do occur and they are mainly concerned with the mechanical presentation of the printed material.

MS 227, "Improvement in the Classification of Vids," a letterpress copy of a holograph manuscript, displays a curious notation in which all subscripts are inscribed directly below each letter. But in the *Linear Associative Algebra* itself, upon which the manuscript is based, these subscripts are printed in the usual angled position. We have silently adopted the LAA printing method.

Peirce regularly used the nineteenth-century calligraphic convention of superscribing and underlining portions of abbreviations such as M<sup>1</sup> or 1<sup>st</sup>. For convenience in printing we have placed such superscriptions on line and have ignored the double underlining.

Several of Peirce's previously published scientific papers contain long tables, with repeated column headings on successive pages. We have moved these headings to accommodate our 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 ditto marks are used in place of text when page breaks in this edition differ from the original printing.

We mentioned earlier that, in transcribing Peirce's manuscripts, we have modernized the placement of points of punctuation in relation to single and double quotation marks. In order to eliminate

<sup>7</sup>Those offprints in which Peirce reworked his text for incorporation into later, larger works—specifically the 1893 "Search for a Method," the 1894 "How to Reason," and the 1909 planned collection of his essays on pragmatism—will be published in their revised state in later volumes of this edition.

conflicting publishers' styling, we have similarly modernized such punctuation in his previously published papers.

All previously published titles are printed in italics, and unpublished manuscript titles in roman type. Also 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. 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, modern standard form is followed. Various printer's conventions for first lines of text or paragraph openings have been dropped and, unless the material calls for special treatment, all papers begin with our usual paragraph indentation.

# Explanation of Symbols

## *Within the Text*

Titles supplied by the editors appear in italic brackets.

Ellipsis points within italicized brackets indicate the loss of at least one full manuscript page.

Italicized brackets surrounding only a few words or parts of words indicate an editorial reconstruction of a damaged portion of manuscript.

Sets of slashes are used to signal Peirce's unresolved alternative readings. The double slashes mark the beginning and ending of such a reading, while the single slash divides the two alternatives.

Blank spaces in the text represent corresponding spaces in the amanuensis manuscripts. Their length approximates that of the original.

## *Within the Apparatus*

All page and line numbers refer to the present edition with each line of text, excluding running heads, counted. Footnotes are lined separately from the text and are indicated by an *n* following the page number. Number(s) within parentheses following a line number indicate the first, second, or third appearance of the key word within that line. The use of *Also* following a complete entry indicates the same emendation occurs at the listed places within that selection.

*Et seq.* following a page and line number indicates that all subsequent readings within the particular selection agree with the recorded reading.

Page and line numbers preceded by an asterisk (\*) indicate a reference to a textual note in which that particular reading is discussed more fully.

A double dagger (‡) preceding a page and line number alerts the reader to the presence of an editorial note explaining a change in a direct quotation.

All readings to the left of the square bracket are from the present edition; in the Emendations list, they also correspond to the emended readings. When necessary, a source symbol and semicolon follow the bracket. The readings to the right of the bracket, or semicolon, are the rejected readings from the identified copy-text. The source for all readings is given in the headnote for each selection.

*Stet* immediately following the bracket signals the retention of a copy-text reading in cases where one would normally expect to find the copy-text emended.

*Not present* signifies a lack of corresponding text in a designated authoritative source.

The use of the abbreviations *ital.* or *rom.* to the right of the square bracket indicates that the listing to the left of the bracket was originally printed in italic (in the case of manuscripts, underlined) or roman (not underlined in the manuscripts) type.

The term *reinstate* to the right of the bracket applies to manuscript material only and signifies that the reading to the left of the bracket was deleted by Peirce in the original but is reinstated by the editors.

Two common conventions are used here which pertain to punctuation and paragraphing changes only, the curved dash (~) and inferior caret („). The caret signals the absence of punctuation, and the curved dash is used in emendations of punctuation to indicate repetition to the right of the bracket of the word which appears to the left of the bracket.

A vertical stroke (|) indicates a line-end break and is used in cases where it helps to clarify an emendation.

## Textual Notes

- 9.15 In this case, the vertical stroke represents not only a line break, but a page break as well.
- 18.21 In the manuscript, the phrase “Reading Advertizer” is interlined above the word *adopted*, though Peirce neglected to indicate its placement with a caret.
- 19.3 The vertical stroke represents a page break as well as a line break. Though it may appear that a page of the manuscript is missing, the editors have concluded this is not the case.
- 104.7 Peirce clearly interlined the “copy A” notation with a caret here, but rather than inserting the additional two pages of manuscript labeled “A,” we have published them following the end of his originally inscribed text. See the Essay on Editorial Method, p. 561, for a more detailed explanation.
- 165.1–3 At this point, the manuscript copy-text has one-quarter page blank, a space Peirce left for filling in the citation at a later date. We have emended the copy-text here, using the *Nation* reading, but as the next three emendations show, we have editorially altered the adopted *Nation* reading.
- 389.21 The 1878 printing of *Photometric Researches* contains five unnumbered plates appended to the text. In one instance (482.10) Peirce speaks of a plate by number, but in all other instances his references to the plates are very general. The editors have rearranged the order of the plates, added one (see Textual Note 407.33), and given each a number. The text here and at 406.23, 407.33, 482.1, 482.10 is emended for the convenience of the reader in allowing him to locate the proper plate.
- 407.33 This plate does not appear in the 1878 publication, but it is contained in MS 302 of the Peirce Papers. It consists of one printed page, measuring approximately seven by ten inches, which Peirce may have had set in type as early as 1872 to be used as a working tool.
- 458.27 and 460.8 In the original printing, asterisks are used for footnote notation in the text. At 458.27 and 460.8 an asterisk is appended to stars numbered 169 and 206, and a single footnote is given for both. Since it is our policy to number footnotes consecutively, we have duplicated the footnote and given each a separate number.
- 473.9 Lacking an 8 April 1876 reading for the star  $\pi$  Cassiepeae, the editors cannot emend this oversight in the text.

# Emendations

## *Educational Text-Books*

Copy-text is the publication of this article in the *Nation* 14 (11 April 1872): 244–46. All emendations are supplied by the editors.

- |         |   |
|---------|---|
| 1.1     | <i>Text-Books, II]</i> ~-~. ~   |
| 1.4     | „ <i>Star-Atlas</i> ,“ “Star-Atlas”   |
| 1.29    | „ <i>Uranometria</i> ,“ “Uranometria”   |
| 2.15    | Elijah Burritt’s] Elihu Burritt’s   |
| 2.18    | „ <i>Theory of Heat</i> ,“ “Theory of Heat”   |
| 2.35–36 | „ <i>Lectures on the Psychology . . . Human</i> ,“ “Lectures on the Psychology . . . Human” |
| 3.8     | „ <i>Deductive Logic</i> ,“ “Deductive Logic.”  |
| 3.12    | „ <i>Deductive Logic</i> ,“ “Deductive Logic,”  |
| 3.22    | Ockham] Oldham  |
| 3.25    | Rudolf Agricola] George Agricola  |
| 3.28    | „ <i>Novum Organum</i> ,“ “Novum Organum,”  |
| 3.29    | „ <i>Conduct of the Understanding</i> ,“ “Conduct of the Understanding,”                    |
| 4.13    | Thomson] Thompson   |
| 4.18    | „ <i>Elementary Lessons</i> ,“ “Elementary Lessons”   |
| 4.20    | „ <i>Deductive Logic</i> ,“ “Deductive Logic”   |
| 6.14    | Boole] oole   |
| 6.30    | „ <i>Science of Thought</i> ,“ “Science of Thought”   |
| 6.39    | „ <i>System of Logic . . . Doctrines</i> ,“ “System of Logic . . . Doctrines,”              |
| 7.24    | „ <i>The Rival Collection</i> ,“ “The Rival Collection”                                     |

## *Lecture on Practical Logic*

Copy-text for this item is MS 191. All emendations are supplied by the editors.

- 8.4 settlement<sub>u</sub>] ~.  
 8.8 discussion<sub>u</sub>] ~<sub>u</sub>  
 8.9 agreement<sub>u</sub>] ~;  
 8.12 this] This  
 8.15 or] *reinstate*  
 8.20 opinions] opinions  
 9.7 reference] refence  
 9.8 taking] take  
 \*9.15 true, true] ~<sub>u</sub>|~  
 9.21 extent.] ~<sub>u</sub>

### *Third Lecture*

Copy-text for “Third Lecture” is MS 192. The single emendation is supplied by the editors.

- 10.22 looking-glass] ~<sub>u</sub>~

### *Logic, Truth, and Settlement of Opinion*

Copy-text for this item is MS 179. All emendations are supplied by the editors.

- 14.11(3) the] The  
 14.23(1) we] We  
 14.28 believed] beleived  
 14.30 non-existent] nonexistent  
 14.31 an] An  
 15.14 succeeds] suceeds  
 15.14 true<sub>u</sub>] ~,  
 15.18 and] and and  
 15.19 desirable] desireable  
 15.19(2) to] To  
 16.18 Bryant] Pope

### *Investigation and Settlement of Opinion*

Copy-text for this item is MS 180. All emendations are supplied by the editors.

- 17.2 first<sub>u</sub>] ~<sub>u</sub>  
 17.4 one's] ones  
 17.23 paleontology] paleontogy  
 17.34 no] no no  
 17.36 choose] chose

- 17.37 appropriate] appropriate  
18.9 we] which  
18.12 point.] ~<sub>λ</sub>

***Chapter 1 (MS 181)***

Copy-text for “Chapter 1” is MS 181. All emendations are supplied by the editors.

- 18.18 one's] ones  
\*18.21 [Reading *Advertiser.*] ~<sub>λ</sub>Reading Advertiser<sup>..</sup>  
18.22 [Ostrich.] ~<sup>..</sup>  
\*19.3 influences will] in-will  
19.19 other's] others  
19.23 in] is  
19.25 situation,] ~<sup>..</sup>  
19.25 conditions,] ~<sub>λ</sub>  
19.33 investigation] invesigation  
19.35 parts:] ~<sub>λ</sub>

***Chapter 1 (MS 182)***

Copy-text for this item is MS 182. There are no emendations to the copy-text.

***Chapter 1 (MS 183)***

Copy-text for this abstract of “Chapter 1” is MS 183. All emendations are supplied by the editors.

- 21.9 Enlarged] Enlarge  
21.13 a] as  
21.16 important] impotant  
21.23 cases;] ~,  
21.23 matter,] ~<sup>..</sup>  
21.26 all. Belief ] ~<sub>λ</sub>belief

***Chapter 1. Doubt and Belief***

Copy-text for this item is MS 187. The single emendation is supplied by the editors.

- 22.1 Chapter] Logic—Chapter

*Chapter 2. Of Inquiry*

MS 188 serves as copy-text for “Chapter 2. Of Inquiry.” The emendations are supplied by the editors.

- |          |                        |
|----------|------------------------|
| 23.4     | Chapter] Logic—Chapter |
| 23.15(2) | the] The               |
| 23.21    | does not] does         |
| 23.24    | a very] very           |

*Chapter 3. Four Methods of Settling Opinion*

Copy-text for “Chapter 3” is MS 189. All emendations are supplied by the editors.

- |          |  |
|----------|--|
| 24.7     | Chapter] Logic—Chapter                     |
| 24.22-23 | “I . . . wholesome.”] ‘I . . . wholesome’. |
| 25.27    | other’s] others                            |
| 25.33    | same time] time                            |
| 26.20(1) | depend,] ~ <sup>^</sup>                    |
| 26.30-31 | immeasurable] immeasureable                |
| 27.31    | realities,] ~ <sup>^</sup>                 |
| 27.32    | did,] ~ <sup>^</sup>                       |

*On Reality (MS 194)*

Copy-text for this item is MS 194. The emendations are supplied by the editors.

- |       |  |
|-------|--|
| 29.18 | an] a                                      |
| 29.34 | standpoints] stand-points                  |
| 30.20 | meaning] meaing                            |
| 30.31 | support] report                            |
| 30.37 | circumstances] circum <sub>^</sub> stances |
| 32.13 | acquiring] aquiring                        |

*Chapt. 4 (MS 195)*

Copy-text for this version of chapter 4 is MS 195. All emendations are supplied by the editors.

- |       |                           |
|-------|---------------------------|
| 32.15 | Chapt.] ~ <sup>^</sup>    |
| 33.3  | intelligible] inteligible |

33.5	retina] ritina
33.13–14	sensitive] sensative
33.15	without] withought
33.21	necessary] neces <sub>^</sub> sary
33.23	is not] not
33.28	judgment] judgment
33.40	because] becaus
34.29	phenomena] phenomina

*Chap. 4 (MS 196)*

Copy-text for this “Chap. 4” is MS 196. The emendations are supplied by the editors.

35.3	Chap.] LOGIC Chap <sub>^</sub>
35.19	presupposes] presupposes <i>Also</i> 37.6
36.10	which] for which
36.17	such] Such
37.7	suppose] suppose
37.10	analyzed] analized
37.14	unanalyzed] unanalyzerd

*On Reality (MS 197)*

Copy-text for this item is MS 197. Emendations are supplied by the editors.

37.24	This] The
37.26	information,] ~ <sub>^</sub>

*On Reality (MS 198)*

MS 198 serves as copy-text for this version of “On Reality.” The emendations are supplied by the editors.

38.22	constitute] constitute
38.31	complex] incomplex
39.12	arises.] ~ <sub>^</sub> :
39.16	if it is] it is
39.31	nothing] nothig
39.33	philosophically,] ~ <sub>^</sub>

*Chap. 4. Of Reality*

Copy-text for “Chap. 4. Of Reality” is MS 200. All emendations are supplied by the editors.

40.1	Chap.] LOGIC Chap.
40.1	4. <sub>~</sub> ] ~.—
40.7	beforehand,] before <sub>~</sub>  hand, <sub>~</sub>
40.7	but <sub>~</sub> ] ~,
40.16	<i>excellence.</i> ] ~ <sub>~</sub>
40.18	us,] ~ <sub>~</sub>
41.30	are] are are
41.34	observation] Observation
42.7	ask] Ask
42.10	judgment] judgment
42.33	other's] others
42.40	no] No
43.6	conclusion] con <sub>~</sub>  clusion
43.32	how] How
44.22	among] from
45.19	metaphysicians. While] ~ <sub>~</sub> while
45.33	the truth] the truth the truth
46.3-4	<i>Gulliver's Travels</i> ] rom.
46.20	my] my my
46.36	they] the
47.2	affected by] by
47.8	there are] other
47.17	unity] unit

### *Of Reality*

Copy-text for this item is MS 203. All emendations are supplied by the editors.

47.25	beforehand] before hand
48.3	for the element] for the element which the element
48.7	ones] one's
48.10	an] a
48.12	otherwise] Otherwise
48.27-28	judgments. ¶It] judgments. But ¶It
49.30	a] A
49.34	think] thinks
50.17	proposition.] ~,
50.23	self-contradictory] ~ <sub>~</sub> ~
51.2	sensitive] senitive
51.21	proclaiming] proclaiming
51.28	to be] to
52.27	together] to-gether
53.16-17	self-existence] ~ <sub>~</sub> ~

***Chapter IV. Of Reality (MS 204)***

Copy-text for this version of “Chapter IV” is MS 204. All emendations are supplied by the editors.

54.19	Chapter] Logic—Chapter
55.38	phrase,] ~^
56.7	be,] ~..
57.2	that] that that
57.18	another's] another
57.26	itself ] itself is
58.23	it, it] ~^ ~
58.31	emergence,] ~^
58.33	preferred] preferred
59.7	existence of ] existence
59.13	accelerations] accelerations
59.22	identical,] ~^
59.25	observation] we observation

***Chapter IV. Of Reality (MS 205)***

MS 205 serves as copy-text for this item. The emendations are supplied by the editors.

60.1	Chapter] Logic—Chapter
61.4	nothing.] ~^

***The list of Categories***

MS 207 serves as copy-text for “The list of Categories.” The only emendation is supplied by the editors.

61.5	Chapter _____. The] Logic Chapter _____. [The
------	---

***On Representations (MS 212)***

Copy-text for this version of “On Representations” is MS 212. All emendations are supplied by the editors.

62.12	connection with] connection
62.20	every] Every

62.27	it.] ~ <sup>^</sup>
62.29	sensation.] ~ <sup>^</sup>
63.1	another.] ~ <sup>^</sup>
63.2(1)	that] That
63.3	clear.] ~ <sup>^</sup>
63.4	itself.] ~ <sup>^</sup>
63.12	ago.] ~ <sup>^</sup>
63.16	same.] ~ <sup>^</sup>
63.20	mind).] ~ <sup>^</sup> ) <sub>λ</sub>
63.38	representation] rep <sup>n</sup> <i>Also</i> 63.39(1,2), 63.40, 64.1, 64.2, 64.5, 64.6, 64.8(1,2), 64.9-10, 64.12, 64.15-16, 64.16, 64.17, 64.18-19, 64.20, 64.21
63.39-40	represented] rep <sup>d</sup> <i>Also</i> 64.16, 64.20(1,2)
63.40	representing] rep <sup>g</sup> <i>Also</i> 64.4
64.3	one.] ~ <sup>^</sup>
64.7	way. Or] ~ <sup>^</sup> or
64.10	represents] rep <sup>ts</sup> <i>Also</i> 64.19
64.10	represents.] rep <sup>ts</sup> <sub>λ</sub>
64.16	object—] ~ <sup>^</sup>
64.17	represent] rep <sup>t</sup>
64.17	object,] ~ <sup>^</sup>
64.21	B, to] ~ <sup>^</sup> . To
64.22	'mortal'] ' ~ <sup>^</sup>
64.24	object.] ~ <sup>^</sup>
64.25	qualities. For] ~ <sup>^</sup> for
64.28	nothing. It] ~ <sup>^</sup> it
64.34	action, it] ~ <sup>^</sup> . It
64.35	complete. The] ~ <sup>^</sup> the
64.35	is] as is
64.35	an] a
64.40	&c.] ~ <sup>^</sup>
65.1	names. 'Man'] ~ <sup>^</sup> 'man'
65.4	used. We] ~ <sup>^</sup> we
65.7	picture. The] ~ <sup>^</sup> the
65.8	nature.] ~ <sup>^</sup>
65.14	resemblance. If] ~ <sup>—</sup> if

*On Representations (MS 213)*

MS 213 serves as copy-text for this version of "On Representations." There are no emendations to the copy-text.

*On the nature of signs*

Copy-text for this item is MS 214. All emendations are supplied by the editors.

67.1	not. Thus] ~ <sup>thus</sup>
67.5	be,] ~ <sup>^</sup>
67.14	him.] ~ <sup>^</sup>
67.16	painter's] painters <i>Also</i> 67.20
67.20	mind.] ~ <sup>^</sup>
67.31	one's] ones
67.33	application.] ~ <sup>^</sup>
67.38	application.] ~ <sup>^</sup>
67.40	thing.] ~ <sup>^</sup>
68.2	object.] ~ <sup>^</sup>
68.3	thinking.] ~ <sup>^</sup>
68.14	facts.] ~ <sup>^</sup>
68.22	has] is
68.23	complete. Nevertheless] ~ <sup>nevertheless</sup>

***Time and Thought (MS 215)***

Copy-text for “Time and Thought” is MS 215. All emendations are supplied by the editors.

69.4	discrete] discreet <i>Also</i> 69.10, 69.13
69.25	other] other is
69.40–41	abandoned,] ~.
69.41	it] It
69.41	presupposed] presupposed
70.5	matter <sup>^</sup> ,because] ~. Because
70.36	defined] defind

***Time and Thought (MS 216)***

MS 216 serves as copy-text for this version of “On Time and Thought.” The emendations are supplied by the editors.

73.26	supposes] suposes
73.37	minute] minuet
74.5	takes] take
74.30	thing] thin

***Chap. 5th***

Copy-text for this item is MS 217. The emendations are supplied by the editors.

- 75.8           Chap.] ~  
 75.10          March 10] *Logic* March 10  
 76.32          symptom] symptom  
 76.38          poisonous] poisonus *Also* 76.40

### *Chap. 6th*

MS 218 (written in the hand of amanuensis B) serves as copy-text for this item. Emendations to the copy-text are from two sources. Those labeled E are supplied by the editors. Those labeled 219 cite the appearance of corrections the editors normally would have made on their own authority but which are derived from MS 219 (a direct copy of MS 218, written in the hand of amanuensis A). For a further discussion of the editorial handling of this item, see the Essay on Editorial Method. The reader is also directed to the MS 219 List of Variants for a complete listing of variants between the two manuscripts.

- 77.25          *March 10*] E; *Logic March 10*  
 78.12          think it] 219; think  
 78.22          every] 219; evry  
 79.4           a mind] 219; a a mind  
 80.6           The “is”] E; The the “is”

### *Memorandum*

Copy-text for this item is MS 220. The emendations are supplied by the editors.

- 81.20          Memorandum: Probable] ~.|~  
 81.23          Chap.] ~<sub>^</sub>

### *Chap. 7. Logic as a Study of Signs*

Copy-text for “Chap. 7” is MS 221. The emendations are supplied by the editors.

- 82.7           Chap.] *Logic Chap.*  
 83.17          is] in  
 83.36-37       investigate.] ~<sub>^</sub>

*Chap. 9th*

MS 223 serves as copy-text for “Chap. 9th.” All emendations are supplied by the editors.

84.12	Chap.] <i>Logic Chap.</i> ~
84.25	individual] idividual
85.37	<i>dictum de omni</i> ] <i>dictum diomni</i> Also 86.11–12, 86.14, 86.17
86.6	etc.] ~ ^
86.6	<i>b,</i> ] ~ ^
86.7	in] an
86.25	and] and and
86.36	all. It] ~, it
87.17	<i>a,b</i> ] <i>a,b</i> +
87.26	and] an
87.28	that <i>b</i> is] that is
87.38	changing] changeing

*Chap. VIII. Of the Copula*

Copy-text for this item is MS 229. The emendations are supplied by the editors.

90.1	Chap.] ~ ^
90.7	expresses] express
90.17	“is,”] “~ ^”
91.23	<i>containing.</i> ] ~ ^
91.28	b.] ~,

*Chap. IX. Of relative terms*

Copy-text for “Chap. IX” is MS 230. All emendations are supplied by the editors.

93.1	Chap. IX.] ~ ^ ~ ^
93.8	term,] ~ ^
93.9	with _____,”] ~ _____ ^ ”
93.11	woman’.] ~ ^
94.14	is,] ~ ^
94.22	Negro] negro Also 94.23
94.31	there] the there
95.12	else.] ~ ^

*Chap. X. Copula and Simple Syllogism*

MS 232 serves as copy-text for this item. Emendations are supplied by the editors.

95.17	sign.] ~ <sup>^</sup>
95.21-22	judgment.] ~ <sup>^</sup>
96.22	child'] ~ <sup>^</sup>
96.22	'B'.] '~~' ^
96.28	die'.] ~ <sup>^</sup>
96.29	priests <sup>^</sup> .] ~ <sup>,</sup>
96.31	read. <sup>^</sup> .] ~ <sup>,</sup>
96.35-36	thunders'.] ~ <sup>^</sup>
96.38	conceive] concieve
97.7	'whatever] ~~
97.8	happens'] ~ <sup>^</sup>
97.11	happens'.] ~ <sup>^</sup> ..
97.17	&c.] ~ <sup>^</sup>
97.19	equiparant] equipalent
98.4	B. <sup>^</sup> .] ~ <sup>.</sup>
98.5	B. <sup>^</sup> .] ~ <sup>^</sup>

*Chap. XI. Logical Breadth and Depth*

Copy-text for this item is MS 233. All emendations are supplied by the editors.

98.7	XI.] ~ <sup>^</sup>
98.11	things—.] ~ <sup>^</sup>
98.12	not—.] ~ <sup>^</sup>
98.13	signs.] ~ <sup>^</sup>
98.17	class.] ~ <sup>^</sup>
98.19	conceived.] ~ <sup>^</sup>
98.20	conception is] conception as it is
99.1	predicated. The] ~ <sup>^</sup> the
99.2	it.] ~ <sup>^</sup>
99.3	objects—.] ~ <sup>^</sup>
99.3	things—.] ~ <sup>^</sup>
99.10	them. Then] ~ <sup>^</sup> then
99.13	abstract.] ~ <sup>^</sup>
99.14	&c.] ~ <sup>^</sup>
99.16	object's] objects
99.17	whiteness.] ~ <sup>^</sup>
99.21	white.] ~ <sup>^</sup>
99.36	thought. But] ~ <sup>^</sup> but

100.15	S''s] S''
100.16	S'''s] S'''
100.29	distinctness.] ~^
100.33	the state] in which the state
100.40	this:] ~^
101.1	P'''.] ~'^
101.1	T.] ~^
101.11	predicable'] ~^
101.18	depth.] ~^
101.25	whatever.] ~^
101.33	whatever.] ~^
101.37	them. They] ~^ they
101.37	rhetorically.] ~^
102.4	depth'] ~'^
102.6	depth] breadth
102.8	truth.] ~^

*Chapter IV. The Conception of Time (MS 237)*

Copy-text for this item is MS 237. All emendations are supplied by the editors.

103.1	they] the
*104.7	[copy A]] ~^ ~^
104.15	literally..] ~..
104.16	therefore] there^ fore
104.32	acute-angled] ~^ ~
105.4	though that] that though
105.6	lasts] last
105.6	as] <i>reinstate</i>

*Chapter IV. Conception of Time (MS 238)*

Copy-text for this version of “Chapter IV” is MS 238. There are no emendations to the copy-text.

*Chapter V. Reference to the Future*

Copy-text for this item is MS 239. The emendations are supplied by the editors.

107.8	established.] ~^
107.11–12	other ideas] others idea
108.17	future.] ~^

*Notes on Logic Book*

Copy-text for “Notes on Logic Book” is MS 240. The single emendation is supplied by the editors.

108.23 it is] is

*Peirce to Conger*

Copy-text for this letter is L 248. There are no emendations to the copy-text.

*Errors of Observation*

MS 224 serves as copy-text for “Errors of Observation.” The emendations to the copy-text are supplied by the editors.

111.17	” $g_i \epsilon_i$ ”] $g_i \epsilon_i$
111.18	” E ”] E
112.10	$\frac{E^2}{w_1}$ . ] $\sim \wedge$
113.11	= 0.] = $\sim \wedge$
113.12	7.] $\sim \wedge$

*Theory of Errors of Observations*

Copy-text is the publication of this report in *Coast Survey Report 1870*, pp. 200–224, plus Plate 27, one of many large fold-out sheets at the end of the *Report*. This plate is reproduced here in reduced size on page 160. The emendations to the text are from two sources. Those labeled E are supplied by the editors. Those labeled er are from an errata list, printed and bound into the *Report* following page 200.

There exists a Peirce-annotated offprint containing two dated (1909) notations. For a discussion of these, see Editorial Notes 114.25 and 115.20.

116.8	[m.] E; [m.]
119.16	$\xi \Xi$ ] er; $\xi \Xi$ Also 119.22, 120.6 (upper and lower), 120.14
124.11	abscissæ] E; abscissæ Also 124.16, 124.18
125.2	Astronomy] E; rom.

- 125.3       $\wedge$ *Fundamenta Astronomiae*,  $\wedge$ ] E; “Fundamenta Astronomiae,”  
 125.20      $\wedge$ *Elemente der Psychophysik*,  $\wedge$ ] E; “Elemente der Psycho-  
               physik,”
- 126.17–18     $\wedge$ *Tabula Logarithmorum Sex Decimalium*,  $\wedge$ ] E; “Tabula  
               Logarithmorum Sex Decimalium,”
- 126.33       $\wedge$ *Astronomisches Jahrbuch*  $\wedge$ ] E; “Astronomisches Jahrbuch”
- 126.36      mentioned. ¶[When] er; mentioned. They should be as fol-  
               lows: ¶[When]
- 127.7      used] er; induced
- 130.2       $\epsilon_1^2$ ] E;  $\epsilon_2^1$
- 130.9       $g_1^2$ ] E;  $g_2^1$
- 131.2       $(x_1 - x_2)^2$ ] E;  $(x_1 - x_2)^2$
- 131.6       $\frac{1}{\epsilon_1^2}$ ] E;  $\frac{1}{\epsilon_2^2}$
- 155.12     1872] E; 1871

### *Classification of Vids*

Copy-text for this item is MS 227. All emendations are supplied by the editors.

- 161.6      Benjamin Peirce] BP
- 161.12      $i_1$ ,]  $\sim$ <sup>^</sup>
- 161.12      $i_2$ ,]  $\sim$ <sup>^</sup>
- 161.12      $i_3$ ,]  $\sim$ <sup>^</sup>
- 161.12     etc.]  $\sim$ <sup>^</sup> Also 161.16, 161.19, 161.24, 162.26, 163.23, 163.27
- 161.13     another.]  $\sim$ <sup>^</sup>
- 161.17     = C.] =  $\sim$ <sup>^</sup>
- 162.9      = 0.] =  $\sim$ <sup>^</sup>
- 162.11     = I,] =  $\sim$ <sup>^</sup>
- 162.12     1,]  $\sim$ <sup>^</sup>
- 162.12     =  ${}_1j_1$ ,] =  $\sim$ <sup>^</sup>
- 162.13     = 0,] =  $\sim$ <sup>^</sup>
- 162.14     = I,] =  $\sim$ <sup>^</sup>
- 162.15     = I,] =  $\sim$ <sup>^</sup>
- 162.15     =  ${}_1j_1$ ,] =  $\sim$ <sup>^</sup>
- 162.22     algebra.]  $\sim$ <sup>^</sup>
- 162.23     I,]  $\sim$ <sup>^</sup>
- 162.23     I,]  $\sim$ <sup>^</sup>
- 162.25      ${}_1j_1$ ,]  $\sim$ <sup>^</sup>
- 162.25      ${}_1k_1$ ,]  $\sim$ <sup>^</sup>
- 162.25      ${}_2j_2$ ,]  $\sim$ <sup>^</sup>
- 162.25      ${}_2k_2$ ,]  $\sim$ <sup>^</sup>
- 162.28      ${}_1k_2$ ,]  $\sim$ <sup>^</sup>
- 163.15      $i_3$  ""]  $i_3$

*Lazelle's One Law in Nature*

The draft MS 236 serves as copy-text for this review for all but page 164, lines 1-15 (including footnote 1). These lines correspond to the missing first page of the manuscript, and the editors have substituted the 1873 *Nation* publication as copy-text for this section. Of the twenty-one substantive changes which occur between the extant manuscript copy-text and the publication, all but one (at 166.31) are accepted and entered into the copy-text as emendations. These emended readings appear to the left of the square brackets, followed by the source, either N (the *Nation* publication) or E (the editors). Unless otherwise noted, the readings to the right of the semicolons are the rejected readings from the respective copy-texts. For a discussion of choice of copy-text and treatment of this item, the reader is referred to the Essay on Editorial Method.

- 164.1       ^One . . . Nature.] E; 'ONE . . . NATURE'  
 164.3       ^One . . . Nature.] E; 'One . . . Nature'  
 164.8       ^Though] E; " ~  
 164.10      undeniable.] E; ~."  
 164.17      which,] N; ~ ^  
 164.18      case,] N; ~ ^  
 164.19      is,] N; ~ ^  
 164.20      least,] N; ~ ^  
 164.20      there] N; There  
 164.22      and] N; & *Also* 165.22, 166.2, 166.14(1,2)  
 164.24      19:] N; ~ ^  
 164n.1-3     ^One Law . . . of Energy] E; 'One Law . . . of Energy  
                 Army.] E; ~.'  
 \*165.1-3     Though this . . . time.] N; *not present*  
 165.1        ^Though] E; " ~ N  
 ¶165.1       [gravitation]] E; (~) N  
 165.3        time.] E; ~." N  
 165.5        observe the] N; what  
 165.6        reasoning:] N; ~!  
 165.6        Gravitation] N; It  
 165.16      propagation] N; polarization  
 165.18      made them] N; attempted it  
 165.20      physicists] N; students of the forces of nature  
 165.21      favor,] N; ~.  
 165.21      but] N; Only  
 165.23-24    This . . . not done.] N; If Captain Lazelle fancies he has so

- developed his ideas he is further removed from a conception of scientific accuracy than we suppose.
- 165.26            expressed] N; defined
- 165.35            hydrogen).] N; ~.)
- 165.36–37        we can expect to find no others and know of no others] N;  
we know of no others and can expect to find no others
- 165.39            (second)<sup>2</sup>,] N; ~<sup>~</sup>
- 166.1            light,] N; ~<sup>~</sup>
- 166.2            time,] N; ~<sup>~</sup>
- 166.19            a body] N; a rigid body
- 166.19–20        without . . . strain,] N; *not present*
- 166.25            the rotation of] N; rigid
- 166.26            there is,] N; *not present*
- 166.27            natural force] N; law of nature
- 166.29            forces] N; other laws
- 166.30            forces] N; laws
- 166.31            modulus] *stet* 236; modules N
- 166.32            are] N; is
- 166.38            have,] N; ~<sup>~</sup>
- 166.38            therefore,] N; ~<sup>~</sup>
- 166.38            while] N; though
- 166.38–39        are . . . precise] N; have not the final

### *Rainfall*

Copy-text for “Rainfall” is the publication of it in the *Atlantic Almanac*, 1874 (Boston: James R. Osgood & Co., 1873), p. 65. Editorial emendations are labeled E, and 225 represents an emendation adopted from MS 225 (written in the hand of amanuensis B). For a discussion of the choice of copy-text and other related manuscripts, see the Essay on Editorial Method. For a listing of all the variants between the five pages of MS 225 that represent a draft of two pages of the copy-text, see the MS 225 List of Variants.

- 168.4            two-thirds] E; ~<sup>~</sup>~ *Also* 168.5
- 168.28            place,] E; ~<sup>~</sup>
- 172.38            late] 225; last
- 172.39            one-half] E; ~<sup>~</sup>~

### *On Political Economy*

Copy-text for this item is MS 267. All emendations are supplied by the editors.

173.7	propositions:] $\sim_{\wedge}$
173.15	(or] $\wedge \sim$
173.15	$= p]$ $= \sim_{\wedge}$
174.5	negative.] $\sim_{\wedge}$
174.5	$> z.$ ] $> \sim_{\wedge}$
174.6	propositions.] $\sim_{\wedge}$
174.7	increase] in $\wedge$ crease
174.10-11	unchanged.] $\sim_{\wedge}$
174.12	have:] $\sim_{\wedge}$
174.18	seller's] sellers
174.26(1,2)	$y_c,$ ] $\sim_{\wedge}$
174.26	$= \varphi_2 y,$ ] $= \sim_{\wedge}$
174.32	$> \varphi_1 y,$ ] $> \sim_{\wedge}$
175.2	$x_0,$ ] $\sim_{\wedge}$
175.2	$x_{fc},$ ] $\sim_{\wedge}$
175.2	$= F_2 x,$ ] $= \sim_{\wedge}$
175.3	$< x_c,$ ] $< \sim_{\wedge}$
175.14	$y,$ ] $\sim_{\wedge}$
175.23	variations.] $\sim_{\wedge}$
176.1	Sep.] $\sim_{\wedge}$
176.18	Economy.] $\sim_{\wedge}$
176.19	desirability] desireability
176.26	true.] $\sim_{\wedge}$
176.30	be] be be
176.32	strawberries.] $\sim_{\wedge}$
176.32	much,] $\sim_{\wedge}$
176.33	less,] $\sim_{\wedge}$

### *Multiple Algebra*

Copy-text for “Multiple Algebra” is MS 273 which has the third page missing. Copy-text for that missing page, running here from 178.6 through 179.2, is the publication in *Proceedings of the American Academy of Arts and Sciences* 10 (May 1874–May 1875): 393. Only two substantive variants (at 179.5 and 179.9) occur between the manuscript and the publication, and both have been rejected as emendations to the manuscript copy-text. The *Proceedings* (PAAAS) is cited as the source for seven emendations of accidentals to the manuscript copy-text which normally would have been made by the editors. In addition, the editors (E) have removed twelve points of terminal punctuation from the *Proceedings* copy-text. For a discussion of the editorial treatment of this article, see the Essay on Editorial Method.

- 177.12 etc.] PAAAS; ~<sup>^</sup> Also 177.13, 177.14, 177.16, 177.17, 177.19  
 177.19 *i*<sub>11</sub>.) E; ~<sup>^</sup>  
 177.19 *i*<sub>12</sub>.) E; ~<sup>^</sup>  
 177.21 self-contradiction] PAAAS; ~<sup>^</sup> ~  
 178.6 *bi*<sub>5</sub>.) E; ~.  
 178.13 *D:E*<sub>~</sub>) E; ~.  
 178.14 *A:C*<sub>~</sub>) E; ~.  
 178.15 *A:F*<sub>~</sub>) E; ~.  
 178.16 *D:C*<sub>~</sub>) E; ~.  
 178.18 *F:C*<sub>~</sub>) E; ~.  
 178.19 *bd*<sub>5</sub>.) E; ~.  
 178.23 *rj*) E; 0  
 178.26 *C:F*<sub>~</sub>) E; ~.  
 178.27 *A:F*<sub>~</sub>) E; ~.  
 178.28 *E:F*<sub>~</sub>) E; ~.  
 179.1 *B:F*<sub>~</sub>) E; ~.  
 179.2 *C:F*<sub>~</sub>) E; ~.  
 179.5 *m*<sub>[0]</sub>] stet 273; *m*<sub>[ ]</sub>  
 179.9 *m*<sub>[0]</sub>] stet 273; *m*<sub>[ ]</sub>

### *Abstract of Photometric Researches*

Copy-text for this abstract is MS 279. All emendations are supplied by the editors.

- 180.19 showed,) ~<sup>^</sup>  
 180.21 *Annalen*] rom.  
 180.22 Helmholtz's] Helmholtz's  
 181.3 Per cent<sub>~</sub>) ~ ~.  
 181.5 Calc.) ~<sup>^</sup>  
 182.1-2 observers') observers.  
 182.7 *Durchmusterung*] rom. Also 182.14, 182.18, 183.1, 183.35  
 182.7 *Uranometria*] rom. Also 183.1, 185.9  
 183.5 by<sub>~</sub>) ~,  
 183.7 galactic] galactic  
 183.8 necessary] nesseccary  
 183.9 Wm.) ~<sup>^</sup>  
 183.34(2) mag.) ~<sup>^</sup>  
 184.2 lying] lieing Also 185.10  
 184.15 parallel,) ~<sup>^</sup>  
 184.17 distance,) ~<sup>^</sup>  
 184.28 increase] increases  
 185.7 etc.,) ~<sup>^</sup> Also 185.14  
 185.10 stars,) ~<sup>^</sup>  
 185.21 were] was

*Fundamentals of Algebra*

Copy-text for the two discrete texts on the “Fundamentals of Algebra” is MS 287. The emendations are supplied by the editors.

186.6	be.] $\sim_{\wedge}$
186.9	= y.)] = $\sim_{\cdot})$
187.20	ligation.] $\sim_{\wedge}$
188.8	$\prec$ y,] $\prec$ $\sim_{\wedge}$
188.10	$\prec$ y,] $\prec$ $\sim_{\wedge}$
188.12	$\prec$ y,] $\prec$ $\sim_{\wedge}$
188.12	$\prec$ 'y,] $\prec$ $\sim_{\wedge}$

*Axioms of Geometry*

MS 293 serves as copy-text for “Axioms of Geometry.” The emendations are supplied by the editors.

189.16(1)	etc.] $\sim_{\wedge}$
190.14	OA,] $\sim_{\wedge}$

*Logical Contraposition and Conversion*

Copy-text for this single, coherent, and finished piece consists of two parts. The first, running from page 191, line 1, through page 192, line 11, is the published portion as it appears in *Mind* 1 (July 1876): 424-25. MS 291 serves as copy-text for the remainder.

191.4	<i>Mind</i> ] MIND
192.8	Negroes] negroes <i>Also</i> 192.9
192.30	a certain] certain
193.16	v-X,] $\sim,$
194.3	Y,] $\sim_{\wedge}$

*Addition to the note for Mind*

Copy-text for this item is MS 292. The emendations are supplied by the editors.

195.1	$\wedge$ Addition] [ $\sim$
195.1	<i>Mind</i> ,] MIND]
195.11	abbreviation] abbrievation

- 196.8        forms of] forms  
 196.19      'B s B']  $\sim$  s  $\sim$   $\wedge$

### *Non-Associative Multiplication*

Copy-text for this item is MS 294. The emendations are supplied by the editors.

- 199.6        etc.]  $\sim$   $\wedge$  *Also* 199.7, 199.10, 199.11, 199.12, 199.13, 199.14,  
                 199.15, 199.16, 199.17, 199.18, 199.20, 199.21, 199.22(1,2),  
                 199.23(1,2), 199.24(1,2), 199.32, 200.34, 201.21, 201.22,  
                 201.23  
 200.27      Taylor's] Taylors  
 200.30      definable] definible  
 201.6        *with,*]  $\sim$   $\wedge$   
 201.6        *than,*]  $\sim$   $\wedge$   
 201.13       $+ \Sigma_i \Sigma_j \xi_{ij}.$ ]  $+$   $\sim$   $\wedge$

### *Principles of Mechanics*

Copy-text for each of the four discrete texts is MS 298. Emendations are supplied by the editors.

- 202.4        glossing] glosing  
 202.21      3rd.]  $\sim$   $\wedge$   
 203.6        desirable] desireable  
 203.26–27    hypothesis] hopethesis  
 204.4        viz.]  $\sim$   $\wedge$   
 206.16      + H  $\wedge$ ]  $+$   $\sim$  –

### *Nicholas St. John Green*

Copy-text for this obituary notice is the publication of it in *Proceedings of the American Academy of Arts and Sciences*, n.s. 4 (1877): 289–91. The only emendation to the text is supplied by the editors.

- 209.11       $\wedge$  *American Law Review,*  $\wedge$ ] “American Law Review,”

### *Sensation of Color*

Copy-text for “Note on the Sensation of Color” is its original publication in *American Journal of Science and Arts*, 3d ser. 13 (April

1877): 247-51. The article was republished a few months later in the supplement to (PM) *Philosophical Magazine and Journal of Science*, 5th ser. 3 (1877): 543-47, here cited as the first appearance of five corrections which normally would have been made by the editors. The other three emendations are supplied by (E) the editors. See the Essay on Editorial Method for a discussion of choice of copy-text and the Historical Collation for a complete listing of variants between the two publications.

- |           |  |
|-----------|--|
| 211.1     | <i>Note</i> ] E; ART. XXVI.— <i>Note</i>       |
| 212.8     | sensation] E; sensa <sub>n</sub> tion          |
| 212.12    | spaces be] PM; spaces,                         |
| 213.11-12 | <i>Photometric Researches</i> ] E; <i>rom.</i> |
| 215.14    | dz] PM; <i>dz</i>                              |
| 215.15    | <i>y</i> ,] PM; ~ <sub>n</sub>                 |
| 215.16    | <i>y</i> ,] PM; ~ <sub>n</sub>                 |
| 215.17    | position] PM; postion                          |

### *Flexibility of the Pendulum Support*

Copy-text for this item is the publication of it in *Coast Survey Report 1881* (Washington: Government Printing Office, 1883), pp. 427-36. All emendations to the text are supplied by the editors. For helpful information in emending the text, the editors relied on the two French versions of this paper (the handwritten lithograph copy distributed prior to the 1877 Geodetic Conference and the printed version in the conference proceedings) and a manuscript summary report that Peirce turned in to the Coast Survey three weeks before the conference (National Archives, Record Group 23); none of these is listed as a source of emendation, however.

- |           |  |
|-----------|--|
| 218.6     | knife-edge] ~ <sub>n</sub> ~                                     |
| 222.20    | $\frac{g}{(l + MS^h i)} \cdot t]$ $\frac{g}{(l + MS^h i)^t}$     |
| 224.9     | +0.356] +.0356   |
| 225.17-18 | friction-wheels] friction-whels                                  |
| 225.36    | ± 0 <sup>mm</sup> .0001] ± 0 <sup>mm</sup> .001                  |
| 226.21    | pages 223-25] pages 430-431                                      |
| 227.3     | Rogers] Rodgers  |
| 229.12    | S' J' Also 229.17  |
| 230.14    | 303.9376] 303.9374   |
| 230n.3    | <i>in</i> - <i>i</i> ( <i>i</i> - 1)] - <i>i</i> ( <i>i</i> - 1) |

- 231.1         $\times 0.0000186] = \sim$   
 231.21      299.9561] 299.9591  
 233.18–234.1   ....|Mean  $T_1 = 1^s.0063371$ .|PENDULUM] .....|PENDULUM

### *New Class of Observations*

Copy-text for this item is MS 311. The emendations are supplied by the editors.

- 235.7        individual] individial  
 235.20       logically] logical  
 236.26       there] their

### *Grassmann's Calculus of Extension*

Copy-text for this item is the publication of it in *Proceedings of the American Academy of Arts and Sciences* 13 (May 1877–May 1878): 115–16. The emendations are supplied by the editors.

- 238.4        „*Mathematische Annalen*,“ “Mathematische Annalen”  
 238.21       Grassmann] Grassman

### *Fixation of Belief*

Copy-text for this article is the publication of it in *Popular Science Monthly* 12 (November 1877): 1–15. The single emendation to the copy-text is supplied by the editors. For a discussion of this article and related manuscripts, see the Essay on Editorial Method and the Historical Collation.

- 243.2        „*Novum Organum*,“ “Novum Organum,”

### *How to Make our Ideas Clear*

Copy-text for this article is the publication of it in *Popular Science Monthly* 12 (January 1878): 286–302. The emendations are supplied by the editors.

- 260.29       of] ef  
 273.21       Lissajous] Lissajoux

- 274.24            „Full] “~  
 274.27            air.„] ~.”

### *Doctrine of Chances*

Copy-text for “The Doctrine of Chances” is the publication of it in *Popular Science Monthly* 12 (March 1878): 604-15. All emendations are supplied by the editors.

- 278.17            Quetelet] Quetelet  
 279.37            „But] “~  
 280.4            kind.„] ~.”  
 280.5            „*Essay concerning Humane Understanding*.„] “Essay concerning Humane Understanding”  
 281.13            slightest] slightest  
 281n.2            „*Logic of Chance*.„] “Logic of Chance.”  
 283.17            he plays] plays

### *Probability of Induction*

Copy-text for this article is the publication of it in *Popular Science Monthly* 12 (April 1878): 705-18. All emendations are supplied by the editors.

- 292.6-7            „*Formal Logic: . . . Probable*.„] “Formal Logic: . . . Probable.”  
 292.30            incorrectly] correctly  
 292.30             $\frac{7 \times 19}{100 \times 100}.$ ] ~;  
 293.19-20          consequences] conse-|sequences  
 297.11            *Ampliative*] *Ampliative*  
 298.9            Quetelet] Quetelet  
 298.19            another.] ~^  
 298.20            third,] ~^  
 301.37            page 299] page 713

### *Order of Nature*

Copy-text for “The Order of Nature” is the publication of it in *Popular Science Monthly* 13 (June 1878): 203-17. The emendations are supplied by the editors.

- 308.16            this] thus  
 310.6            letters.] ~,  
 310.7            disorderly,] ~.  
 313.4-5            „*Biographical Dictionary*.„] “Biographical Dictionary.”

### *Deduction, Induction, and Hypothesis*

Copy-text for this final paper of the series is the publication of it in *Popular Science Monthly* 13 (August 1878): 470–82. The emendations are supplied by the editors.

329.8	amplificative] ampliative
336n.2–3	<i>Proceedings . . . Sciences,</i> ] “Proceedings . . . Sciences,”
337.4	possession] posession
338.21	pages 325–26] page 472

### *Comment se fixe la croyance*

Copy-text for the French translation of this first paper in the “Illustrations” series is the publication of it in *Revue Philosophique de la France et de L’Étranger* 6 (December 1878): 553–69. All emendations are supplied by the editors.

339.36	Tycho_Brahé] ~-~
344.13	que] qui
344.13	nous_] ~,
344.24	dispositions] propositions
348.19	universel] universelle
349.33	créateurs] créatures
350.2	causes] choses
351.34	sensations] relations
354.6	traquait] tracassait
354.19	très-facile] très-faible
354.23	qui] que

### *Comment rendre nos idées claires*

Copy-text for the French version of “How to Make our Ideas Clear” is the publication of it in *Revue Philosophique de la France et de L’Étranger* 7 (January 1879): 39–57. Emendations are supplied by the editors.

358.30	L’histoire] L’hlstoire
358.32–34	peuple ayant les idées en aussi . . . sur les idées qu’il a.] peuple ayant les idées qu’il a.
360.34	complètement] complétement
362.23	croyances] croyancee
365.5–7	l’objet. III ¶Quelques] l’objet. ¶Quelques
365.27	augmente] angmente

- 372.22 Lissajous] Lissajoux  
 372n.2 événements] évènements  
 373.22 dépend] ne dépend pas

### *Ferrero's Esposizione*

The publication of this review in the *American Journal of Mathematics* 1 (1878): 59-63 serves as copy-text. The only emendation to the copy-text is supplied by the editors. For a discussion of this article and a related manuscript, see the Essay on Editorial Method and the Historical Collation.

- 379.23  $y'''_{1,}] \sim_{\wedge}$

### *Photometric Researches*

The original 1878 publication of *Photometric Researches* (Leipzig: Wilhelm Engelmann) serves as copy-text for the three chapters printed here. All emendations are supplied by the editors.

- 382.1 *Researches: Made]*  $\sim . | \sim$   
 383.22 self-evident]  $\sim_{\wedge} \sim$   
 384n.1 *Philosophical Transactions]* rom.  
 384n.1 150 (1861):] CL,  
 385n.1 "Ueber . . . Farbenmischung,"]  $\sim_{\wedge} \sim . . . \sim_{\wedge}$   
 385n.1 *Annalen der Physik und Chemie]* Poggend. Ann. Bd.  
 385n.2 150 (1873):] 150,  
 385n.2 71-93 and 221-47.] p. 71. 221.  
 385n.3 *Archiv für Anatomie und Physiologie,*] Archiv für Anat. und Physiol.  
 385n.3 1852,]  $\sim$ .  
 387.12(2) curve] curves  
 389.2 page 382] page 1  
 389.5 Abbreviations] Abbrieviations  
 \*389.21 Astrophotometer (see Plate II).] Astrophotometer.  
 390.5 a tenth] tenth  
 392.30 No.]  $\sim_{\wedge}$  Also 393.1, 394.1, 395.1, 396.1, 397.1  
 392.35 light,]  $\sim_{\wedge}$ ,  
 393.33 Clear,]  $\sim_{\wedge}$   
 394.29 Clear,]  $\sim_{\wedge}$   
 394.42 Clear,]  $\sim_{\wedge}$   
 396.12 27] 72  
 399.7 convex] concave  
 403.4 1874] 1847  
 403.38 Urs.]  $\sim_{\wedge}$

- 406.2 result in] result  
406.23 Plate III] a plate  
407.3 *Uranometria] rom.* Also 473.11, 474.11, 474.20–21, 475.1,  
475.14, 475.27  
407.15 VII,] ~<sup>..</sup>  
407.28 etc.,] ~<sup>..</sup>  
\*407.33 memoir (see Plate VI).] memoir.  
408.8 –164.0] –164<sub>..</sub>0  
410.1 *s, Aurigae]* s, Aurigae  
410.10  $\psi^9$ , *n.*]  $\psi^9$   
411.18 successive] succesive  
412.19  $f_3,$ ] ~<sup>..</sup>  
412.19 etc.,] ~<sup>..</sup>  
413.10 values of] values of of  
414.18 where] Where  
418.17–18 *Durchmusterung] rom.* Also 475.28, 478.3–4  
427.23 (3)] ~<sup>..</sup>  
\*458.27 169<sup>9</sup>] 169\*  
460.2 majoris] majorrs  
\*460.8 206<sup>12</sup>] 206\*  
466.8 49<sub>..</sub>53] 49.53  
473.8 brighter] fainter  
\*473.9 but little than] *stet*; but little fainter/brighter than  
475.1 nor] now  
479.8 magnitude] magnitudes  
482.1 ordinates (see Plate IV).] ordinates.  
482.10 Plate V] Plate III  
490.2 from page 481] from page 178

# Historical Collation

## *Sensation of Color*

The following lists all variants occurring between the two publications of “Note on the Sensation of Color.” In all cases, the readings to the left of the brackets coincide with the present text and unless otherwise noted are from (AJ) *American Journal of Science and Arts*, 3d ser. 13 (1877): 247–51 which serves as copy-text. Other references are to (PM) *Philosophical Magazine and Journal of Science*, 5th ser. 3 (1877): 543–47 and (E) the editors.

- 211.1 ART. XXVI.—*Note on the Sensation of Color*; by C. S.  
PEIRCE.] NOTE ON THE SENSATION OF COLOUR. BY C.  
S. PEIRCE. PM
- 211.5 *et seq.* color] colour PM
- 211.6 favor] favour PM
- 211.7 *hypothesis*] Hypothesis PM Also 211.10
- 211.14 form,] ~<sup>^</sup> PM
- 211.18 taken] taken as PM
- 211.20 <sup>^</sup>and] (~ PM
- 211.20 approximately<sup>^</sup>] ~) PM
- 211.20 true,] ~<sup>^</sup> PM
- 211.26 principles,] ~<sup>^</sup> PM
- 211.26 that<sup>^</sup>] ~, PM
- 211.28 be,] ~<sup>^</sup> PM
- 212.3 Thus,] ~<sup>^</sup> PM
- 212.5 + Kk. And] + ~; and PM
- 212.6 color] colour PM
- 212.7 *et seq.* colors] colours PM
- 212.7 which] in which PM
- 212.11 find,] ~<sup>^</sup> PM
- 212.11 fact,] ~<sup>^</sup> PM
- 212.12 contiguous] continuous PM
- 212.12 spaces<sup>^</sup> be] PM; spaces, AJ

- 212.15      *red*,] ~, PM  
 212.23      cross,—] ~<sup>—</sup> PM  
 212.24      wave<sub>λ</sub>] ~<sub>—</sub> PM *Also* 213.13  
 212.26      violet,<sub>λ</sub>] ~; PM  
 212.31      violet,<sub>λ</sub>] ~, PM  
 212.35      another;<sub>λ</sub>] ~— PM  
 212.38      Now,<sub>λ</sub>] ~<sub>—</sub> PM  
 213.3      cannot,<sub>λ</sub>] ~<sub>—</sub> PM  
 213.6      principles]<sub>λ</sub> principle PM  
 213.7      less<sub>λ</sub>, refrangible]<sub>λ</sub> ~<sub>—</sub> PM  
 213.7      may,<sub>λ</sub>] ~<sub>—</sub> PM  
 213.7      therefore,<sub>λ</sub>] ~<sub>—</sub> PM  
 213.11–12    *Photometric Researches*,<sub>λ</sub>] E; *Photometric Researches*,<sub>λ</sub>  
                  AJ; ‘Photometric Researches,’ PM  
 213.14      b]<sub>λ</sub> *ital.* PM  
 213.15      green,<sub>λ</sub>] ~; PM  
 213.24      same<sub>λ</sub>, colored]<sub>λ</sub> same-coloured PM  
 213.25      quantities,<sub>λ</sub>] ~<sub>—</sub> PM  
 213.25      <sup>λ</sup>namely] (~ PM  
 213.26      constant,<sub>λ</sub>,<sub>λ</sub>) ~), PM  
 213.31      R,<sub>λ</sub>] ~<sub>—</sub> PM  
 213.32      colored]<sub>λ</sub> coloured PM  
 213.36      r,<sub>λ</sub>] ~; PM  
 213.37      or,<sub>λ</sub>] ~<sub>—</sub> PM  
 213.37      least,<sub>λ</sub>] ~<sub>—</sub> PM  
 213.38      r';]<sub>λ</sub> ~, PM  
 214.6      unity,<sub>λ</sub>] ~<sub>—</sub> PM  
 214.18      Blue<sub>λ</sub>, violet]<sub>λ</sub> ~<sub>—</sub> PM  
 214.21      <sup>λ</sup>After]<sub>λ</sub> ¶~ PM  
 214.21      experiments,<sub>λ</sub>] ~<sub>—</sub> PM  
 214.22      following:<sub>λ</sub>] ~<sub>—</sub> PM  
 214.31      .0032]<sub>λ</sub> .2032 PM  
 215.2      1.2,<sub>λ</sub>] ~, PM  
 215.3      0.6,<sub>λ</sub>] ~, PM  
 215.4      .96,<sub>λ</sub>] ~; PM  
 215.6      is, however]<sub>λ</sub> however, is PM  
 215.8      still,<sub>λ</sub>] ~<sub>—</sub> PM  
 215.9      endeavor]<sub>λ</sub> endeavour PM  
 215.14      dz]<sub>λ</sub> PM; dz AJ  
 215.15      y,<sub>λ</sub>] PM; ~<sub>—</sub> AJ  
 215.16      y,<sub>λ</sub>] PM; ~<sub>—</sub> AJ  
 215.17      position]<sub>λ</sub> PM; postion AJ  
 215.18      uniform,<sub>λ</sub>] ~<sub>—</sub> + *fn. not present* PM  
 215.20      that,<sub>λ</sub>] ~, PM  
 215.23      way)<sub>λ</sub>,<sub>λ</sub>) ~, PM  
 215.23      brightest,<sub>λ</sub>] ~— PM  
 216.5      =  $\frac{x}{z}$  ] = ~, PM

- 216.5        =  $\frac{y}{z}$  ] = ~; PM  
 216.13      is,] ~<sup>~</sup> PM  
 216.13-14    therefore,] ~<sup>~</sup> PM  
 216.15      green,] ~<sup>~</sup> PM  
 216.17      discrimination.] discrimination.—Silliman's *American Journal*, April 1877. PM  
 216.18-22     ¶My observations . . . Professor Rood.] *not present* PM

### *Fixation of Belief*

MS 190 is a two-page fragment belonging to the 1872-73 "Toward a Logic Book" series. Five years later, Peirce repeated the approximately three paragraphs covered by this fragment in "The Fixation of Belief." Below are listed all variants between the printed version as it appeared in *Popular Science Monthly* 12 (November 1877): 1-15 and MS 190. The fragment begins "Gallileo, and Gilbert. . ." (243.12) and ends "... certain properties of" (244.15). In all cases, the readings to the left of the brackets coincide with the present text and are from the *Popular Science Monthly* copy-text. The readings to the right of the brackets are from the earlier manuscript fragment.

- 243.12      Galileo] Gallileo  
 243.13      more] much more  
 243.14      Kepler] Keppler  
 243.14 + *fn.* Mars;<sup>1</sup> 1. Not quite so, . . . words.] Mars\*; \*This is not an exact statement, but it is near enough for the purpose.  
 243.15      on] upon  
 243.16      done,<sub>^</sub>] ~,  
 243.21      us,) ] ~<sub>^</sub>)  
 243.22      until,) ] ~<sub>^</sub>  
 243.23      fell,) ] ~<sub>^</sub>  
 243.23      orbit,) ] ~<sub>^</sub>  
 243.24      well,furnished] ~<sub>^</sub>~  
 243.26      every] Every  
 243.26      science,<sub>^</sub>] ~<sub>^</sub>,  
 243.27      generations,<sub>^</sub>] ~<sub>^</sub>,  
 243.29      written; and] ~<sub>^</sub>. And  
 243.31      chemist's] chemists  
 243.37      fact:] ~<sub>^</sub>—  
 243.37(1)    his] His  
 243.37      laboratory,) ] ~<sub>^</sub>  
 244.1        reasoning,) ] ~<sub>^</sub>  
 244.2        open,) ] ~<sub>^</sub>  
 244.2        things,<sub>^</sub>] ~<sub>^</sub>,

- 244.3–4      fancies.] ¶ The fancies. Since his time the result of the internal operation of the mind, has no longer been the conclusion reached, but has only been to ask the question to be put to nature.] ¶ The
- 244.8      would] will
- 244.9      Clausius and Maxwell] we
- 244.9      were] are
- 244.11      run,] ~,
- 244.12      would,] will,
- 244.12      circumstances,] ~ ~
- 244.12      acquire] have
- 244.12      velocities,] ~ ,
- 244.13–14      would take place, . . . collisions,] will be such a number of collisions between them each second,
- 244.14      etc.;] ~ ,
- 244.14      were] we are

### *Ferrero's Esposizione*

MS 315 is a two-page untitled fragment, a very early version of not quite the first two full paragraphs of Peirce's review of "Esposizione del metodo dei minimi quadrati" as published in the *American Journal of Mathematics* 1 (1878): 59–63. Listed below are all variants between the AJM copy-text version and the corresponding manuscript fragment which begins with "Recent discussions . . ." (375.5) and ends with an alternative reading to ". . . in the text" (375.28). In all cases, the readings to the left of the brackets are the AJM copy-text readings. The readings to the right of the brackets are from MS 315.

- 375.5      country,] ~ ^
- 375.5      method] Method
- 375.6      have . . . mention] (in other respects incomplete) betray a singular ignorance of
- 375.6      views] well-known views
- 375.6      of] of] of
- 375.7      accomplished] *not present*
- 375.7      Survey] survey
- 375.8      which was] *not present*
- 375.8      published,] ~ ^
- 375.8      in part,] *not present*
- 375.9      here,] ~ ^
- 375.10–11      at all . . . method,] and a summary of the reasoning may be found of interest.
- 375.12      Lieut. Col.] Colonel

- 375.12 begins by considering] establishes, first,  
375.12 principles] principle  
375.13 mean.] ~; from which the rest of the method, as is well  
known, is easily deduced.  
375.13 observed,] ~  
375.14 independently,] ~  
375.14 value] value of the unknown quantity  
375.15 might] is to  
375.15(1) the] these  
375.17 of their] of time of their  
375.17 occurrences] occurrence  
375.18-19 the circumstances . . . times.] if they are all taken under like  
circumstances they only differ by not having been made at  
the same time.  
375.20-22 the value . . . value.] if all the results of observation are in  
complete agreement, their common value must be assumed  
as the value of the unknown quantity.  
375.22 The author] Ferrero  
375.24 all] *not present*  
375.24 and] &  
375.24 2d,] 2nd,  
375.25 equal,) ] ~  
375.27 mean,] ~,  
375.27-28 of Gauss, the quadratic mean,<sup>1]</sup>] *not present* + *fn. not*  
*present*  
375.28 in the text.] by our author.

## List of Variants

### ***MS 219***

The following lists both substantive and accidental variants occurring between copy-text MS 218 (in the hand of amanuensis B) and the MS 219 copy made directly from it (by amanuensis A). Unless otherwise noted, the readings to the left of the brackets are from MS 218, and those to the right, the MS 219 variants. In all cases, the readings to the left of the brackets correspond to the present text.

- |            |   |
|------------|---|
| 77.25      | <i>10] 10th</i> 219   |
| 77.25      | <i>73.] 1873</i> <sup>~</sup> <sub>A</sub> 219              |
| 77.27      | <i>these]</i> had 219                                       |
| 77.29–78.1 | <i>Fourth Chapter]</i> fourth chapter 219                   |
| 78.10      | <i>mind;]</i> <sup>~</sup> <sub>A</sub> 219                 |
| 78.12      | <i>think it]</i> 219; think 218                             |
| 78.12      | <i>be,]</i> <sup>~</sup> <sub>A</sub> 219                   |
| 78.14      | <i>thoughts,]</i> <sup>~</sup> <sub>A</sub> 219             |
| 78.17      | <i>reality;]</i> <sup>~</sup> <sub>A</sub> , 219            |
| 78.19      | <i>analysis,]</i> <sup>~</sup> <sub>A</sub> 219             |
| 78.20      | <i>vibrations,]</i> <sup>~</sup> <sub>A</sub> 219           |
| 78.22      | <i>every]</i> 219; evry 218                                 |
| 78.25      | <i>struggle,]</i> <sup>~</sup> <sub>A</sub> 219             |
| 78.31      | <i>kind,]</i> <sup>~</sup> <sub>A</sub> 219                 |
| 78.33      | <i>thing,]</i> <sup>~</sup> <sub>A</sub> , 219              |
| 78.36      | <i>place,]</i> <sup>~</sup> <sub>A</sub> 219                |
| 78.38      | <i>two]</i> the two 219                                     |
| 79.4       | <i>a mind]</i> 219; a a mind 218                            |
| 79.6       | <i>out,]</i> <sup>~</sup> <sub>A</sub> 219                  |
| 79.13      | <i>cognition,]</i> <sup>~</sup> <sub>A</sub> 219            |
| 79.19      | <i>thought,]</i> <sup>~</sup> <sub>A</sub> 219              |
| 79.20–21   | <i>does not change,]</i> <i>not present</i> 219             |
| 79.22–23   | <i>can not]</i> cannot 219 <i>Also</i> 80.21                |
| 79.24      | <i>reasoning. And,]</i> <sup>~</sup> <sub>A</sub> , and 219 |

- 79.32 reality<sub>1</sub>] ~, 219  
 79.33 expressions;]<sub>1</sub> ~<sub>1</sub> 219  
 79.34 seen,]<sub>1</sub> ~<sub>1</sub> 219  
 80.1 anything]<sub>1</sub> any thing 219  
 80.1 thought;]<sub>1</sub> ~<sub>1</sub> 219  
 80.4 thought,]<sub>1</sub> ~<sub>1</sub> 219  
 80.11 would] could 219  
 80.12-13 It could . . . negative.] *not present* 219  
 80.14 universe,]<sub>1</sub> ~<sub>1</sub> 219  
 80.16 sense,]<sub>1</sub> ~<sub>1</sub> 219  
 80.17 We,]<sub>1</sub> ~<sub>1</sub> 219  
 80.17 mind,]<sub>1</sub> ~<sub>1</sub> 219  
 80.18 be] by 219  
 80.33 crystal,]<sub>1</sub> ~<sub>1</sub> 219  
 80.40 occasion,]<sub>1</sub> ~<sub>1</sub> 219  
 80.40 its] this 219  
 81.4 center] centre 219  
 81.8 exist] existed 219  
 81.18 ]? [ ] *not present* 219

*MS 225*

The following lists all variants between pages 6-10 of MS 225 (written in the hand of amanuensis B) and the corresponding two paragraphs of "Rainfall" as published in the 1874 *Atlantic Almanac*. Unless otherwise noted, the readings to the left of the brackets are the AA copy-text readings, and the readings to the right are the MS 225 variants. Reference is also made to (E) the editors. In all cases, the readings to the left of the brackets correspond to the present text.

- 171.2 ¶There] . . . there 225  
 171.3 States,]<sub>1</sub> ~, 225  
 171.4 rainfall] rain-fall 225 *Also* 171.7  
 171.6 Schott,<sub>1,2</sub>] ~\*, \*The work is published by the Smithsonian Institution. 225  
 171.6 are,]<sub>1</sub> ~<sub>1</sub> 225  
 171.8 rainy] dry 225  
 171.8 dry] rainy 225  
 171.9 are,]<sub>1</sub> ~<sub>1</sub> 225  
 171.9 moreover,]<sub>1</sub> ~<sub>1</sub> 225  
 171.10 any one] anyone 225  
 171.11 recognize] recognise 225  
 171.11 month,]<sub>1</sub> ~<sub>1</sub> 225  
 171.12 January,]<sub>1</sub> ~<sub>1</sub> 225  
 171.14 Southern] southern 225

- 171.14 Europe,] ~; 225  
 171.15 Florida; still] ~. Still 225  
 171.20 dog-days] the dog-days 225  
 171.20 summer] Summer 225  
 171.21 country,] ~, 225  
 171.21 might,] ~, 225  
 171.22 perhaps,] ~, 225  
 171.25 midwinter] mid-winter 225  
 171.25(1) is] are 225  
 171.26 dog-days] dry, days 225  
 171.29 Md.,] ~.~ 225  
 171.30(1-6) "] not present 225 *Also* 171.31-34(1-6)  
 171.30 Va.,] ~.~ 225  
 171.31 S.C.,] ~.~.~ 225  
 171.32 S.C.,] ~.~.~ 225  
 171.33 Ga.,] ~.~ 225  
 171.34 Fla.,] ~.~ 225  
 171.34 10.6] 11.6 225  
 171.35 ¶In] ~ 225  
 171.37 are,] ~— 225  
 171.38 seasons,] ~.~ 225  
 172.1 North] north 225 *Also* 172.7  
 172.2 August,] ~, 225  
 172.5 so-called] ~.~ 225  
 172.5 subtropical] sub-tropical 225  
 172.8 April,] ~.~ 225  
 172.8 May,] ~, 225  
 172.9 Farther] Further 225 *Also* 172.30-31  
 172.12 Boston,] ~.~ 225  
 172.21 Amherst,] ~, 225  
 172.22 driest] dryest 225 *Also* 172.30  
 172.24 Williamstown,] ~.~ 225  
 172.27 West] west 225  
 172.27 modifications,] ~.~ 225  
 172.30 again,] ~.~ 225  
 172.31 west,] ~, 225  
 172.32 rainfalls] rain-fall 225  
 172.33 Territory,] ~, 225  
 172.33 is] not present 225  
 172.34 California,] ~, 225  
 172.35 1st] first 225 *Also* 172.36  
 172.36 Territory,] ~, 225  
 172.37 Sitka,] ~, 225  
 172.38 late] 225; last AA  
 172.38 June,] ~, 225  
 172.38-39 three and one-half] E; three and one half AA; 3½ 225  
 172.39 fall] falls 225

## Word Division

The following list records the editors' resolutions of compounds or possible compounds hyphenated at the end of a line in the copy-texts.

2.22	text-book
5.33	forthcoming
10.23	looking-glass
10.31	otherwise
31.37	inkstand
36.21	cloven-hoofed
52.30	non-entity
62.12	weathercock
67.7	weathercock
101.4	coextensive
110.5	tradesmen
133.24–25	crown-wheel
133.29	crown-wheel
135.14	non-conducting
135.32	fall-apparatus
170.10	weather-maps
171.19	thunderstorms
172.32	rainfalls
210.10–11	overflowing
213.2–3	self-luminosity
213.27	twenty-six
217.15	overlooked
236.22	overthrows
237.17–18	pro-metaphysical
238.21–22	eight-fold
245.2	self-satisfied
255.2	ill-calculated
260.5	over-valued
262.6	railway-station
263.33	starting-place
271.2	hair-splitting

294.24	psycho-physical
310.30	A-ness
312.24	chance-medley
326.21	horseback
355.7	Par-dessus
357.12	c'est-à- dire
366.4	c'est-à- dire
372.30	eux-mêmes
382.8	wave-length
390.37	color-circle
397.30(1)	pinholes
397.32	pinhole
405.6	Pinhole
475.36	star-density
477.26	star-density

The following is a list of those words which are broken at the end of a line in the present text and which should be transcribed as hyphenated. All other ambiguously broken compound words or possible compound words should be transcribed as single words.

3.1	anti-supernaturalists
43.25	ninety-two
85.8	Self-lover
133.15	hand-gearing
133.17	electro-magnet
133.23	crown-wheels
133.24	crown-wheel
135.8	break-circuit
137.3	one-eightieth
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