Frontmatter

Titlepage

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Part Two: 1894-1900

Compiled and Annotated by
Kenneth Laine Ketner and James Edward Cook

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CONTENTS

58 (11 January 1894) 31

NOTES [on Langley's 1
"Internal Work of the 8
Wind"]

Probably by Peirce

58 (11 January 1894) 34-35

HUXLEY'S 1
ESSAYS 9

58 (8 February 1894) 105-107

SCOTT'S 2
FAMILIAR 3
LETTERS

58 (15 February 1894) 124-125

EARLY 2
MAGNETICAL 9
SCIENCE.--I.

58 (22 February 1894) 139

NOTES [on Langley's *Internal Work of the Wind*] 3 3

Probably by Peirce

58 (22 February 1894) 141-142

EARLY MAGNETICAL SCIENCE.--II. 3 4

58 (8 March 1894) 180-181

FUNK'S STANDARD DICTIONARY 3 9

58 (15 March 1894) 197-199

MATHEMATICA 4
L FUNCTIONS 4

58 (29 March 1894) 234-236

LOCKYER'S DAWN 4
OF ASTRONOMY 8

58 (12 April 1894) 278

The Monism of Man, by David Allyn Gorton

Genetic Philosophy, 5
by David Jayne Hill 3

58 (19 April 1894) 299

An Elementary Treatise on Fourier's Series and Spherical and Ellipsoidal Harmonics, by William Elwood Byerly

59 (12 July 1894) 34-35

Basal Concepts in Philosophy, by Alexander T. Ormond62

Probably by Peirce

59 (19 July 1894) 52-53

The Animal as a Machine and a Prime Motor, by R. H. Thurston64

59 (23 August 1894) 144-145

ALCHEMY AND CHEMISTRY 65

59 (13 September 1894) 191-193

HELMHOLTZ 69

59 (27 September 1894) 237-238

FOUR HISTORIES OF PHILOSOPHY.--I. 7 3

59 (4 October 1894) 251-252

FOUR HISTORIES OF PHILOSOPHY.--II. 7 6

59 (25 October 1894) 312-313

Essays in Historical Chemistry, by T. E. Thorpe 8 0

Probably by Peirce

59 (8 November 1894) 343

NOTES [on the Proceedings of the Psychical Research Society] 8 2

By an anonymous author

59 (8 November 1894) 344-345

SPINOZA'S ETHIC 83

59 (22 November 1894) 381

HALLUCINATIONS 87

Letter

59 (22 November 1894) 383

NOTES [on Wundt's *Vorlesungen über Menschen und Thierseele*]88

Probably by Peirce

59 (29 November 1894) 409

NOTES [on Peirce's review of Spinoza] 88

By an anonymous author

59 (6 December 1894) 430-431

Modern Scientific Whist, by C. D. P. Hamilton	8 9

59 (27 December 1894) 476-477

DESCARTES AND HIS WORKS	9 1

60 (14 March 1895) 208

Logic, by Christoph Sigwart	9 4

60 (21 March 1895) 226-227

Philosophy of Mind, by George Trumbull Ladd 96

Probably by Peirce

60 (4 April 1895) 265

Nicolai Ivanovich Lobachevsky, by A. Vasiliev, translated by George Bruce Halsted 98

Probably by Peirce

60 (11 April 1895) 284-285

Comte, Mill, and Spencer, by John Watson 100

Probably by Peirce

61 (14 November 1895) 353-354

Studies in the Evolutionary Psychology of Feeling, by Hiram M. Stanley 112

61 (28 November 1895) 395

The Psychology of Number, by James A. McLellan and John Dewey 113

61 (19 December 1895) 447

ACETYLENE	1
AND	1
ALCOHOL	5

By an anonymous author

61 (19 December 1895) 453

Great Astronomers,	1
by Sir Robert S. Ball	1
	6

	61 (26 December 1895) 464	
	ACETYLENE AND ALCOHOL	118
Probably by Peirce		
	62 (2 January 1896) 16-18	
	BENJAMIN'S HISTORY OF ELECTRICITY	120
62 (9 January 1896) 32-33		
	ADDITION AND SUBTRACTION	125
Letter by William D. Mackintosh		

62 (9 January 1896) 42

Science and Art Drawing,	1
by J. Humphrey Spanton	2
	6

Probably by Peirce

62 (6 February 1896) 122

NOTES [on Halsted's translation of Bolyai's *Absolute Science of Space*] 127

Probably by Peirce

62 (13 February 1896) 147

Molecules and the Molecular Theory of Matter, by A. D. Risteen 128

62 (26 March 1896) 261-262

Mind and Motion, and	1
Monism, by George John	3
Romanes	0

62 (23 April 1896) 330-331

Algebra und Logik der Relative, von Ernst Schroeder 132

62 (21 May 1896) 404

The Number Concept, by Levi Leonard Conant 134

63 (23 July 1896) 71-72

KÜLPE'S OUTLINES OF PSYCHOLOGY 135

63 (3 September 1896) 181-182

Elements of the Theory of Functions of a Complex Variable, by H. Durège 139

64 (25 March 1897) 227

NOTES [on the death of Sylvester]	142

65 (4 November 1897) 362-363

Studies in Psychical Research, by Frank Podmore	142

65 (25 November 1897) 424

The Principles of Chemistry, by D. Mendeléeff	145

65 (9 December 1897) 458-459

NOTES [on *The Consolation of Philosophy of Boëthius*]146

65 (30 December 1897) 515

PROPOSED SYLVESTER MEMORIAL 1 4 7

Letter by Raphael Meldola

65 (30 December 1897) 518

NOTES [on Perry's *Calculus for Engineers*] 1 4 8

65 (30 December 1897) 524-525

The Conception of God, by Josiah Royce, Joseph Le Conte, G. H. Howison,

and Sidney Edward Mezes 1 4 9

66 (3 February 1898) 96

Some Unrecognized Laws of Nature, by Ignatius Singer and Lewis H. Berens 153

66 (31 March 1898) 250-251

Social and Ethical Interpretations in Mental Development, by James Mark Baldwin 154

66 (21 April 1898) 311

Memory and its Cultivation, by F. W. Edridge-Green 157

66 (28 April 1898) 330-331

Astronomy, by Agnes M. Clerke, A. Fowler, and J. Ellard Gore158

Probably by Peirce

66 (16 June 1898) 467-468

Dynamic Idealism, by Alfred H. Lloyd

Practical Idealism, by William De Witt Hyde 1
5
9

Probably by Peirce

67 (14 July 1898) 31

NOTES [on Reye's *Geometrie der Lage*] 1
6
2

67 (14 July 1898) 38-39

The New Psychology, by E. W. Scripture 1
6
2

Probably by Peirce

67 (25 August 1898) 154-155

67 (20 October 1898) 300-301

Logic, Deductive and Inductive, by Carveth Read	1 7 5

67 (24 November 1898) 390

NOTES [on Lambert's *Differential and Integral Calculus for Technical Schools and Colleges*]

	1 7 7

67 (24 November 1898) 397

The Story of Marco Polo, by Noah Brooks	1 7 7

67 (1 December 1898) 417-418

Radiation, by H. H. Francis Hyndman	1 7 9

67 (22 December 1898) 469-470

DARWIN'S TIDES	1 8 0

68 (2 February 1899) 95-96

Matter, Energy, Force, and Work, by Silas W. Holman	1 8 4

68 (16 March 1899) 210

Liebniz, translated 1
by Robert Latta 8
6

68 (20 April 1899) 296

NOTES [on 1
Kepler's 8
Somnium] 8

68 (27 April 1899) 316-317

A History of Physics in its Elementary Branches, by Florian Cajori 189

68 (4 May 1899) 338

My Inner Life, by 1
John Beattie 9
Crozier 2

68 (18 May 1899) 376

GALILEO'S REASONING 1
9
3

68 (25 May 1899) 403

The Gambling World, by Rouge et Noir

The History of Gambling in England, by John Ashton 1
9
4

68 (25 May 1899) 405

Outlines of Industrial Chemistry, by Frank Hall Thorp197

68 (22 June 1899) 482-483

Stars and Telescopes, by David P. Todd 1
9
8

68 (29 June 1899) 499-500

MARSHALL'S INSTINCT AND REASON 200

69 (6 July 1899) 18

An Introduction to the Theory of Analytic Functions,

by J. Harkness and F. Morley 203

Probably by Peirce

69 (27 July 1899) 77-78

Old Clocks and Watches and their Makers, by F. J. Britten205

69 (3 August 1899) 97-98

LEIBNIZ REWRITTEN 2
0
6

69 (10 August 1899) 118

Through Nature to God, by John Fiske 2
1
0

69 (24 August 1899) 154

NOTES [on Sampson's edition of the works of Berkeley] 2
1
2

Probably by Peirce

69 (7 September 1899) 192-193

From Comte to Benjamin Kidd, by Robert Mackintosh

Better-World	2
Philosophy, by J.	1
Howard Moore	2

69 (21 September 1899) 231

Mathematical Essays and Recreations, by Hermann Schubert, translated by

Thomas J. McCormack

The Study and Difficulties of Mathematics, by Augustus De Morgan 214

69 (28 September 1899) 248-249

Observational Geometry,	2
by William T. Campbell	1
	6

Probably by Peirce

69 (19 October 1899) 303-304

The Boy's Book of Inventions, by Ray Stannard Baker	2 1 7

69 (9 November 1899) 355-356

FORD'S FRANKLIN	2 1 8

69 (14 December 1899) 455

The Life of James Dwight Dana, by Daniel C. Gilman	2 2 2

70 (4 January 1900) 11

NOTES [on Lewis' *Treatise on Crystallography*]	2 2 5

70 (15 February 1900) 128

NOTES [on Thorp's 2
Outlines of Industrial 3
Chemistry] 5

70 (1 March 1900) 163

NOTES [on Watson's *A* 2
Text-Book of Physics] 3
6

70 (15 March 1900) 203-204

NOTES [on Pearson's 2
Grammar of Science] 3
7

70 (15 March 1900) 204

NOTES [on Hertz' *The Principles of Mechanics Presented in a New Form*]
237

70 (22 March 1900) 230

The Teaching of Elementary Mathematics, by David Eugene Smith 238

— 10 —

70 (5 April 1900) 267

The World and the Individual, by Josiah Royce 239

70 (19 April 1900) 302-303

GROSSETESTE 241

70 (10 May 1900) 366

Inorganic Evolution as Studied by Spectrum Analysis, by Sir Norman Lockyer 245

70 (17 May 1900) 384-385

History of Ancient Philosophy, by W. Windelband 246

70 (31 May 1900) 417

NOTES [on Bottone's *Wireless Telegraphy and Hertzian Waves*]249

70 (21 June 1900) 480-481

Introduction to Ethics, by Frank Thilly 249

70 (28 June 1900) 502-503

Illustrations of Logic, by Paul T. Lafleur 251

70 (28 June 1900) 504-505

A Short History of Free Thought, Ancient and Modern, by John M. Robertson 253

71 (5 July 1900) 14

NOTES [on Boethius' *De Consolatione Philosophiae*] 254

71 (19 July 1900) 59

Theory of Differential Equations, by Andrew Russell Forsyth 255

71 (26 July 1900) 78-79

A History of Modern Philosophy, by Harald Höffding 256

71 (26 July 1900) 79

The Kinetic Theory of Gases, by Oskar Emil Meyer 259

71 (16 August 1900) 131

THILLY AND WUNDT

Letter by	2
Frank	6
Thilly	1

71 (30 August 1900) 178

The Theory of Electrolytic Dissociation, by Harry C. Jones262

71 (6 September 1900) 192-193

NOTES [on Perry's "Practical	2
Mathematics: Elementary	6
Stage"]	4

71 (20 September 1900) 235-236

Bordeaux and its 2
Wines, by Édouard 6
Feret 5

71 (27 September 1900) 257

Acetylene, by 2
Vivian B. 6
Lewes 7

71 (11 October 1900) 295-296

Joseph Glanvill, by 2
Ferris Greenslet 6
9

71 (18 October 1900) 314-315

A Brief History of Mathematics, by Wooster Woodruff Beman and

David Eugene Smith 272

INTRODUCTION

Charles Sanders Peirce's contributions to *The Nation* represent not only a valuable gift to philosophy, but also an encyclopedic intellectual time capsule for the latter part of the nineteenth century. It is our goal to make public a second time, in a convenient format, all of the verified Peirce contributions along with those articles that we or other Peirce scholars believe to be his work. So that discussions presented in these materials may be followed to the fullest, relevant items by other authors have also been included. The actual contributions will be issued in three separate parts, this volume being the second. This three-part division is not to be construed, however, as reflecting any surmised structure in these writings. It is adopted solely for convenience of publication. A final fourth part is planned that will include indices and appendices for the preceding pages.

The editor of *The Nation* during most of the period of Peirce's collaboration was Wendell Phillips Garrison.[†1] It is clear, on the basis of several sources,[†2] that Garrison often cut Peirce's contributions, sometimes chopping off whole paragraphs or rewriting some sections. After 1881, *The Nation* was acquired by the New York *Evening Post*. Thereafter, many reviews from *The Nation* would also be published in *The Post*. Thus, there are a great many items by Peirce in *The Post*. It is likely that some of those reviews avoided Garrison's knife. Moreover, there are probably other items by Peirce in *The Post* not yet discovered. For purposes of the present project, however, we have not undertaken to survey material published in *The Post*. An investigation into that material is now in progress at the Institute for Studies in Pragmaticism.

In editing these materials, we have followed a strictly chronological sequence. Instead of providing a special identifying number (as Burks did in his bibliography in volume eight of the *Collected Papers*--the review of Porter, for example, being numbered as N-1869-1), we decided to let the full citation of volume number, date, and page number serve in much the same role. Our system has the advantage of allowing for easy addition of any later discoveries of new Peirce contributions without doing any damage whatsoever to previously established numbering. Following the citation of volume, date, and page, we have reproduced the column heads, titles, and bibliographic data exactly as they appeared in *The Nation*, making only minor changes in typography in some instances. A major book review was given a separate title--for example, "Professor Porter's 'Human

Intellect'." Smaller reviews and notices were included in a section entitled "Notes." Some of the shorter reviews are preceded by publication data for the book reviewed; others are simply incorporated into the Notes section with no special designation. Correspondence was also given a special title--for example, "Mr. Peirce and the Realists." All such titles or distinguishing heads, as provided by the editorial staff of *The Nation*, are reproduced in our text.

Annotations preceding the body of most items are intended to give the reader additional useful information about the piece that follows. We felt that a reader first would want to know what evidence is available to show that the item was written by Peirce (or by another author, if such were the case). Therefore, we cite all information or argument known to us that either confirms or makes probable Peirce's authorship. The only published bibliography that we take to be conclusive in identifying Peirce's contributions is Daniel C. Haskell, *Index to the Nation* (volumes 1-105, 1865-1917), New York: The New York Public Library, volume 1, *Index of Titles*, 1951 and volume 2, *Index of Contributors*, 1953. This index was prepared using original ledger books from *The Nation* offices (for further information, consult Haskell's introduction). We have also used other bibliographic sources (listed below), which we cite in our notes, but we have not taken them as conclusive; instead, we have sought outside confirmation of entries in these bibliographies, and have cited such confirmatory evidence (when available) in the annotations.

The manuscripts of the Peirce collection in the Houghton Library at Harvard University have been catalogued by Richard S. Robin in *Annotated Catalogue of the Papers of Charles S. Peirce*, Amherst: University of Massachusetts Press, 1967; see also Richard S. Robin, "The Peirce Papers: A Supplementary Catalogue," *Transactions of the Charles S. Peirce Society*, 7(1971):37-57. Several of these manuscripts have been of considerable importance in confirming Peirce's authorship. These will be mentioned in our notes using the numbering system adopted by Robin. We shall often be referring to the Garrison correspondence file (MS L 159) according to a numbering system that is an extension of Robin's numbers (for example, the first item in the Garrison file will be labelled L 159.1, the second L 159.2, and so on). A complete catalogue of that correspondence file hopefully will be included in the final volume of this work. We also intend to include in that volume other manuscripts that are important for identification or comparison.

The annotations are concluded, in many cases, with a brief biography of the principal personality in the article that follows. Cases in which a biography is not given are usually cases for which data were not readily available in standard source books. We believe that this will be of considerable assistance to the reader in that it will enable one to get an idea of the kinds of persons with whom Peirce was engaged in his writings for *The Nation*. In preparing these biographies, we have consulted primarily the works listed below as biographic sources.

In regard to the text itself, we have reproduced it exactly as it originally appeared in *The Nation*, including minor editorial or printing errors. Therefore, with the exception of any errors we might accidentally introduce in resetting the type, the materials stand as they were first published.

Numerous persons have contributed either directly or indirectly to the realization of this project. We would still be near to the starting point were it not for the presence of many good bibliographies and bibliographic supplements that other students of Peirce's work have prepared. We have added only a few items not previously known by other scholars. As our work developed, we were indeed grateful for the very generous advice and assistance of Max H. Fisch, who, along with Bill Davenport, called our attention to two items by Peirce which had escaped our notice. Berti Ketner's assistance in proofreading has been invaluable, and Peggy Cooper helped us to complete some difficult notes based on Garrison's letters to Peirce.. Carolyn Eisele also offered helpful counsel, for which we are thankful. We are indebted to Vice President J. Knox Jones, Jr., and Dean Lawrence Graves for their support in funding this task as part of the work of the Institute for Studies in Pragmaticism, Texas Tech University. James J. Storrow, present publisher of *The Nation*, encouraged us by his counsel and good wishes as our project started. Our sincere thanks also the the staff of the Houghton Library at Harvard University who have assisted us in obtaining access to materials in the Peirce papers.

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Fisch, Max H. "A First Supplement to Arthur W. Burks's Bibliography of the Works of Charles Sanders Peirce." Pp. 477-485, in *Studies in the Philosophy of Charles Sanders Peirce*, edited by Edward C. Moore and Richard Robin. Amherst: University of Massachusetts Press, 1964.

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Fisch, Max H., and Daniel C. Haskell. "Some Additions to Morris R. Cohen's Bibliography of Peirce's Published Writings." Pp. 375-381, in *Studies in the Philosophy of Charles Sanders Peirce*, edited by Philip P. Wiener and Frederic H. Young. Cambridge: Harvard University Press, 1952.

Ketner, Kenneth Laine, Christian J. W. Kloesel, Joseph M. Ransdell, Max H. Fisch, and Charles S. Hardwick, eds. *A Comprehensive Bibliography and Index of the Published Works of Charles Sanders Peirce*. Greenwich, Connecticut: Johnson Associates, 1977.

"A List of Articles, Mostly Book Reviews, Contributed by Charles S. Peirce to 'The Nation' to Which Is Appended Some Additions to the Bibliography of His Published Writings in this Journal, December 21, 1916." *The Journal of Philosophy, Psychology, and Scientific Methods*, 15(1918):574-584.

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Kenneth Laine Ketner

James Edward Cook

Volume 2

CHARLES SANDERS PEIRCE: CONTRIBUTIONS TO *THE NATION*: **1894**

58 (4 January 1894) 19: Utility of Quaternions in Physics.

By A. McAulay, Lecturer in Mathematics and Physics in the University of

Tasmania. Macmillan & Co. 1893.

CSP, identification: MS L 159.50. See also: Fisch, *First Supplement*.

When a man has acquired a considerable reputation for intellectual force and attainments, he begins to find people hanging upon his lips and adopting every proposition he may lay down; and, thereupon, being human, he straightway begins to talk about things concerning which he is not qualified to form an opinion. In that way, Maxwell, in his 'Electricity and Magnetism' (§ 10) said: "I am convinced that the introduction of the ideas, as distinguished from the operations and methods, of Quaternions will be of great use to us in all parts of our subject"; which is as much as to say that he would adopt the addition-process of multiple algebra, without its multiplication-process; and it is amazing what an influence this dictum has had in preventing men from studying quaternions. Yet in truth Maxwell was not enough versed in the subject to know what the ideas of quaternions are. Everybody who has ever exhibited any mastery of quaternions has, we believe without a single exception, pronounced Maxwell's opinion frivolous and superficial. Mr. McAulay writes to demonstrate that it is so, and to convince mathematicians of the high value of quaternions as the *calculus* which it essentially is, and not merely as involving a good idea, and especially of the high value of this calculus in the study of physical problems. We hold this thesis to be sound; but we are sorry to say we fear this book will do little to bring men into the way of truth. There is a sort of an egotistic spirit, an arbitrary self-will, displayed in it that is far from the quality best adapted to the purpose of persuasion. Its innovations in notation are confusing and quite inadmissible. Worse still, the reasoning is in many places decidedly obscure, and often quite needlessly intricate. We can but agree with Prof. Tait, the first of living quaternionists, that some sections of the book are veritable models of "how not to do it." Moreover, Mr. McAulay does not seem to be as familiar with the works of Hamilton as it behooves an apostle of quaternions to be.

Yet, with all these serious faults, the work is not useless, and as it has probably not stood in the way of the publication of any other, we are glad to get it. Though it will not convert the scoffer, it here and there offers suggestions which disciples of Hamilton will receive with satisfaction.

58 (11 January 1894) 30: NOTES

CSP, identification: MSS 1310, 1581. See also: Burks, *Bibliography*; Fisch and Haskell, *Additions to Cohen's Bibliography*.

A work in twelve volumes, each distinct, is contemplated (and partly executed) by Mr. Charles S. Peirce, member of the National Academy of Sciences. Its full title reads: 'The Principles of Philosophy; or, Logic, Physics, and Psychics, considered as a Unity, in the light of the Nineteenth Century.' The first volume, which is ready for the press, will be 'A Review of the Leading Ideas of the Nineteenth Century.' Mr. Peirce also issues a prospectus of a limited edition, now in course of printing, in two colors on hand-made paper, at the De Vinne Press, of 'The Epistle of Pierre Pelerin de Maricourt to Sygur de Foucaucourt, Soldier, On the Lodestone.' The original treatise dates from 1269, and "occupies a unique position in the history of the human mind, being without exception the earliest work of experimental science that has come down to us." The transcript of Peter Peregrinus's text has been made afresh from a contemporary MS. in the Paris Library, and is reproduced in black-letter together with a translation and notes. Subscriptions for either work should be addressed to Mr. Peirce at Milford, Pa.

58 (11 January 1894) 31: NOTES

The note on Langley's *Internal Work of the Wind* at 58 (22 February 1894) 139 is very likely by Peirce. That note refers to this notice. Hence this piece is probably authored by Peirce. This item is unassigned in Haskell's *Index to* The Nation, vol. 1.

Samuel Pierpont Langley (1834-1906) was a pioneer in aviation, although he is perhaps better known for his solar studies. For several years, Langley served as secretary of the Smithsonian Institution, where he and Peirce often conversed. In 1887, he began a series of studies of air currents for experiments on the lifting force and resistance of plane surfaces set at different angles. The results of this labor were published later as *The Internal Work of the Wind*, and were reviewed by Peirce [58 (22 February 1894) 139]. During the 1890's, Langley built a number of model flying machines to demonstrate mechanical flight, and in 1898, the United States Army appropriated $50,000 for the construction of such a machine to carry soldiers. A test run was scheduled for 8 December 1903, but, due to the malfunction of some minor part, the flight failed, and Langley was ridiculed by the press. Nine days later, near Kitty Hawk, North Carolina, the Wright brothers achieved the first manned flight. Ironically, in 1914 Glenn Curtis flew Langley's rejected machine over Lake Keuka, New York, in a long flight without trouble.

"The Internal Work of the Wind," or the want of uniformity and consistency in the movement of the wind stream, its gusty and intermittent character, and its consequent utilization by birds in soaring to maintain themselves without exertion of the wings, was the subject of a paper by Prof. S. P. Langley, read at Chicago before the International Conference on Aerial Navigation in August last. It was perhaps as original and important as any delivered in all the series of congresses, and one may now read it in the January issue of *Aeronautics*, the "annex" of the *American Engineer and Railroad Journal* of this city. In connection with it

should also be read the illustrated account of Mr. Maxim's air-ship in the current number of *McClure's Magazine*, and the (also illustrated) account of a German's experiments in man-flying (or, better, soaring) in *Nature* for December 14, 1893. As the last writer well says, here is promise of fine and tolerably safe sport.

58 (11 January 1894) 34-35: HUXLEY'S ESSAYS

Method and Results: Essays.

By Thomas H. Huxley. D. Appleton & Co. 1893.

CSP, identification: Haskell, *Index to* The Nation. See also: Burks, *Bibliography*;*List of Articles*; MSS L 159.40, L 159.47, L 159.59; MS 1389 (draft).

Huxley's collected essays are to appear in nine volumes, of which this is the first. It is well-printed and agreeable to read. An introductory autobiography will serve to remind readers what Huxley's real profession is. He has, to use his own language, "subordinated" his "ambition for scientific fame" to the "popularisation of science" (in his separate treatises) and (in his essays) to an "endless series of battles and skirmishes" with ecclesiasticism and other powers. Intellectual nettles are these essays, suggestive and stimulating to the point of painfulness. Though Huxley is not a physiologist, his branch of science lies near to physiology, and physiology borders close upon metaphysics; and a remarkably well-read man in philosophy (for an outsider) Huxley is. This goes to feed and strengthen his originality, and gives it breadth. At the same time, it greatly heightens the literary interest and animation of his essays. Not that he does not sometimes show that his reading has been hasty, and that the tedious operation of rumination, which is so necessary in philosophical thought, has been a little abridged. Thus, he adheres to the sect of English nominalism--the school of Ockham, Hobbes, Locke, Hartley, Berkeley, Hume, Bentham, and the Mills--without perceiving how antagonistic they are, upon the whole, to the spirit of science. One of the prime doctrines of these men, for instance, a doctrine inherited from the pre-scientific ages, is that all generalization is a *mere matter of convenience*. The scientific man, on the other hand, without theorizing about generals, implicitly holds that laws are really operative in nature, and that the classification he is so painfully trying to find out is *expressive of real facts*. In short, the two classes of thinkers take the *con*, and the *pro* of the question concerning universals. As the printer's devil would have it, Huxley's opinion is expressed on the bottom of a right-hand page and the top of the following left-hand one. We read (p. 117): "Classification. . . is *merely a convenient* [turn over] *expression of* . . . *facts*"; which sounds like a patch-work of two sentences. He follows the Mills in speaking of Hobbes with an extravagance of laudation which is amazing, as coming from a scientific man. What! will you call that man a great reasoner who could write treatises to uphold a circle-squaring fallacy, and whom John Wallis, hardly a mathematician of the first class, could so utterly demolish as he did? What! will you speak of him as an exponent of the spirit of science who is known in physics only by his peculiarly virulent opposition to Boyle's method and its results, and whom Boyle, a weakish sort of disputant, laid out so handsomely? Huxley praises Hobbes's "vigorous English"; but Hobbes's style, both of writing and of thinking, as well as the chief substance of his doctrines, is borrowed, or closely imitated, from William of Ockham. Now Ockham was a brilliant thinker, and some people fancy there was something modern-like about his thought. Not a bit of it; another mind so completely steeped in scholasticism of the most intensely wordy kind can hardly be instanced. Hobbes was an original thinker, even if not a very great one, but his method of thinking is scholastic, wooden, what Huxley calls "*a priori*," and anti-scientific in the extreme. Two extraordinary ideas have come to the modern consciousness from Hobbes. One is the association of ideas, which he undoubtedly stole from Aristotle; and the other is the theory of Motives, which, though it has imposed upon the world, is, in the view of many modern psychologists, a mere logical jugglery, a circle-squarer's style of thought.

Two of the most impressive essays in this volume, making a fifth of its bulk, are devoted to the praise of Descartes as the great scientific philosopher of his age. In fact, when Huxley entitles this volume 'Method and Results,' he means thereby to declare that the 'Discours de la methode' is the true exposition of the method of science. More profitable reading than these two essays of Huxley on Descartes the literature of our time has not afforded. Nevertheless, it is impossible that the judgment of history upon Descartes should be reversed. That judgment is that while his geometry was simply the making of modern mathematics, all his other ideas proved utterly unprofitable and unscientific. Prof. Huxley may persuade us to dock the last epithet, but, about the other, history cannot be wrong.

"There are some men," says Huxley, "who are counted great because they represent the actuality of their own age, and mirror it as it is. Such an one was Voltaire, of whom it was epigrammatically said, 'he expressed everybody's thoughts better than anybody.' But there are other men who attain greatness because they embody the potentiality of their own day, and magically reflect the future. They express the thoughts which will be everybody's two or three centuries after them. Such an one was

Descartes."

What a thing that to say of a man! And Huxley fairly makes it out. How can it be that, for all that, Cartesianism was scientifically barren, except in geometry, while there so richly fertile? It was a pretty complete theory of logic, nature, and the soul--the three categories under which Hegel has well summed up philosophy. But systematic completeness, as Hegel's own system well shows, is about the idlest decoration that can be attached to a philosophy. The great desideratum for a philosophy, its indispensable condition, was first stated by a thinker whom Huxley treats with uncalled-for *hauteur*--Auguste Comte; that is to say, a philosophy, to be fruitful, must be "positive"--it must lead to unmistakable consequences comparable in great detail with observation. If it does that, and if those consequences are verified to any considerable extent, it will aid the advance of knowledge. It is that which has made evolution, in the definite form given to it by Darwin, a great agent of discovery. But was not this character possessed by the theories of Descartes? Perhaps; but if they were "positive" theories, they were not theories which there was any

prospect of being able to put to the test to any considerable extent, then and there, in the state of mathematics and of observational means which were at command. Hence, though Huxley can show us *now* that the Cartesian ideas had a scientific character, yet, for practical purposes, they had not that character for the men of that time. But this was not their only defect. It has been repeatedly pointed out by students of the history of mechanics that Descartes's theories really grievously offended the very rule of philosophizing upon which he had himself so much insisted. They were not *clear and distinct*. Worse that that--for that, in itself, would not have been fatal--they were not capable of being made clear and distinct. Like the works of many other philosophers, at first glance they seemed beautifully sharp-outlined, but, when closely studied, they were found to be a composite of nebulae which no scrutiny could resolve. They wanted that fundamental perspicuity to which so few writers except mathematicians attain, which consists in this, that, unintelligible as they may seem at first reading, yet when they are closely studied they are seen to be based upon the distinctions which were pertinent to the problem.

In his long and deeply interesting discussion of Descartes's theory that animals are automatons, Huxley manifests the great disadvantage under which a comparative anatomist must labor who is not an engineer, or who has not a practical acquaintance with analytical mechanics. He thinks he makes out very clearly that animals (man included) act like machines. But he uses neither the language nor conceptions of dynamics. Far from convincing a student of mechanics, he leaves him profoundly convinced of the disparity between machines and animals. He talks about "causes," but the student of dynamics has nothing to do with "causes." That a machine could possibly act as a frog deprived of his front brain is described as acting is what Huxley does not make at all clear to a man whose business has lain with machines and with mechanical systems.

Another fifth of the volume is taken up with a review of the Progress of Science in the first fifty years of the reign of Victoria, 1837-1887. Among its many suggestins [[sic]], we have only space to notice the following:

"The doctrine of evolution, so far as the present physical cosmos is concerned, postulates the fixity of the rules of operation of the causes of motion in the material universe.... But it is possible to raise the question whether this universe ... may not itself be a product of evolution from a universe of such matter, in which the manifestations of energy were not definite--in which, for example, our laws of motion held good for some units and not for others, or for the same units at one time and not at another--and which would therefore be a real epicurean chance-world? For myself, I must confess that I find the air of this region of speculation too rarified for my constitution, and I am disposed to take refuge in ignoramus et ignorabimus."

It is always unphilosophical to say *ignorabimus*, and the shores of science are strewn with the wrecks of such predictions. It is particularly rash to base such a prediction on the circumstance that the author of it would be perplexed to see how the problem is to be made amenable to exact reasoning.

One-third of the volume is taken up with recent essays concerning the general theory of politics. They are very far below the level of Huxley's work of twenty years

ago, and, in comparison, seem almost mediocre, although they contain a good deal of interesting information concerning the history of some of the theories. The pretence that one can see no meaning in the statement that all men are born free and equal, would hardly have been patiently tolerated by our Revolutionary forefathers. Huxley reviles Rousseau as a mere sentimentalist. Of course, Rousseau was a sentimentalist by conviction, and it is quite true that, since he wrote, the world has received terrible proof of the evil of exaggerated sentimentalism. Still, civilization rests, and must rest, mainly upon sentiment. Prof. Huxley seems to pass a sweeping condemnation upon the application of what he calls "*a priori* reasoning" to questions of justice. By "*a priori* reasoning" he means deduction from general principles, such as Rousseau practised; but Huxley's general condemnation of this mode of argumentation makes it incumbent upon him to explain how he would have a court of justice reason. He even goes so far as to sneer at the principle of toleration. Where would Huxley, or any other evolutionist who lived in the sixties, have been without that principle? The principle of toleration is intimately connected with the fundamental principle of science, for it can have no rational basis except the acknowledgment that nothing is absolutely certain. In those branches of physics where knowledge is the most perfect, in metrology, geodesy, and astronomy, no self-respecting man would consent to put forward an assertion without coupling with it his estimate of its *probable error*. What scientific men mean by "science" is not knowledge, but *investigation*. Now the scientific man will not shut off any question whatever as too sacred or too well known for further investigation, and therefore he must tolerate every opinion. But, further, in regard to questions of politics and the like, the scientific man must admit that not only can the true alternative not be certainly named, but also that no formula can be framed all whose possible consequences shall be just; and for that reason it is most desirable that, alongside of the formula which is less erroneous, the opposite formula which is more erroneous should constantly have its advocates, so that it may not be forgotten when the moment comes at which it is to be preferred to the other. In this point of view, what Huxley calls "the pet doctrine of modern liberalism, that the toleration of error is a good thing in itself," appears to be not, after all, out of harmony with the ideas of science.

In the last essay Huxley discusses, in an ever-interesting manner, the opposing claims of Individualism, or *laissez-faire*, and Socialism, or, as he chooses to call it Reglementation, and reaches the easy conclusion that neither can be admitted as an absolute principle. There, rather lamely, he leaves the matter, without making the obvious remark that evolutionism supplies a third political maxim, perhaps superior to either of the others. For, the moment we admit that man was developed from an ape, whether suddenly or by insensible degrees, we are led to surmise that the rudiments of government antedated humanity. At any rate, government must be considered as one of those adaptive characters of the *genus homo* which result from development. This is true not only of government, generally, but of each special form of it, such as the United States Constitution. Now, since the characters of races are generally highly adaptive, and are also unchangeable, except under the operation of those almost cosmical causes which gradually bring about changes in races, the evolutionary philosopher will not attempt to do more than deflect very slightly the actions of these forces; whence will result a maxim of political conduct something like this: Aid only such changes as are either inevitable or else both natural and beneficial; and so act that those changes may be brought about with the least total harm. If we were to write *integral* in place of *total*, it would make the formula sound more mathematical; and sound is almost everything in matters like this.

Huxley himself has clearly put his finger upon that one of his qualities by virtue of which he has for so long commanded the respect and admiration of the public. It lies "in the conviction which has grown with my growth and strengthened with my strength, that there is no alleviation for the sufferings of mankind except veracity of thought and of action, and the resolute facing of the world as it is when the garment of make-believe by which pious hands have hidden its uglier features is stripped off." The hopes and consolations of religion will, we believe, never be reinstated in their position of authority (if at all) until this lesson of intellectual integrity has been thoroughly learned and accepted with humility.

58 (8 February 1894) 105-107: SCOTT'S FAMILIAR LETTERS

Familiar Letters of Sir Walter Scott.

2 vols. Boston: Houghton, Mifflin & Co. 1894.

CSP, identification: Haskell, *Index to* The Nation. See also: Burks, *Bibliography*; *List of Articles*; MSS L 159.40, L 159.50-51; MSS 1390, 1390(s) (drafts).

These letters unequally cover the time from 1797, when Scott, twenty-six years old, was known only as a young barrister of fair prospects and the author of some poetical translations from the German, down to 1825, the year before his earthquake of calamity They are conveniently separated into chapters, mostly of one year each; and at the beginning of each chapter is inserted a little chronological table of family events and literary achievements. The initial letter urges his suit to Miss Carpenter, whom he married on Christmas eve, three months later; and there are two other love-letters, tender, rational, and honest. Announcing his approaching marriage to one of the friends who might be useful to him, he thus describes his *fiancée*:

"A smart-looking little girl with dark brown hair would probably be her portrait if drawn by an indifferent hand. But I, you may believe, should make a piece of work of my sketch as little like the original as Hercules to me."

He wrote in 1810:

"Mrs. Scott's match and mine was of our own making, and proceeded from the most sincere affection on both sides, which has rather increased than diminished during twelve years' marriage. But it was something short of love in all its forms."

— 24 —

The earliest period, extending to the publication of the 'Lay of the Last Minstrel' in January, 1805, is scantily illustrated by three old-fashioned epistles from Scott (besides the love-letters) and one note. To most of the events of this part of his life there is no allusion. His 'Border Minstrelsy' is referred to in a remarkable letter from Hogg, the Ettrick shepherd, from which we copy the first sentence:

"DEAR SIR: I have been perusing your *Minstrelsy* very diligently for a while past, and it being the first book I ever perused which was written by a person I had seen and conversed with, the consequence hath been to me a most sensible pleasure; for, in fact, it is the remarks and modern pieces that I have been delighted most in, being, as it were, personally acquainted with many of the antient pieces formerly."

In chapter ii., embracing 1805 and 1806, we find interesting correspondence with Wordsworth and Southey. The following, addressed to Miss Seward, is noteworthy because it was written when Southey was approaching his culmination as a poet without having begun his other career, or indeed having written any prose to speak of except his delightful 'Letters from Spain and Portugal,' while Wordsworth, having published only his 'Lyrical Ballads,' had in his portfolio and must have read to Scott both "Peter Bell" and other pieces that have been passionately liked by some lovers of poetry as poetry, but which have not won the suffrages either of the great public or of the fastidious few:

"My poetical friends, Wordsworth and Southey, are certainly men of very extraordinary powers. Wordsworth in particular is such a character as only exists in romance--virtuous, simple, and unaffectedly restricting every want and wish to the bounds of a very narrow income, in order to enjoy the literary and poetical leisure which his happiness consists in. Were it not for the unfortunate idea of founding a new school of poetry, these men are calculated to give it a new impulse; but I think they sometimes lose their energy in trying to find, not a better, but a different path from what has been travelled by their predecessors."

The same letter contains this:

"Many good-natured country Tories (myself, for example) take great pleasure in coursing and fishing, without any impeachment to their amiabilities, and probably Jeffrey feels the same instinctive passion for hunting down the bards of the day."

There is a letter from Jeffrey about his recent loss of his wife. It is curious to see this cold and superficial mind under deep emotion. Southey's impressions are thus expressed: "Of Edinburgh society I think very little. Jeffrey . . . is a mere *homunculus*, and would do for a major in Gog and Magog's army.... [Compared with Coleridge and Wordsworth] the Scotch *literati* are very low indeed. But Scott is a much superior man." Scott writes to Lady Dalkeith to recommend "our Ettrick shepherd" as a valuer of sheep-land, "in which he has given great satisfaction to those who engaged him, being a remarkably intelligent, clever fellow in the line of his business." The offer being refused, Scott again writes: "I am sure that your sympathy with his situation and extreme delicacy of expression

must tend greatly to alleviate his feelings of disappointment, if he, indeed, harbors any."

A letter to Miss Seward (February 20, 1807) gives a bit of criticism, with a just estimate of Scott's own verse:

"As for poetry it is very little labor to me; indeed 't were pity of my life should I spend much time on the light and loose sort of poetry which alone I can pretend to write. Were all the time I wasted upon the *Lay* put together--for it was laid aside for long intervals--I am sure it would not exceed six weeks. The last canto was written in three forenoons, when I was lying in quarters with our yeomanry. I leave it with yourself to guess how little I can have it in my most distant imagination to place myself upon a level with the great Bards you have mentioned, the very latchets of whose shoes neither Southey nor I are worthy to unloose. My admiration of Chaucer, Spenser, and Dryden does not blind me to their faults, for I see the coarseness of the first, the tediousness occasioned by the continual allegory of the second, and the inequalities of the last; but, my dear Miss Seward, 'in those days were giants in the land,' and we are but dwarfs beside them."

Another letter to Miss Seward, November 23, 1807, when she was about to write a review of Hogg's poems, illustrates Scott's warmth of favor for every fellow-craftsman in poetry, and also his skill in the fine art of graceful hinting, which was surpassed only by (what, unfortunately, from the nature of things, it is impossible here to illustrate) his wonderful power of expressing a thing in such a way that, of two persons who were to read his letter, the one whom he wished should see what his meaning was and the other not. The whole of this letter to Miss Seward is good, but we can quote only the following:

"I do not at all like the task of reviewing, and have seldom myself undertaken it; on Poetry never, because I am sensible there is a greater difference of tastes in that department than in any other, and that there is much excellent poetry which I am not nowadays able to read without falling asleep, and which would nevertheless have given me great pleasure at an earlier period of my life. Now I think there is something hard in blaming the poor cook for the fault of our own palate or deficiency of appetite.... My reason for transporting *Marmion* from Lichfield was to make good the minstrel's prophecy of Constance's song. Why I should have taken him there I cannot very well say.... I am quite glad you have seen Southey. Delighted with him you must be, yet in conversation (great as he is) he is inferior to Wordsworth, perhaps because he is a deeper and more elaborate scholar. Southey rarely allows you any of those reposes of conversation when you are at liberty to speak, as the phrase is, 'whatever comes uppermost.' . . . I am a pretty hard worker when I once set about it, and, in fact, my literary life resembles the natural life of a savage, absolute indolence interchanged with hard work."

But Scott made a pretence to indolence--a Tory quality to which he was not honestly entitled.

Next following the above letter comes the first of a series of fifteen from Lady Louisa Stuart, known to the public by her admirable introduction to the works of her grandmother, Lady Mary Wortley Montagu.

'Marmion' was felt by the author as a great effort: "As turnips," he says, "come after wheat according to the best rules of agriculture, I take it that an edition of *Swift* will do well after such a scourging crop as *Marmion*." But when Lady Abercorn suggests that the poem was too hastily written, he hastens to admit it:

"No one is so sensible as I am of what deficiencies occur in my poetry from the want of judicious criticism and correction, above all from the extreme hurry in which it has hitherto been composed. The worst is, that I take the pet at the things myself after they are finished, and I fear I shall never be able to muster up the courage necessary to revise *Marmion* as he should be revised. But if I ever write another poem, I am determined to make every single couplet of it as perfect as my uttermost care and attention can possibly effect. In order to secure the accomplishment of these good resolutions, I will consider the whole story in humble prose, and endeavor to make it as interesting as I can before I begin to write it out in verse, and thus I shall have at least the satisfaction to know where I am going, my narrative having been hitherto much upon the plan of *blindman's buff*."

He tried this plan afterwards, and the result was that he found himself with a manuscript to burn up. All his novels were written in the same headlong way. Of course, they had been thought *about* beforehand; but that they had not been thought *out*, is proved by Scott's potitive [[sic]] testimony, backed by demonstration from the rapidity with which they were composed. Another of the world's greatest historical story-tellers, Alexander Dumas, wrote with even more railroad speed; and though some of his collaborateurs pretended, and perhaps thought, they had written his novels, it is certain none of them unaided could ever write conversations at all like his.

We may add concerning Scott's method of working, what he wrote to Lady Louisa Stuart in 1816 about 'Tales of my Landlord': "As no man that wrote so much ever knew so little what he intended to do when he began to write, or executed less of the little which he had premeditated, I totally altered my plan before I had completed my first volume." Again: "I quarrelled with my story, and bungled up a conclusion, as a boarding-school miss finishes a task which she had commenced with great glee and accuracy." In 1810, he writes: "The truth is there are weeks and months in which I do not only not use pen and ink, but have a sort of horror of the very sight of them." In 1808: "I believe no man now alive writes more rapidly than I do (no great recommendation), but I never think of making verses till I have a sufficient stock of poetical ideas to supply them." Finally: "The enclosed jangling verses are the only effort I have made in rhyme since I came to Edinburgh for the winter. They were written within this hour."

In 1809, the conduct of the *Quarterly Review*, just launched in London, in Tory opposition to the *Edinburgh*, was not Tory enough to suit Scott. He writes:

"A good deal happened when I was in London to show me that Gifford wants much of that tact which is necessary to conduct with spirit the work he has undertaken.... There is a lame and cowardly caution which prepares all the world for the defeat of the combatant who exhibits such a suspicious symptom. When the

sword was once drawn I would have hurled the scabbard into Thames.... All Gifford's excellent talent, and no less excellent principle, will do little to save the *Review* unless he will adopt a more decisive tone of warfare and greater energy in his mode of conducting it."

However, after the circulation of the *Quarterly* had passed 5,000, Scott never, in this correspondence, again refers to Gifford at all. The 'Lady of the Lake,' begun, apparently, in September, 1809, and finished the following January, was composed with more care than the 'Lay of the Last Minstrel' or 'Marmion.' Yet when Scott's friend Morritt remarked that, after a great fuss had been made over the birth of Brian, it was rather disappointing not to hear of him in the denouement, the poet makes a reply which shows the inconsiderate manner in which even this poem was written. He says:

"Your criticism is quite just as to the Son of the dry bone, Brian. Truth is, I had intended the battle should have been more detailed, and that some of the persons mentioned in the third canto, and Brian in particular, should have been commemorated. I intended he should have been shot like a *corbie on a craig* as he was excommunicating and anathematizing the Saxons from some of the predominant peaks of the Trossachs. But I found the battle in itself was too much misplaced to admit of being prolonged by any details which could be spared. For it was in the first place *episodical*, and then all the principal characters had been disposed of before it came on, and were absent at the time of action and nothing hinged upon the issue of consequence to the fable."

Concerning the dramatization of it, he writes:

"As for the metamorphosis of the *Lady of the Lake* into drama, or rather three dramas, for the same adventure is to be tried at Dublin, London, and

Edinburgh, I would not willingly have you believe either that I affect or possess stoicism enough to be insensible to the applause of a crowded theatre; on the contrary, I think that of all kinds of popular plaudits this is the manner in which an author has his most satisfactory, and perhaps intoxicating, draught of success. But I shall have no more honor, supposing any of these attempts successful, than the cook who roasted a turkey yesterday has for the caporata (I think housewives call it so) . . . presented us to-day out of the reliques of the feast."

If a *capitolade* was really called a *caporota*, that would seem to settle the derivation of the word.

In 1811, not yet moved to Abbotsford, Scott brought out the 'Vision of Don Roderick.' The Spenserian metre being objected to, he writes September, 1811: "I agree with you respecting the lumbering weight of the stanza, and I shrewdly suspect it would require a very great poet indeed to prevent the tedium arising from the frequent recurrence of rhymes." In the following January, the first two cantos of 'Childe Harold' appeared; whereupon Lady Abercorn asks Scott how he likes it. He replies: "Very much. There is more original strength and force of thinking in it, as well as command of language and versification, than in almost any modern poem of the same length that I have happened to meet with." Miss Baillie thought "Lord Byron has Walter Scott perpetually in his eye."

In December, 1812, 'Rokeby' appeared and was treated as a wonderfully great poem by Scott's Edinburgh worshippers. In 1813, he published anonymously the 'Bridal of Triermain.' July 7, 1814, was the date of the publication of 'Waverley'; and now for several years the letters fall off in quantity, and still more in quality, while Scott is fresh at his new *métier*. In

January, 1815, appeared the 'Lord of the Isles,' in February 'Guy Mannering'; then a 'Memoir of the Somervilles' in two volumes, a description of the 'Field of Waterloo,' in November 'Rowland's Poems,' and in December 'The Ettrick Garland.' These, with two articles for the 'Encyclopædia Britannica' and a review of Miss Austen's 'Emma,' did not suffice for his year's work, for he also began 'Harold the Dauntless' and wrote 'Paul's Letters to His Kinsfolk' and the 'Dance of Death.' In 1816 came 'The Antiquary,' 'The Black Dwarf,' and 'Old Mortality,' with several other things. He also wrote "Pibroch an Donnuil Dhu." This will suffice as a specimen of the years when the novels were appearing.

The second volume contains four letters of Lockhart to Sir Walter, chiefly about the affair of John Scott, and the duel which resulted. There are also seven letters from Lockhart to his wife, Scott's daughter. These are full of incident if not of wit. The following is characteristic of describer and describee:

"This morning met Sir Humphry Davy, in the highest glee, and a spick and span new white waistcoat, on his way to Connemara, etc., to fish. Broad-brim white Beaver, lined green as of yore. He told a story of an Irish steward of a steamboat from Holyhead, who was endeavoring to be very polite to a distressed Lady passenger. He said the best thing was whiskey. Lady said she had an idea that whiskey *could* not agree with her stomach. 'And is that against it?' quoth Paddy; 'won't your Ladyship have all the pleasure of tasting it over again in its way up?' . . . He told *once more* the story of the late Pope asking him, in 1814, if he thought there was anything in his (the P.'s) power to do for the good of the government of England in testimony of his gratitude. Sir H. wrote Mr. W. Hamilton to tell Castlereagh now was the time if they wished to get the Pope to give the King the nomination of the Catholic Bishops of Ireland; but they were busy, and nothing done. Probably 'Sromfridevi' overrated the value of his Holiness's pretty words; but it may be otherwise."

We will close with another specimen of Lockhart's epistolary power:

"We [Scott, daughter Anne, and himself], with Wordsworth and his daughter, went to Keswick, he spouting his own verses very grandly all the way. It was a fine sunshiny day, only too hot, and we certainly saw and heard many fine things. This I remark once for all, that during all these rides, etc., the Unknown [Scott] was continually quoting Wordsworth's Poetry and Wordsworth ditto, but that the great Laker never uttered one syllable by which it might have been intimated to a stranger that your Papa had ever written a line either of verse or prose since he was born.... Wordsworth spoke kindly, I think on the whole, of Hogg, . . . of Byron contemptuously; of Shelley well and rightly, saving that (as is the custom of all one edition clubs) he said Shelley was a greater genius than Byron (*i. e.*, a less successful one).... Wordsworth says Crabbe is always an addition to our classical literature, whether he be a poet or not."

58 (15 February 1894) 124-125: EARLY MAGNETICAL SCIENCE.--I.

William Gilbert of Colchester, physician of London, on the Load-stone and Magnetic Bodies and on the Great Magnet, the Earth: A New Physiology, demonstrated with many Arguments and Experiments.

A translation by P. Fleury Mottelay. John Wiley & Sons. 1893.

CSP, identification: Haskell, *Index to* The Nation. See also: *List of Articles*; MSS L 159.21, L 159.32; MSS 1391, 1391(s) (drafts).

William Gilbert (1540-1603) was a man of many talents. He served as physician to Queen Elizabeth I and King James I. He was president of the College of Physicians, in 1600, and he is best known for his work in astronomy and physics. His *De Magnete, Magnetecisque Corporibus* (1600) declared the earth to be a magnet, and it is said that this work is the first great scientific book published in England.

The lodestone, which Mr. Mottelay will write load-stone, is a mass of oxide of iron of the approximate composition Fe_3O_4, which is susceptible to magnetic induction, but only very slowly, and which has lain for such eons in the telluric field of magnetism as to have become polarized, often most intensely. Almost all iron ore of this low degree of oxidation, which alone is magnetic, shows more or less polarity. The properties of a lodestone are sensibly different from those of a steel magnet, owing to its great resistance to magnetic induction--a circumstance which simplifies some of the phenomena.

If the scientific stage of experimental inquiry be considered as commencing with the application to it of special apparatus, then the earliest scientific magnetician was Petrus Peregrinus, a Picard, and elder contemporary of Roger Bacon. His treatise on the lodestone was printed in 1558, but that volume is excessively rare, and the contents of the work are not generally known even to specialists. We shall refer here to the text of a MS, of the thirteenth century in the Bibliotheque Nationale (MSS. latins 7378), certainly older than a date often assigned to the composition. The ancients had noticed that lodestones would pick up iron and draw it from a distance, and that one piece of iron hanging from a lodestone would pick up another. But they did not remark the poles, nor that iron could be magnetized. But before Peregrinus wrote, the use of the magnetic needle had begun to come into use with sailors. His treatise (*Epistola*) is addressed to a friend. It begins: "Amicorum intime! Quondam magnetis lapidis occultam naturam a te interpellatus," etc.--in English as follows:

"Inmost of friends! Having been formerly interrogated by thee, I will in a rude narration disclose everywhere the occult nature of the lodestone. For among philosophers nothing is pleasant without participation of knowledge, and the nature of good things is bereaved and clouded in darkness until it is lifted up into the light of mutual surrender. For the love of thee I will write things that to the mob of students are utterly unknown. Nevertheless, of nothing but what is open to observation (*manifestus*)[†*] shall we in this letter deliver knowledge, in that this delivery will be part of the treatise in which we shall treat of constructing physical instruments. To

treat of the occult properties of this stone concerns the art of sculpturing stone, and though I call the works of which I have inquired open to observation, still they will be inestimable, and to the vulgar they are as illusions and fancies, and therefore in respect to the vulgar they are secret: but to astrologers and naturalists[††] they will be sufficiently open to observation; and to them they will be a solace and for sailing travellers no slight assistance."

Truly, the scientific spirit and insight of this and other passages of the epistle are surprising. Apart from the affectionate warmth which has in all ages been characteristic of scientific men, but is almost incomprehensible to outsiders--well-nigh incomprehensible, for instance, that Agassiz, greatly injured as a scientific leader, and, indeed, worn nearly to death by his long and vehement dispute with Darwin, should, though a strong hater when occasion was, welcome with the utmost delight, "for their father's sake," the young Darwins, whose father had, as a matter of course, recommended them to Agassiz--apart from this, there are other little traits, veritable shibboleths among the race of *Naturforscher*. We remark that the above passage contains a sort of definition of what was meant by an "occult quality" among the mediæval physicists. It means a quality not deducible from the Aristotelian doctrine of hot and cold, moist and dry, and not discoverable except by experiment, and therefore a secret to all who do not make experiments, though "satis manifesta" to those who do. We also remark that the present treatise is regarded as an instalment of a larger one which is to treat of what?--of the "construction of physical instruments"; and directly after follows a passage (imitated, in form, from Geber, but far superior to the original in matter), to the effect that a physicist ought to be a mechanician, because it is not by mere thinking but by handiwork that he is to discover the natures of things ("Scito verissime quod oportet hujus artificem scire," etc., with which compare Geber's "Scias, carissime," etc., and "Oporteat artificem huius operis in scientiis philosophiæ naturalis eruditum esse," etc. There were many mediæval imitations of the same chapter of Geber, but none to compare with this).

Accordingly, the first thing that Pilgrim Peter does is to put a lodestone into a lathe, or in some way (to be explained in another work) reduces it to a spherical form. A happy thought that. Then he lays a bit of iron wire (*acus*) upon the ball, and finds it at once turns round in a definite direction. He marks this upon the stone, and pushes the needle along in the same line. This is done at several different places on the stone, and the different lines so drawn are found, on being continued, to intersect at two points only. At those points alone the wire stands up at right angles to the surface of the sphere. They are called by Peter its *poles*; they have retained that name ever since. He next floats the lodestone with its axis horizontal in a little round boat, and finds that a certain one of the poles always turns to the north. "Si millesies amoveatur, millesies ad suum locum revertetur nutu Dei." The pole pointing north he calls the north pole, and *vice versa*. Next, taking a second lodestone, he finds that unlike poles attract, while like poles repel, one another. Next, he shows that an oblong piece of iron touched at its end by a pole of the lodestone

acquires the properties of a lodestone. He also shows the effect of breaking the stone across its axis, and that of uniting two stones in the way which satisfies their attractions. He thus recognizes magnetization by contact, but not induction at a distance.

So far, he has done his thinking, apparatus in hand; he has hardly so much as made suggestions; certainly, the conclusions were those of nature's own mind. But now he tries a little thinking with his individual brain. He concludes that the poles of the magnet direct themselves to the poles of the heavens. If he had only tried letting his lodestone sphere sink through water or some more viscous fluid, he would have discovered the magnetic dip, and would have refuted his error. But his mediæval mind was beginning to chafe under the harness of experiment. The poles of the lodestone point, he declares, to the poles of the heavens; and "no doubt every part of the lodestone points to an appropriate part of the heavens."

So he suggests that his correspondent should try the following experiment: Let a spherical lodestone be so mounted between bearings as to turn freely on its axis, and be in equilibrium in every position, while the axis is held parallel to the axis of the heavens. "Then," he says, "if the stone turns with the revolution of the heavens, you may rejoice to have discovered a wonderful secret. But if not, it is to be imputed rather to your want of skill in mounting the stone than to any defect of nature." Is not this human?

After Peregrinus, three centuries and a half elapsed without the invention of any new apparatus, and consequently without any important discovery. That of the variation of the compass (the consequence of that invention) seems to have been made by navigators without reclamation. Its different amounts in different places are shown on the maps of the Venetian Andrea Biancho, made in 1436. The dip was first clearly proved by Robert Norman in his book, 'The newe Attractive, containing a short discourse of the Magnes or Lodestone, and amongst his other vertues, of a newe discovered secret and subtill propertie, concernyng the Declinyng of the Needle touched therewith, under the plaine of the Horizon. Now first found out by R. Norman Hydrographer' (2 parts, London, 1581, 4to; other editions 1585, 1596, 1611). Norman was led to this discovery from having constructed a very accurately balanced needle.

William Gilbert came of an ancient family, seated at Clare, in Suffolk, on the border of Essex. His father was a prominent and apparently wealthy lawyer in Colchester. The boy attended the grammar school, and in 1558 obtained the scholarship in St. John's, Cambridge, belonging to that school. He arrived in Cambridge just at the beginning of one of the religious revolutions there. St. John's, then altogether the most distinguished college in England, had for its master, when Gilbert first went there, an excellent scholar, George Bullock, who, on account of his Romanism, was within a year displaced by a still more learned man, and a wise one, James Pilkington, an ardent Calvinist, later Bishop of Durham. The Cambridge undergraduates were at that time coarse enthusiasts for Calvinism; but the dons mostly looked upon the church as a union to uphold holy living and put down evil, and thought theology was a secondary matter. They changed about from Protestantism to Catholicism and from Catholicism to Protestantism, without enthusiasm and without reluctance, at the royal bidding. It is rather a remarkable fact that Gilbert's great work contains not a single direct reference to God. In Book VI., cap. iii., he refers to the waters above the firmament "of the divine Moses," merely to explode the idea. Being a strong Copernican, he does remark that the Holy Scriptures recognize no *primum mobil* nor rotation of the entire firmament. But this is the only reply he condescends to make to the theological objections to the Copernican opinion, except that in one place he exclaims, "Excutiant theologi et spongiis deleant aniles istas" (Let theologians shake off and wipe out such old woman's nonsense). He speaks of "the divine and perspicacious genius" of Aquinas; but to translate "Divus Thomas," where *Divus* is simply the title of his grade of official sainthood, by "the godlike Thomas," as Mr. Mottelay does, is hardly warranted. However, these expressions pretty clearly show that Gilbert was a convinced Anglican, or, as we may say, Elizabethan, in religion.

Gilbert showed an early bent for chemistry. The latest novelty in that line was then Giambattista Porta's 'Natural Magic,' first published in Naples in 1558; and though this work was written when the author was only fifteen years old, it had an immense popularity, and was soon translated into Italian, French, Spanish, and Arabic. It was republished, greatly enlarged, in 1589. (We shall refer to the corrected edition of 1591.) But a passion for chemistry would imply readings in Ripley and in Paracelsus. Accordingly, we find in the 'De Magnete' several passages of Paracelsist leaning. During Gilbert's first year in Cambridge, he had, in accordance with the statutes then in force, to devote himself chiefly to the study of arithmetic, although the commission of 1559 changed this to rhetoric. Gilbert's writings amply attest his unusual accomplishments in both directions, but more particularly in mathematics. It is true that, in the last chapter of the 'De Magnete,' Mr. Mottelay (p. 354) makes Gilbert sneer at mathematicians; but the use of *hi*, "the latter," shows that not the mathematicians, but the philosophers, are meant. During his second year at the university, he was required to devote his studies to logic. Sir T. Wilson's 'Rule of Reason' was the only book on the subject that had been printed in England, but Gilbert certainly went deeper than that; his writings show it unmistakably. He was graduated in 1560, and obtained a fellowship. If, as Anthony a Wood asserts, he studied also in Oxford, it was most probably about this time. In 1565 he was mathematical examiner in St. John's, in 1569 he took his degree in medicine, and was appointed Senior Fellow.

On leaving college he went to travel on the Continent. He returned in 1573, and established himself in London. His house was on St. Peter's Hill, between Upper Thames Street and Little Knightrider Street. Mr. Mottelay locates this in Colchester; but the street (in the parish of St. Peter Paul's Wharf), called St. Peter's Hill, a continuation of Sermon Lane, appears on the old maps of London, and exists to this day. Upon being appointed principal physician to Queen Elizabeth, Gilbert seems to have moved to Whitehall. This broke up a society, the precursor of Gresham College and the Royal Society, which had for a long time met at his house. During his years in London he filled several offices in the Royal College of Physicians, those of censor, treasurer, consilarius, and finally, in 1600, the year of the publication of his great work, that of president. He died November 30, 1603, which was a plague year.

Gilbert is described as tall in stature. If we may trust to his portrait, he was robust and of energetic bearing. His head was square, his forehead square and smooth, his chin long, his physiognomy that of a practical man. At the same time, his appearance is refined and somewhat delicate. He was probably handsome. He never married. He was of a calm and cheerful disposition, "reserved but not morose," says Thomas Fuller. His book abounds in irascible expressions concerning certain classes of writers: "May the gods damn all such sham, pilfered, distorted works, which do but muddle the minds of students" (Book II., chap. xxxv.). He occasionally speaks rather haughtily of individuals. Thus, Jean Baptiste Besard (whose name Mr. Mottelay twice writes Bessard), certainly a very distinguished physician, is referred to as "one Besardus, a Frenchman." Although in 1600 Francis Bacon had not published anything about science, he had, no doubt, talked about it with Gilbert, and he is not improbably alluded to among the *rabulis*, or "pettifoggers," to whom the magnetic philosophy will not prove acceptable. Bacon, when his turn came, spoke contemptuously of Gilbert; but he gave dignity to what he said by making it quite true: "Gilbert has not unscientifically introduced the question of magnetic force. But he has himself become a magnet. That is, he has ascribed too many things to that force, and built a ship out of a shell."

58 (22 February 1894) 139: NOTES

That Peirce authored this note is strongly suggested by several pieces of evidence. In MS 1013, Peirce discusses Langley's theory of the aerodynamics of soaring. In a letter to Peirce which is dated 3 October 1894, Langley mentions that Peirce is in possession of a copy of this paper. Reference is also made to Langley's researches in MS 1015. See also: Fisch, *First Supplement*. This note is unassigned in Haskell's *Index to* The Nation, vol. 1.

--The Smithsonian Institution has just published the memoir by Prof. S. P. Langley, on the "Internal Work of the Wind," to which we made a brief reference a month ago. The fact which is here established is very simple, and naturally to be expected, namely, that all wind is very puffy, and a high wind more so than a gentle breeze. Although the author's apparatus had too much inertia to show how very great the real changes were, yet he got such results as this: On a certain day and hour, the wind, having a "velocity of twenty-three miles an hour, . . . rose within ten seconds to a velocity of thirty-three miles an hour, and within ten seconds more fell to its initial speed. It then rose within thirty seconds to a velocity of thirty-six miles an hour, and so on, with alternate risings and fallings, at one time actually stopping, and passing through eighteen notable maxima and as many notable minima, the average interval from a maximum to a minimum being a little over ten seconds, and the average change of velocity in this time being about ten miles an hour." It must evidently be practically impossible to put air into motion without setting up waves of condensation and rarefaction; therefore, the fact of the puffiness of the wind is not surprising. But the use to which Prof. Langley puts this fact is quite unexpected. His title indicates that he conceives air in such an oscillating condition as being substantially heated, though the heat is of a peculiar kind. However, he points out that

in this case it is possible to violate the second law of thermo-dynamics; and he advances the hypothesis (which he puts very nearly if not quite out of doubt) that this is the way birds sail in the air. Namely, imagine a bird with outspread wings nearly motionless. Let it incline the plane of the wings so that the wind shall strike the under side. Then, Prof. Langley shows, from previous experiments of his own, that, owing to the inertia of the bird's body, instead of being carried on, it will at first be lifted. Gradually, however, the velocity of the wind will be communicated to the bird. But as it begins to feel the pressure on its wings diminishing, it either tilts the plane of the wings, or wheels, so that in the lull of the wind it will slip back upon an inclined plane to its first position. When the next puff comes, its wings are inclined as at the beginning, and its momentum is actually against the wind. This is not the whole story; but we must refer our readers to the memoir itself, which is much clearer and easier to understand than our attempt to state the matter. In fact, the perspicuity of expression in it is almost as remarkable as the perspicacity of thought. How great that is is shown by the failure of all previous attempts to explain the flight of birds.

58 (22 February 1894) 141-142: EARLY MAGNETICAL SCIENCE.--II.

William Gilbert of Colchester, physician of London, on the Loadstone and Magnetic Bodies and on the great Magnet, the Earth; A New Physiology, demonstrated with many Arguments and Experiments.

A translation by P. Fleury Mottelay. John Wiley & Sons. 1893.

CSP, identification: Haskell, *Index to* The Nation. See also: Burks, *Bibliography*.

The immediate success of Gilbert's book on the lodestone was surprising. No doubt it astonished the author. He had said in his preface, *paucis philosophandum censuimus*, "we have resolved to philosophize for a few" (which Mr. Mottelay translates, We have held that philosophy is for the few), and it is said that the first edition consisted of but a small number of copies. Yet a burst of the most valuable admiration followed the publication. Kepler refers to it twice in his *magnum opus*, and in many other places, with the highest praise. Galileo said it excited his envy. Even Descartes, with his French indisposition to build up any other man's reputation, referred to it with approval. It is true, as might have been expected, that Descartes was able very much to improve upon Gilbert's conceptions. All this applause was somewhat more than the work really deserved. Those from whom it came were not acquainted with Peregrinus, and did not know that, notwithstanding his reclamatory asterisks, much of what so delighted them was not original with Gilbert. Still, today, with all the facts before us, we cannot but admit that the book displays a wonderful Faradayesque power of carrying along experimentation and cerebration, hand in hand. If we ask what positive new principle Gilbert defined, we must admit that there is nothing except that of induction at a distance; but that is a good deal. He also has a vague conception of curved lines of force, which, imperfect as it is, is of inestimable value.

Furthermore, he is the author of the theory that the earth is a magnet. His apparatus consisted of spherical lodestones imitated from Peregrinus, and also of the versorium, or small compass-needle, of which Peregrinus made no use. Gilbert's discoveries were inevitable consequences of his improved apparatus. When he has gone successfully as far as his apparatus will carry him, he begins to theorize in a way that merits Bacon's strictures. He wishes to account for the invariability of the direction of the earth's axis and for the rotation of the earth, by magnetism. He thinks that magnetic polarity is an essential character of the pure chemical element, earth. That is almost as unscientific as Peregrinus, towards whose vagaries Gilbert betrays some leaning.

Although Dr. Gilbert had the honor of giving its name to electricity (that is, he invented the adjective *electric*, from which the abstract noun followed as a matter of course), yet his electrical experiments are contemptible. The reason was that he made no electroscope, but used only an unmagnetic versorium. The consequence was that almost everything escaped him, even to electrical polarity, and even to repulsion, which one does not clearly see how he could help noticing. In short, he left electricity just about where the ancients left magneticity.

Celebrated and lauded as the 'De Magnete' always has been throughout Europe, it is singular that it has been printed only twice. The first edition is in folio. Twenty years ago, for an exceptionally fine copy, the exorbitant Quaritch would make a man pay 25 shillings, and now the multiplication of rich electricians has made $25 cheap for it. The second edition, which first appeared in 1628, is in quarto. It bears the imprint, "Excusus Sedini typis Gotzianis." Mr. Mottelay easily concludes that the town meant is Stettin; indeed, he might have pointed out that the volume contains some laudatory verses signed "Gregorius Westphal. super. Dicaster. Pomeranus, Advocatus et Schabinatus Sedinensis Assessor." Here is plainly a Sedinum in Pomerania; and it would be extravagant to doubt that it is the Sedinum of the title-page. Some copies of a quarto edition printed at the same place bear the date 1633. One of these is in our hands. The horizontal water-lines in the first sheet are distant from one another 24 millimetres; in all the rest of the book their distance is 32 millimetres. In short, the paper of the first sheet is different, and the printing of it (including, of course, the title-page) was presumably executed at a different time from the rest. On the whole, it seems probable that this is nothing but the edition of 1628 with a new first sheet substituted, a common bookseller's device. If any person owning a copy bearing the date 1628 will look at page 219, tenth line from the bottom, he can verify or overthrow this surmise. That line, in the edition of 1633 reads: "verint] in contrariam sophantium quorundam stupor, qui cum," from which some words have clearly dropped out. The quarto text is somewhat more accurate than that of the folio; many of the illustrations are incomparably finer; it has the advantage of an index of 23 pages, and it can be had for a reasonable price. The only objection to it is that it omits the asterisks of two sizes which Gilbert put in his margin to draw attention to matter which he claimed as original. But these can now readily be supplied from Mr. Mottelay's translation. A new edition of the Latin, accompanied by a translation and full annotations, is a desideratum. It requires for the execution of it a competent acquaintance with mediæval and Paracelsian philosophy and with the writings of the medical men of the sixteenth century.

Gilbert treats every part of his subject historically. Thus, in his first chapter, he refers to fifty, sixty, or more authors. The reader naturally wishes to know who these were, in a general way, at least, and something of their characteristics; and the passages referred to should be put before him. Mr. Mottelay supplies a few notes which sometimes contain welcome information. But really it was not needful for him to state who Thomas Aquinas was, nor to offer a judgment upon him; nor are we particular to be referred to the page of Webster's, or Worcester's, or Ogilvie's dictionary where some term of ancient astronomy may be found defined, for better or for worse. It is no use telling us to "consult" works of which not half-a-dozen copies exist, and those in another quarter of the globe. Nor do we see the value of such references as this: "Beati Alb. Magni, Ratisboniensis, . . . lib. vii., Lugduni, 1651," where the name of the book is omitted apparently to make room for the long name of the archbishopric. When we are advised to "consult Vincentii Burgundi, *Spec. Mai.*," as we are several times, we understand that the writer usually called Vincentius Bellovacensis is meant. At least a dozen times, Brown's 'Pseudoloxia Epidemica' is quoted, instead of 'Pseudodoxia.'

We notice only a small proportion of the faults which have chanced to catch our eye. On p. 4, several authors' names are wrongly given, as Brasevolus instead of Brasavolus, Hermolaus Barbatus instead of Barbarus, Hannibal Roserius Calaber instead of Rosetius; while Aëtius of Amida is omitted. For Hali Abbas the translator writes "Abohali (Hali Abbas)" in severa [[sic]]

places. Now Hali Abbas is the usual designation of a very famous physician, Ali-ibn-al-Abbas, and Abohali is no part of his name. On the other hand, on p. 76, where the Latin does read Abohali, we have in the translation (it is the word *carab*, meaning amber, that is under discussion):

"In Arabic *carab* means oblation, not *rapiens paleas* (snatching chaff), as Scaliger would have it, quoting from the Arabic or Persian of Abohali (Hali Abbas)."

The fact that Gilbert does not know whether the language is Arabic or Persian shows that he does not know who it is that Scaliger (J. C., of course) means by Abohali. The "Hali Abbas" is Mr. Mottelay's gloss. But the writer Scaliger meant was no other than *Abu-Ali*-al-Hasan-ibn-Abdallah-ibn-Sina, usually called Avicenna. For in this physician's Canon, Lib. II., tr. ii., cap. 364, we read, in the translation of Gerhardus Carmonensis: "*Karabe*, scilicet, rapiens paleas Persice." The translator sometimes writes names in the Latin forms, sometimes in the vernacular, without any rule. Thus, Pietro d'Abano is twice so called, and once, Petrus Apponensis. When we read of "Marbodæus, a Frenchman," we have no idea who is meant; but when we see the name written "Marbodeus Gallus," we recall that eleventh century poet, surnamed Peliciarius. Looking out Marco Polo, in the index, we are referred to p. 11, where we read of Baptista Porta learning some obvious properties of the lodestone "from Messer Paolo, the Venetian." Now, the original has (in the ablative) "R. M. Paulo Veneto"; and the "R. M," or *Reverend* Master, might have sufficed to show that Marco Polo was not meant. Besides, that traveller, in the text used by Yule, says never a word about the lodestone. Turning, however, to the preface to the seventh book of Porta's 'Natural Magic,' the book treating of magnetism, we read:

"At Venice we knew intent upon the same study 'R. M. Paulum Venetum,' at that time provincial of the order of Servite monks, now the very worthy procurator, from whom far from blushing to have learned some things, we glory in it; for we have never come across a man more learned nor more intelligent than he, born for the Encyclopedia, the splendor and ornament not of the city of Venice only, nor of Italy, but of the world."

This description suits Sarpi alone, who, though his baptismal name was Pietro, was *Fra Paolo* in religion, and who was elected provincial of the Servites in 1579, and was appointed procurator in 1588.

The translator's English is certainly very graceful. Perhaps it might, here and there, be advantageously brought nearer to the author's meaning. When Gilbert writes "nihil præter errores et caligines in tam excellenti cognitione commonet," would it not be better to render this, "he suggests nothing in this lofty inquiry but errors and obscurities," instead of "he does but add errors and obscurities to his otherwise excellent treatise"? On p. 17, we read: "hence one loadstone is male, another female." The Latin is: "mas num sit an femina," which seems to ask, "Is it then male or female?" *Plumbum album* is constantly translated "zinc," but it is certain that George Agricola meant tin by *plumbum candidum*, and we do not see how zinc will answer. It is not white, to begin with. It is easy to suppose that Gilbert, like many writers, meant by *stannum* pewter or some alloy. On p. 47, the translator speaks of things "arranged *according to artificial conditions*." The Latin is "artificiose," better translated "scientifically." On p. 58, by way of antidote to poisoning by iron, the translator represents Avicenna as recommending "a drachm of loadstone taken in a draught of the juice of *dog's mercury*." Gilbert says *mercurialis*, by which he must have understood the French mercury, not the poisonous dog's mercury. But we cannot find that Avicenna really made any such recommendation. He proposes that lodestone should be mixed with iron filings when they are prescribed, and should be taken in "melicratum," or mead, not *mercurialis*. Is there not some clerical error? Page 63, Mottelay: "loadstone is *chiefly* earthy." The Latin is *maxime*, and should certainly be translated "in the highest degree."

On page 94, the translation reads, "So, too, a dry body does not run to the dry rim of a vessel containing water; but, on the contrary, a wet object does." The Latin is: "Ita nec ad limbum vasis siccum appellit humidum, sed humidum petit limbum," which may be rendered, "So a wet object does not approach the dry side of a vessel but seeks the wet side." On p. 87, *gemma Vincentii* is translated "vincentina," but what is that? On p. 97, the translation is: "Electric bodies attract the electric only, and the body attracted undergoes no modification through its own native force, but is drawn freely under impulsion in the ratio of its matter (composition)." The Latin is: "Electrica corpora alliciunt tantum, allectum non immutatur insita vi, sed materiæ ratione sponte appulsum incumbit." Perhaps this might be rendered; "Electric bodies only attract; the attracted body is not changed

in its native force, but by reason of matter, being invoked, willingly exerts itself." Gilbert certainly thinks the excited amber by something very like human persuasion induces things to come to it. On p. 123, the translation reads, "And as light--so the opticians tell us--arrives instantly, in the same way *with far greater instantaneousness* the magnetic energy is present within the limits of its forces." The words we have italicized are, in the original, *multo magis*, "much more." That is, we have much more reason to think magnetic induction is instantaneous. Mr. Mottelay's translation would make Gilbert deny the absolute instantaneousness of illumination, which he never dreamed of doing. The phrase *multo magis* is often used in the sense of *a fortiori*. On p. 169 occurs the word *halinitro*; but this is not English. It would be better to speak either of "halinitrum (carbonate of soda)" or of "mineral alkali."

We could easily multiply the number of such criticisms by ten; but the general rationale of the course of experimentation is not affected by such slips. We wish, however, before closing this notice, to make some remarks upon some ruling practices of the translator. *Manifestus* is almost everywhere rendered by "manifest." Now, we have no objection at all to an effort to so turn the meaning of the English word; only the peculiar acception should be explained. *Manifestus*, in the language of physicists, means open to direct observation after making the appropriate experiment. It does not imply that the thing is evident *unless* the experiment is made. For instance, on p. 217: "Here we must express wonder at a *manifest* error of Baptista Porta, . . . to wit, that iron rubbed with diamond turns to the north." Even when the translator selects a different word, he fails to convey the peculiar meaning of *manifestum*. Thus, on p. 212: "It is *plain* (*manifestum*) that all the bars so hammered out toward the north and so laid down while cooling will rotate round their centres, and when afloat will move about in water, and will point north." It is not plain that they *will* behave so; but if the experiment be made, it *will* become plain that they do so behave.

An admiring translator of an old author very naturally and almost excusably falls unawares into a way of slurring over his author's cruder conceptions, and of representing him as more modern in his ideas than he really was. We have seen how Mr. Mottelay, by a most forced rendering, would lead his reader to think that Gilbert believed in the finite velocity of light. Gilbert's ignorance of forces was profound. At a time before Galileo's achievements were known, Gilbert could not be expected to have a knowledge of dynamics, but one might suppose that so perspicacious a mind would have a not altogether erroneous instinctive idea of force. If we compare Gilbert with Dr. John Dee, whose period of activity antedates the 'De Magnete' by thirty or forty years, we find the older man, for whom posterity has had only contempt, to be far better equipped with conceptions of mechanics than the younger. Gilbert comes, in the course of his book, into conflict with each one of the three laws of motion, but most frequently with the law of action and reaction.

Between the words *potestas*, *dominium*, *potentia*, *vis*, *virtus*, *robur*, *vigor*, he draws no clear distinction, as can be seen in book ii., chapter xxix., first paragraph, where all these words occur in the same sense. Yet Mr. Mottelay throws a

garb of modernness over his translation by an incessant use of the word "energy." Take, for example, the following definition. The original reads, "*Verticitas*, vigor polaris, non {peridinësis}, sed {peridineisios dynamis}: non vertex aut {polos}, sed virtus convertens." The obvious translation is, "*Verticity*, polar vigor, not a rotation, but a tendency to rotation; not a pole, but a property of turning." But Mr. Mottelay translates as follows: "*Verticity*. Polar strength--activity (or what in Gilbert's day was understood as energy); not gyrating, vertiginous, but turning power; nor is it polar revolution, but a directing virtue, an innate turning vigor."

Where, in the original, does he find anything about its being *innate*? It is difficult to understand the clause in parenthesis. If Mr. Mottelay means to say that in Elizabethan English the word *energy* was used to translate *vigor*, he is mistaken. The word *energy* was not in use at all, except in the sense of liveliness in writing. The reason it was not used was that the Greek word was perfectly translated by the Latin *actus* and English *act*. As Sir William Hamilton well says, "Energy, act, operation, are convertible terms." Everybody knows that "energy" was the precise contrary to {dynamis}, the word which Gilbert selects to express what his verticity *is*; so that energy is particularly adapted for *avoidance* in translating it. But if Mr. Mottelay means that the word energy will convey to electricians and other modern readers the conception in Gilbert's mind, we venture again to dissent. We admit that the converse is true. If one wanted to give a single word which should convey in Latin to the prescientific man the nearest idea possible to that of our energy, *vigor* might perhaps be the best choice. But when, on the contrary, the purpose is to put the modern man into the state of mind of the prescientific man--the Gilbert--the word "vigor," if it be at fault, is so because it conveys a too scientific idea. "Energy" ought not to be thought of for an instant. Mr. Mottelay seems really to be at some pains to conceal a thing which it should have been one of his chief concerns to bring to light, the contrast between Gilbert's conceptions of magnetic action and those of any scientific man of our time. Nevertheless, we are very much obliged to him for the translation. The book is more than amply worth its price.

58 (8 March 1894) 180-181: FUNK'S STANDARD DICTIONARY

A Standard Dictionary of the English Language.

Prepared under the supervision of Isaac K. Funk, D.D., etc. In two volumes.

Vol. I. New York: Funk & Wagnalls Co. 1893.

CSP, identification: Haskell, *Index to* The Nation. See also: Burks, *Bibliography*; Fisch and Haskell, *Additions to Cohen's Bibliography*; MSS L 159.22, L 159.47, L 159.49, L 159.54.

Isaac Kauffman Funk (1839-1912) was an American clergyman, editor, and publisher. He was graduated D.D. from Wittenberg College in 1860 and was later granted an LL.D. by the same school. In 1876, Funk founded the *Metropolitan Pulpit*, a radical religious journal later called the *Homiletic Review*. Adam W. Wagnalls began working as a clerk for the *Pulpit* and in 1877 became a partner in I. K. Funk & Co., and, in 1891, the firm's name was changed to Funk & Wagnalls. The company's most lucrative publication was the *Standard Dictionary*, here reviewed, with a reported production cost of nearly $1 million.

Great prominence is given in the advertisements to claims for this dictionary of an enormous number of words ("*Johnson*, 45,000; *Stormonth*, 50,000; *Worcester*, 105,000; *Webster* (International), 125,000; *Century*, 225,000; *Standard*, nearly 300,000"), although the strenuous effort of the good lexicographer is to *keep down* his vocabulary. In an ordinary dictionary of reference, 25,000 words comprise all that anybody ever looks out. The rest is obstructive rubbish. Completeness is not to be thought of in any dictionary. Murray's Dictionary is probably the best that ever was made of any extensive language--better than Grimm, better than the Della Crusca, far better than the performance of Littre. He has had the services of some 1,500 readers. Yet his failure to include every vocable can be made apparent in half an hour by running over the pages of a few books of diction anyways marked in character and which are books Murray's readers may probably have passed by. The statement that Funk's Dictionary contains four words to every three in the Century, of however little consequence it might be, seemed to us, considering the process of manufacture of these compilations, to be sufficiently remarkable to be subjected to some control. We have therefore taken, in the Ds, Fs, Gs, and Hs, evenly distributed alphabetic intervals, amounting in the well-proportioned International Webster to the 150th part of the whole, and have counted the words (rejecting mere variations of spelling and other very slight differences) in both the Century and Funk. We find 1,207 in the former and 1,243 in the latter. This would make a total of 181,000 words in the Century and 186,000 in Funk, which we have no doubt is about the truth. Dr. Funk has inserted all the words in the Century Dictionary, with a very few omissions for which no reason is discernible, unless they were made because good manners prescribes that something shall be left. To these he has added, in the first place, a quantity of derivatives. Thus, the Century having cumbered its pages with *Graciliariidæ*, Funk adds *graciliariid* (an individual of the family of *Graciliariidæ*) and *graciliarioid* (like a graciliariid). The next successor in the business will add *graciliarioidally*. In the second place, Dr. Funk has added a number of compound words, mostly of a technological kind, such as *flock-cutter* (a machine for making flocks), *flock-duster* (a machine for cleansing flock by removing ordinary dust), and *flock-opener* (a machine for loosening out bunches of wool-dust, or other flock, in order that it may feed properly to paper that is being flocked). In the third place, in number equal to less than one-seventh of all his additions, are such as really seem to be of value, such as *dadny*, *fawncolor*, *gladsheim*.

One of the first questions to be asked concerning a dictionary is whether it is well proportioned in the sense of doing equal justice to different parts of the alphabet. Since it is useful for many purposes to have a ready means of dividing an English dictionary into approximately equal parts, we give the following table, showing the the proportionate space occupied by different letters in several dictionaries. (The unit is the space occupied by the mean of E, F, G, M, and R, which is estimated for the unfinished dictionaries.)

Int.

	Webster.	Century.	Murray.	Funk.
A	1.49	1.28	0.81	1.63
B	1.25	1.06	1.02	1.22
C--Cni	0.99	1.01	1.00	1.08
Co--Cz	1.31	1.17	1.29	1.15

D	1.39	1.18	.....	1.15
E	0.97	0.92	.....	0.93
F	0.76	0.78	.....	0.77
G	0.85	0.95	.....	0.88
H	1.17	0.94	.....	0.95
I, J	0.97	0.93	.....	0.96
K, L	1.15	1.16	.....	0.98

M	0.93	0.92
N, O	1.06	1.12
P--Pny	1.07	1.09
Po--Quo	1.15	1.14
R	1.01	1.02
S--Shy	0.83	0.82
Si--Spy	0.97	0.86
Sq--Sz	1.30	1.33
T, U, V	0.85	0.71
W, X, Y, Z . . .	0.85	0.79

In almost all dictionaries the As are compiled on a different plan from the rest of the dictionary (or are deliberately increased to make a show). In a thoroughly well-proportioned work, they would occupy a space not far from one unit. Murray's plan seems to have been somewhat expanded after the As appeared. It was between the publication of Murray's second part, concluding the As, and his third, that the 'Century Dictionary' was announced. In the Century and in the International the space occupied by the As is disproportionately great; in Funk's Dictionary it is grotesquely so. In those parts of the alphabet where the dictionary with longer explanations will be most nearly like the briefer work, as in *un-*, *in-*, *de-*, *dis-*, *con-*, the relative space in the Century naturally sinks below that in Webster. In H, which abounds in words calling for long explanations (*hand*, *held*, *hold*, *hang*, *hard*, *he*, *have*, *horse*, *house*, *heart*, etc.) the number for the Century is greater. Funk's Dictionary, owing to its way of packing derivatives and compounds, has proportions closely agreeing with those of the Century. Its As and Bs are disproportionate, owing to its drawing largely from Murray. In order to satisfy ourselves in regard to the causes of the discrepant numbers for A, we have made a careful examination of parts of the above-mentioned dictionaries, together with

the Imperial and some others, in a dozen equably distributed alphabetic intervals in the As and Bs. It is upon this comparison that the above assertions rest.

Dr. Funk has a most imposing array of editors and contributors. March (consulting editor), Newcomb, Dr. Doremus, Max Muller, Titus M. Coan, Huxley, Alexander Graham Bell, Henry M. Stanley, Mark Harrington, Gill, Kunz, F. Horace Teall, Rossiter Johnson, Steinitz, Amelia Edwards, Edward Everett Hale, Edward Morse, Bjerregaard, H. B. Storer, Ray Lankester, Benson J. Lossing, William R. Harper, Harrison Grey Fiske, A. H. Sayce, George W. Smalley, Horace Furness, and many others make a wonderful mixture. But they could not, all of them, put a large part of their souls into pages crowded with "daffadilly‡, daffadowndilly‡, daffodilly‡, daffodowndilly‡, daffydowndilly‡," and the like.

To test the definitions, let us select (without consulting either work beforehand) a dozen words in H, which it seems likely that persons such as would wish to possess a dictionary like this would wish to find definitions for; and for these let us compare Funk's definitions with Webster's:

(1.) *Hæmony*. One of those enigmas with which Milton loved to empuzzle his readers. The two dictionaries give conflicting explanations.

(2.) *Hank*. "Humphrey had this double hank upon her inclinations" (Humphrey Clinker). Webster is slightly the better, and gives an appropriate example: "The devil hath got such a hank over him."

(3.) *Harry*. A common name in England for a cheap grade of playing-cards showing Henry VIII. on the wrappers. Omitted by both Funk and Webster.

(4) *Half-sprit*. "She sailed with a half-sprit, like a yacht." (Defoe.) Neither work explains this.

(5.) *Haw*. What is the haw of a bloodhound? Neither dictionary tells.

(6.) *Headcheese*. Why is it called *cheese*? Neither informs us.

(7.) *Hecastotheism*. One of Major Powell's words, and a good one. Funk has it, the International not.

(8.) *Hector*. Defined by Funk as "A quarrelsome, domineering, threatening fellow"; by Webster as "A bully; a blustering, turbulent, insolent fellow." Neither explains this passage in Pepys: "We had a great many hectors in the same box with us, and one, a very fine one, went into the pit and played his dog, for a wager."

(9.) *Hegelianism* receives in Funk a correct explanation, but one perfectly unintelligible to anybody who does not understand the system. Webster attempts no such statement.

(10.) *Helicoid*. Funk defines it as "A surface generated by a line one end of which moves along an axis while the other end describes a spiral." Now, since some sort of spiral can be described on every ruled surface, the last clause is without effect, and the definer might as well have said, "A ruled surface containing a straight line not a generator." Webster's definition is, "A warped surface which may be generated by a straight line moving in such a manner that every point of the line shall have a uniform motion in the direction of another fixed straight line, and at the same time an angular motion about it." This covers the "developable helicoid," which the other definition (though otherwise far too wide) excludes.

(11.) *Hertstone*. "King John ordered Doncaster to be enclosed in hertstone and pale." (Southey.) Neither dictionary gives *hertstone*.

(12.) *Heterokinesy*. A term much used by Cudworth. In neither Funk nor Webster.

In arranging the various meanings of a word, for the purpose of definition, Dr. Funk adopts the plan of putting the most ordinary everyday meaning first. At first blush, this looks like good sense in the case of a one-volume dictionary, which can make no pretension to serving the turn of those who wish to study the language scientifically, or even that of those who wish to learn its niceties of expression. Besides, he who is considering what words really *do* mean becomes so tired of lexicographers' attempts to *guess* the meaning from the derivation--attempts due to their study of English literature not being sufficient to inform them how that primeval meaning may have become modified in centuries of growth of associations--that he is disposed to welcome a rupture with etymology. But when he comes to see what Dr. Funk's rule leads to, he begins to think that, after all, there is some merit in definitions based on the very early history of words. The spelling in Funk is very nearly the same as that of the Century Dictionary. That the pronunciation is none too conservative is shown by the fact that no alternative is admitted to any of the following: ab'stract (adj.), ad'jectival, bombast, confes'sor, consis'tory, cony, cynosure (atonic sh), disinterested (atonic), dynasty, exhale (atonic), exile (atonic), ex'tirpate, gas (atonic), housewife, im'becile, isolate (atonic).

A few little things that have struck our attention in looking over Dr. Funk's pages may be mentioned.

Burton, in the sense of *ale* ("I am more inclined to think strong beer of myself, cwrw, Burton, audit-ale, old October." Southey) is omitted by Murray, the Century, and all the dictionaries.

Guillaume. Funk does not puzzle us by calling a *rabbit* a *rebate*; but why talk of "joiners"? They are a kind of carpenters hardly known in the breadth of this country.

Diarian. Badly defined. It means pertaining to a diary, an old-fashioned kind of almanac.

Asses'bridge. The figure of the 47th proposition is wrongly drawn.

Face of a polyhedron omitted.

Doddered, a second non-existing word introduced, from not understanding the first.

Blanch, the regular meaning in cookery not given, though right in Murray.

The omissions common to Funk and the Century are very curious unless causally connected. Such are, dissuava (a general), ditch-water (old wine), double-bobmajor, doxoscopy, drop-handkerchief, dubitative (subs.), dudley-nose, dulling, dumrie, dungally, dupper, dyadison, and thousands more.

We should be glad to praise the typography of the Standard Dictionary, but there is no beauty in these pages, and the necessary condensation is extreme.

58 (15 March 1894) 197-199: MATHEMATICAL FUNCTIONS

Theory of Functions of a Complex Variable.

By A. R. Forsyth. Cambridge: University Press; New York: Macmillan. 1893.

A Treatise on the Theory of Functions.

By James Harkness, Associate Professor of Mathematics in Bryn Mawr College, Pa., and Frank Morley, Professor of Pure Mathematics in Haverford College, Pa. Macmillan & Co. 1893.

Traité d'analyse.

Par E. Picard, Membre de l'Institut. Paris: Gauthier-Villars. Tome I. 1891. Tome II. 1893.

CSP, identification: Haskell, *Index to* The Nation. See also: Burks, *Bibliography*; *List of Articles*; MS 1391 a (draft).

This review of Frank Morley's *Treatise on the Theory of Functions* is somewhat blunt and does not reflect Morley's stature in academic circles. Born in England in 1860, Morley was graduated B.A. from King's College, Cambridge, taking his M.A. there in 1886. He returned for his Sc.D. in 1898, following a brief appointment as professor of mathematics at Haverford College. In 1900, Morley became chairman of the mathematics department at Johns Hopkins University and remained there until his retirement in 1928. In addition to his teaching duties, Morley was editor of the *American Journal of Mathematics* from 1900 until 1930. He died in 1937.

Many good people fancy that the advances of mathematics, like those of jurisprudence, become manifest only when the state of things in one generation is compared with that in another; and that they are merely in the nature of extensions of old methods to new cases. In point of fact, there is probably no science in which the rate of *acceleration* of discovery, of the proportion of excess of the discoveries of one year over those of the year before, is so great as it is in mathematics, and no science but mathematics in which discovery seems to be becoming continually more and more fundamental. We are speaking of pure mathematics, not celestial mechanics.

Time was when geometry absorbed so overwhelming a proportion of the studies of mathematicians that "geometer" was understood to be synonymous with "mathematician"; and even to-day geometry is the most studied branch of mathematics. There are various reasons for this. One is that it is a comparatively easy subject. Besides that, the sensuous element of it seduces the mind, and carries it into excesses of study; and other causes there are. But next after geometry, in respect to the quantity of researches annually published about it, and far superior to geometry in its intellectual rank, is the subject of the theory of functions. For the last twenty years and more there has been a perfect freshet of original work in this line. Every year its tide is rising; every year increases the force and value of the new discoveries, which sweep on faster than they can be taken account of. In early days, enthusiasts would sacrifice hecatombs to celebrate the solutions of problems. Later, problems appeared less sublime; theorems were requisite to excite admiration. Now, theorems are as the sands of the sea; original methods alone can command mathematical dithyrambs.

At a not remote period in the history of mathematical thought, a Mystery (with a big M and in the darkest of black-letter) hung over the imaginary unit. It used to be written {sq rt}-1; and what was the square root of a negative? But when it was found that the imaginary unit of algebra was only one of a class of units which, operating upon themselves, in a generalized sense of multiplication, produce -1, the mystery lost its capital; and after the philosophy of ordinary quantity had become better comprehended, the mysteriousness of the imaginary had vanished. The numbers, *one*, *two*, *three*, *four*, etc., are sounds which we have learned to pronounce in a certain order of succession, and which we do pronounce in telling over the individuals of collections. If such a collection is finite, we reach a last individual; and the number pronounced on coming to this last one affords the means of determining whether the individuals of two collections can be made to correspond, one to one, or, if not, in what manner they fail of that. Sometimes the things counted are really in succession like the numbers. Such are trees in a row, degrees of temperature, and years. In other cases, the succession of counting is purely artificial, as in enumerating the population, or the pounds of flour in a barrel. But the counting does not, on that account, cease to be useful, because, in whatever order the individuals are counted, the final number will, in counts of any one collection, always be the same. Even the separation into discrete units (as the gallons of water in a lake) may be artificial, provided that, if it were effected in various ways, it would always lead to the same resulting number. It will be noticed that this is not a nominalistic account of numbers--it does not make them *flatus vocis*, only--but it makes their existence *in re* consist in an experiential constancy; that is, it assigns to reality three elements, (1) sensuous quality, (2) compulsiveness, (3) generality. Besides the system of whole numbers, we often make use of a scheme of quantity connected together like the points on a line. This is useful even when there is no perfect continuity in the things to which it is applied. The scheme of imaginary quantity is simply one that is connected like the points of a plane. Certain natural phenomena, especially in hydrodynamics, correspond exactly, in theory at least, with such a scheme. But since any line upon such a plane is connected like ordinary real, continuous quantity, the usefulness of imaginary quantities extends to almost all cases in which real quantity is used.

The questions to which the theory of functions, so far as yet developed, chiefly addresses itself arise out of the supposition that a correspondence between the points of two different planes of quantity has been established by an equation. It considers the nature of the resulting continuity (so far as this is not resolved in the theory of plane curves), and, more especially, the modes of representing the relation, both geometrical and analytical. The main object of the whole study is to find out how to make use of differential equations, especially such as are the immediate dicta of mechanical laws.

The disciplinary value of the theory of functions is superior to that of any other branch of mathematics. For many minds elementary geometry serves, directly or indirectly, as their model of reasoning. But elementary geometry is so artificial,

and is so permeated with fallacies and caprices, that it must be and ought to be difficult to a healthy and ingenuous young mind; and much of the perverse logic that is current in the world is to be laid at its door. Albegra has done much for every educated man; it has given him an exemplar of perfectly accurate abstraction. It would put a mighty weapon in his hands if the application of it, in the elementary books, were not pretty much restricted to two problems, elimination between linear equations and the solution of the quadratic. The theory of probabilitites [[sic]] is most instructive and useful, but that is only applied algebra. The theory of numbers is an admirable school of reasoning, as far as it goes, and it goes so far that reflection upon it will counteract much of the poison that the text-books of logic inject into the current of thought. Projective geometry imparts the most precious secrets in generalization while making no fundamental analyses. As for analytical geometry and the calculus, all *that* ought to be taught (as in Prof. Benjamin Peirce's 'Curves and Functions' it *was* taught nearly fifty years ago) as part of the general theory of functions. The theory of functions is, in the first place, intrinsically, quite easy-we mean to follow, not to invent. Of course, it is capable of being obscurely stated. Its logic is the most fundamental conceivable, and, at the same time, is the very subtlest that can anywhere be found; so that no man is too eminent never to have made a slip in it. The outlines of the theory ought to be known to every educated person.

There has hitherto been no treatise in our language on the modern development of the theory. At length the same year presents us with two. Though the first has 700 pages of royal octavo, and the other 500 of common octavo, yet the subject is so vast that a considerable part of the contents of either is excluded from the other, and much that we might desire to see is absent from both. Dr. Forsyth has been well known for some years less than half a generation as an indefatigable investigator of functions, and he has already produced two profound treatises on differential equations. The present work contains many not unimportant contributions of his own Messrs. Harkness and Morley are younger men, but, as this volume shows, thoroughly versed in their subject. Dr. Forsyth keeps as much as he can to the general theory, treating such a special subject as elliptic integrals, for instance, with the greatest brevity possible, and at the same time in such a way as to afford a bird's-eye view of it. Messrs. Harkness and Morley, on the other hand, seem to have been of the opinion that it was better to go somewhat deeply into a smaller selection of topics. Many things are crowded out to make room for long chapters on Elliptic and Abelian Functions, while, at the same time, these very subjects are not treated with all the fulness which is requisite for the practical applications of them. This, at least, is certainly true of elliptics. Practical applications of Abelian functions ought, perhaps, in the present state of things, not to be thought of. Certain preliminary branches, absolutely indispensable to the comprehension of the theory of functions, such as the logic of infinity, continuity, etc., and the doctrine of the convergency of series, are entirely omitted by Forsyth, while he inserts matter about substitutions which the reader will be glad to find thus at his hand, but which really belongs in a treatise on algebra. The other writers have followed the opposite course in these respects, though we cannot quite content ourselves with their

attempted reproduction of Cantor's logical ideas. Dr. Forsyth imitates, in a general way, the French lucid style of exposition, though the French accuracy of statement and neatness of demonstration are often wanting in his book. Messrs. Harkness and Morley express themselves in the German manner, which makes the exposition as easy as possible for the writer--and never mind the reader. For an illustration of what we mean it is sufficient to open the book at random. At the top of p. 352, we read (with a slight modification of notation for our printer's sake):

"The symbol MN equals 1 or 0, according as M and N do or do not contain a common letter."

Now, it is inaccurate to speak of a *symbol* being equal to a number; and since M and N are single letters, there can be no question of their containing a common letter. But the authors mean that when they are replaced by the duadic symbols which form one of four or five different ways of expressing the same thing, *those symbols* have or have not a common letter, according as the corresponding quantities equal 1 or 0. The opposite page, 353, presents several singular instances of saying one thing while meaning another; and it is stated that a certain notation "will be" used, which notation is incontinently dropped without another word, and another one, not defined, is constantly used for many pages. So, on p. 355, a notation is defined, in no apparent connection with anything in the vicinity, and is never used for many pages, until it suddenly springs up after we have forgotten all about it. These examples are not culled.

It would be unfair to convey the idea that Forsyth is quite impeccable in his expressions. This is far from being true. Thus, at the beginning of chapter iii., in enunciating Cauchy's fundamental theorem on the expansion of a holomorphic function, the important words "unconditionally and uniformly," as describing the mode of convergence, are omitted, as they are overlooked in the proof given. The first page of chapter vii. has but sixteen lines of text, yet they contain no less than three faults of expression, if not of logic. Indeed, Forsyth is really too negligent in regard to terminology. Thus, that category of surfaces, curves, etc., which the Germans call *Geschlecht*, the French *genre*, and which we should do well to term *genus*, instead of the usual word "deficiency," Forsyth most confusingly designates as the *class*. Both books will be found serviceable to students, alike to those of higher and of lower grades. We may mention, by the way, that Forsyth is rich in illustrative examples, Harkness and Morley pretty poor. But we cannot sincerely pronounce either of them quite satisfactory, whether as a handbook or as a textbook, and both handbook and textbook are certainly needed. The latter ought to be so clear of all pedantical details as to be fit for the use of every young person who seeks a broad intellectual education.

Picard's very admirable work is not properly a treatise on the theory of functions. Indeed, when the first volume appeared, the author's purpose was to treat this subject rather slightly, and we were informed that volume ii. would deal with Differential Equations. Instead of that, only one of the seventeen chapters relates to that subject. It is evident the theory of functions has been growing in importance in the author's mind. Hence it is, probably, that instead of embarking frankly in the vehicle of imaginaries, the author thinks it more philosophical to deal chiefly with real functions, thus making many things difficult and crabbed which would in Cauchy's hands appear delightfully facile. The work is one of truly considerable power. It cannot justly be called a classic. Members of the French Institute are apt, for an obvious reason, to be over-praised. While we admit the great value of this work, we must say that some comments upon it we have seen from men to whose opinions we should naturally be inclined to defer, appear to touch the point of extravagant laudation.

58 (29 March 1894) 234-236: LOCKYER'S DAWN OF ASTRONOMY

The Dawn of Astronomy: A Study of the Temple-Worship and Mythology of the Ancient Egyptians.

By J. Norman Lockyer. Macmillan & Co. 1894.

CSP, identification: Haskell, *Index to* The Nation. See also: Burks, *Bibliography*; Fisch and Haskell, *Additions to Cohen's Bibliography*; MSS L 159.52-53, L 159.55-56, L 159.58; MS 1392 (draft).

Sir Joseph Norman Lockyer (1836-1920) was for many years director of the Solar Physics Observatory and professor of astronomical physics at Royal College of Science. In 1866, he made pioneer observations of the spectrum of sun spots and, in 1868, of solar prominences. Lockyer coined the terms "chromosphere" and "helium."

Mr. Lockyer's power over the enginery of hypothetical reasoning--that which searches out the explanations of observed facts--is, with reference to astronomy, acknowledged by all Europe. When he first entered the field of astronomy, it was as an amateur, and he now audaciously leaps into another arena where there was no thought of his apparition. The principal object of his book is to advance the hypothesis that Egyptian temples were generally oriented to the risings and settings of stars, so that a few priests, standing in the windowless cell which is the holy of holies of every such structure, and looking out along the narrow axal aisle of pillars, would be looking in the right direction to catch sight of the temple's star at its rising or its setting.

Let us see how much there is to make this a reasonable suggestion. Certainly an intimate connection subsisted between the theology and the astronomy of Egypt. Thus, one of the hieroglyphic determinatives for a god is a star. That is to say, a scribe having to suggest the idea of a god by means of a simple picture, draws a star as likely to effect his purpose. What could better betray the Egyptian way of thinking? The very name of the god of earth, Seb, coincides with the usual word for a star (*sba*, written *sb*); at least, so that god's name has usually been read--Brugsch thinks it should be *Geb*, which would destroy this argument. But though Seb, or Geb, is god of earth, there is no doubt the planet Saturn was appropriated to him. It is needless to dwell upon such points, because the idea of the gods having stellar affinities is commonly entertained by Egyptologers.

A certain importance was attached to the orientation of temples. The notion that a man ought to face some particular object when he prays belong to most religions, if not to all. Everybody knows that our churches look the oldest ones to the east, later ones to the west. We learn from Mr. Lockyer that the tradition is, they should be directed toward the sunrise of the patron saint's day. Many temples in Egypt are known to be oriented with precision. In particular, the pyramids of Gizeh, which have been accurately surveyed, are out of position by only 4', 5', and 14', and we remember that 1' is the *minimum visibile*, 31' the mean diameter of the full moon on the horizon. We know, too, from the inscriptions, that the determination of the azimuth for a temple was a matter of ceremony quite as much as our layings of corner-stones, the king being sometimes called upon to perform it; and in this ceremony a star was observed.

We know that the risings and settings of stars were carefully observed by ancient astronomers. The poem of Aratus, which, though itself relatively late, reproduces archaic observations, is in evidence. The last 200 lines are entirely devoted to describing what stars are on the horizon simultaneously at different times. When the eastward movement of the sun among the fixed stars has brought a given star far enough to the west of the sun for its rising to be observed before sunrise, the star is said to "rise heliacally"; and it was by the heliacal risings chiefly that the progress of the seasons, the times for planting, etc., were ascertained. In Egypt this was particularly necessary; and it was by the heliacal risings of Sirius, and later by the cosmical risings, or risings at the same moment as that of the sun, that the date of the inundation was predicted. It would be natural to build temples so that they might serve as observatories in which such risings could, owing to the exclusion of stray light and the direction of vision to the right point of the horizon, be conveniently observed.

Thus it would seem that the hypothesis is antecedently quite probable. That which gives it importance is the fact that the rising point of every star oscillates north and south owing to the precession of the equinoxes. In the course of 130 centuries the shifting will amount to from 54° to 73° for different stars, or about half a degree per century; and in special cases it may become indefinitely rapid. Consequently, if we knew that a hundred temples of the old kingdom of known orientation had been oriented to the risings of known stars with a probable error of 1° each, we could astronomically determine their average age with a probable error of only 20 years. Now there is a good 500 years of doubt at present affecting the dates of the old kingdom; and our general conception of the course of civilization and of the nature of man depends quite considerably upon the manner in which this doubt is to be resolved.

Mr. Lockyer reports that there are but a few temples whose azimuths are sufficiently known for the purpose of testing his theory. Still, he instances 7 which he supposes directed to the rising of Sirius, 12 to that of Phact, 9 to that of Hadar, 3 to that of Dubhe, 7 to that of Etamin, 4 to the setting of Canopus, 5 to that of Capella, and 2 to that of Spica. For each of these forty he calculated the date of orientation; and upon comparison with history he finds that in every case his calculated date either agrees with that of history or antecedes it. But he well remarks that it is quite possible that many temples were erected upon the foundations of previous structures whose orientation they followed, so that the

cases in which his date antecedes the recorded date are not to be counted as against his hypothesis. We add, however, that neither are they to be counted in its favor; so that the result thus far is that Mr. Lockyer's theory is unrefuted by facts, and is even in small measure supported by observation. Considering its great importance, if true, the verdict should be that Mr. Lockyer has done enough to warrant the expenditure of the time of some of the foremen of the world's work and of sufficient money to obtain sufficient data to put the question at rest.

But while we are quite in earnest as to this, we cannot blind our eyes to certain serious difficulties in the way of the theory. For example, Mr. Lockyer finds five temples oriented to the setting of Capella. One of these is the temple of Ptah at Karnak. Another is a temple believed to be dedicated to Ptah at Memphis. Concerning the cult at the others there is little evidence. But why should a temple of Ptah be directed to the setting of Capella? The so-called circular zodiac of Denderah, now in the Louvre, shows us in the place of Capella a mummified cat held out in the hand of a man wearing ostrich feathers. The projection of that zodiac is accurately enough followed to allow no doubt about this. The cat we know was the emanation of the goddess Bast, who was called the beloved of Ptah. Accordingly, if the temples of Ptah had been directed to the *rising* of Capella, we should see no difficulty. But Mr. Lockyer is positive that a star must be either male or female; that if female only its rising is to be worshipped, if male only its setting. It may be said that the figure on the Denderah planisphere that holds the cat represents Ptah. But is that likely? The scarab was Ptah's emblem; and there is said to have been an Egyptian constellation of the scarab. If so, it has been confounded by the Denderah artist with Cancer, which is misplaced where we put Leo minor. In fact, the stars of Leo minor, with a few from the feet of Ursa major, make a very good scarab, as anybody can see these evenings in the sky. This must, then, have been the part of the heavens sacred to Ptah, and it is unlikely there was any other.

Again, Mr. Lockyer makes no less than twelve temples directed to the rising of Phact and seven to that of Etamin. The former is a star of two and three-quarter magnitude, the latter fainter than two and one-third. The risings of such stars cannot be observed, we believe, even in Egypt. Only very bright stars can be seen to rise. Moreover, Etamin and Dubhe were so near north, and their risings and settings consequently so very oblique, as to be indeterminate and unattractive phenomena, which it seems very unlikely persons would be led to observe.

But our objections do not stop here. As long as astronomers busied themselves with the *times* of rising of stars chiefly without much attention to their *amplitudes*, or directions on the horizon, we can readily understand how the precession of the equinoxes could escape detection. But when they had built a few costly temples on purpose to have stars appear at their risings in the exact axes of long corridors, which temples after a few years (in the case of Dubhe after twenty years only) were found to be out of place by a whole degree, the king who had paid for them and who had, perhaps, devoted his personal attention to their orientation, would be likely to grumble and to appoint an investigating committee. Nor could the fact by any possibility be kept quiet. The precession of the equinoxes ought,

then, one would think, to have been discovered and published. Mr. Lockyer supposes that it was indeed discovered, but that "the priests"--which can here only mean the whole body of the priests, say perhaps a tenth of the population--kept the matter secret. Very well, this is pretty violent, but let us suppose that it was so. Did they, then, still go on orienting their temples to the rising of Dubhe, knowing the star would not stay in that place for one generation?

Mr. Lockyer supposes that rival sects used to scheme to build their temples in each other's way, so as to prevent those others from seeing the worshipped star at its rising: and he thinks temples were abandoned and their cult put an end to in that way. At the same time he thinks that when temples were rebuilt at vast expense, the builders simply followed old lines of orientation, without taking the trouble to change them, although they could see the risings would not occur in the directions of the new temples, and even knew the principle by virtue of which there was sure to be such failure. These parts of the theory, we think, will have to be modified in some measure, even if any part of it stands. Whether, after such modification shall have been made, it will retain any great value as an arbiter of chronology is a question not to be hastily answered.

We were speaking of Ptah. Ptah was the god of truth. But that characteristic of truth which is particularly emphasized in the myths and symbolism of the god is unchangeability. Now it is unquestionable that absolute truth, if one could attain to it, is an eternal rock; but attainable truth, the most enlightened opinion, is distinguished from that which is cruder and less truthful by its life and growth and metabolism. When the Egyptians discovered the year had 365 days, they induced the king to enter the temple of Ptah and swear by all that was truthful that he would never change the length of the year nor allow any of his successors to do so. Soon it was found out that the year was 365 ¼ days, and matters of great concern were getting thrown into the utmost confusion. But what was to be done? The king had sworn in the temple of Truth. He had bound his successor and bound that successor to bind his successors for ever. So they kept on with the old year. In like manner it may be that when the king had once oriented a temple and had sworn to Ptah that that orientation never should be changed, the Egyptians may have conceived that it had got to stay so. They had a sort of the-boy-stood-on-the burning-deck idea of truth. But when it had once become quite clear that the changeless orientation was wrong, there would no longer be any stinging incentive to strenuous endeavors for the highest accuracy of orientation.

The Egyptians were priests or engineers or physicians or artisans. They were always pursuing a practical object, without unnecessary generalization. They had a decided distaste for generalization carried beyond practical needs. They hated to see things more regular than there was any use of their being. Right angles bored them. Longfellow said he *hated* science, and so he did; and so did the Egyptians. The greater their cleverness in solving practical problems, the more it brings into relief their utter incapacity for science. The mathematical papyrus is a marvel of inaptitude. The medical papyrus, recently translated, is beneath contempt. Claudius Ptolemy, if he is to be considered an Egyptian, is the only one of the race who ever showed scientific genius. Their weakness is put into a strong light when they are compared with the Chaldees, who, even when magic was their only lore, pursued that study with all the method and industry of modern savants. In particular, the Babylonians were great archæologists; and it is to the diligence with which some of them searched out their own history that the chronological accuracy of our knowledge about it is due. The Egyptians, on the other hand, cared nothing about fact. They covered the walls of their buildings, not with records of the past, but with dreams of what they would do in a future life. They loved to boast that their writings had been found in some ancient tomb or under the statue of some early king, because to their minds what was old and what was true were indistinguishable; but to veracious history they were indifferent. Of course, there are aspects--namely, all practical and theoretically ethical aspects--under which the Egyptians appear as a very admirable people; and Egyptologers become so seized with admiration for them that they are generally blinded to their scientific weakness. Erman alone, in his excellent popular work, 'Ægypten and ägyptisches Leben im Alterthum' (Tübingen, 1885), has told the plain truth.

We refer to this side of the Egyptian character here because we infer from it that it is most unlikely that that people persisted for ages in such scientific accuracy in the orientation of their buildings as to justify any inferences from it involving nice calculations. Lockyer adduces the circular zodiac of Denderah as evidence of the scientific accuracy of the Egyptians, saying (what we fully admit) that that zodiac must have been copied from a regular map of the heavens constructed about 700 B. C. That was Biot's discovery. But Lockyer rather innocently takes it for granted that that map had been made in Egypt. Not only is this gratuitous, but the nature of the projection proves that it was an importation, for it is a central projection from the vertex of a cone tangent to the sphere on the circle of perpetual occultation for a latitude little less than 40º; so that it cannot show any of those stars that are so far south as to be visible only at lower latitudes. We have been to the trouble of trying how a similar projection suited to the latitude of Denderah would look, and find it quite unlike the celebrated planisphere. Most likely, then, the original was executed at Nineveh (latitude 36 ½º) in the reign of Sargon, when the Egyptians, as we otherwise know, after the fashion of defeated nations, were eager to acquire that knowledge that had gained ther [[sic]] adversaries the victory of Raphia, 719 B. C. The copy set up at Denderah in the second century after Christ cannot well have been a copy at first or at second hand, and in it Egyptian constellation figures were substituted for the Assyrian ones, except in its most scientific part, the zodiac, all the figures of which are of Chaldean origin, barring the figure in the place of Cancer, and possibly Libra. But when Mr. Lockyer asks us to judge the astronomy of the Egyptians by this monument, we must in fairness assent to that. That that people, setting up such a costly piece of work in an eminently scientific age, knew no better way than to copy the record made in Assyria eight centuries before, without glancing at the heavens to see what enormous changes the precession of the equinoxes had brought about in the interval--that, we must confess, does indeed speak volumes in respect to the degree of assiduity with which they had been prosecuting the

work of astronomical observation. It is as if we had no star-catalogues better than those of Sûfî and Ulugh Beg; or as if those astronomers had done nothing but make servile transcripts from Ptolemy.

We cannot leave the book without calling attention to the perspicuous and valuable discussion of the Sothic cycle. Some of the chapters upon the religion, on the other hand, seem to us, we must say, to push theorizing further than good sense can go.

58 (12 April 1894) 278: **The Monism of Man; or, The Unity of the Divine and Human**.

By David Allyn Gorton, M.D. G. P. Putnam's Sons. 1893.

Genetic Philosophy.

By David Jayne Hill, LL.D. Macmillan & Co. 1893.

CSP, identification: MS L 159.61. See also: Burks, *Bibliography*.

David Jayne Hill (1850-1932) graduated valedictorian from Bucknell University in 1874 and five years later became president of that school. In 1888, Hill left that position to become the second president of the University of Rochester. He was appointed assistant secretary of state by President McKinley in 1898.

Numberless men write about philosophy without dreaming that any further qualification is necessary than having read a quantity of books on the subject and having something they would like to say. Not desultory reading, but hard, systematic study, with accurate reflection, with continual comparison of one's own ideas with those of the strongest men, to see if one's own are capable of improvement in definiteness and force, and this training carried on for at least half-a-dozen years steadily, are the minimum requirements for enabling a man to address the philosophic world without making himself a nuisance. When this point has been reached, if the philosopher will only take the time to write his book short, saying what he has of new to say in the smallest compass in which it can be stated with perfect clearness, he will earn the benedictions of his readers.

Both the above volumes contain eclectic hodge-podges drawn from authors of various merit, but mostly weak. The writers display a charming unconsciousness of the problems their originals had in mind, and frequently quote passages whose real significance they quite miss. Each has been reading and thinking, not altogether without good sense, until he has collected a bookful of reflections of no value, which he now inflicts upon the public. Dr. Gorton, though a medical man, quotes Eduard von Hartmann as authority for a fact of physiology. He speaks disrespectfully of Hahnemann, as "a peer among peerless physicians"--unless this is meant to convey a compliment. After an inconsequential chapter on "The Unity of Divine and Human Agency," he gives two replies to himself, one signed *Scientist*, the other *Theologus*, both in his own incomparable style of thought and diction, peers of his peerlessness, and then replies to them, signing his reply *Radicus*. If he had made it *Radiculus*, it would have come somewhere near a real word and an appropriate one.

Dr. Hill is president of the Baptist University of Rochester, and as such is entitled to be treated seriously, though we must seriously say he sheds no lustre upon

his institution by writing about matters which he does not understand. Thus, he seems to imagine that the discovery of Neptune is somehow to the credit of "Bode's law," although that "law," which good logic never would have admitted, was finally exploded by the violent violation of it in the case of Neptune. The only thing it had to do with the discovery was to suggest that the planet might be found in a place very remote from that where it was found, and thus probably to cause Leverrier to overlook the possibility of the perturbations of Uranus by Neptune being of the unusual kind which it turned out that they were. Though we have only space for this instance, there are plenty of other proofs of Dr. Hill's incompetence to treat of his subject. It is a pity, because the title of his book and those of several of the chapters are decidedly appetizing. But we can find nothing in it of any value.

58 (19 April 1894) 299: An Elementary Treatise on Fourier's Series and Spherical and Ellipsoidal Harmonics.

By William Elwood Byerly. Boston: Ginn & Co. 1893.

Lectures on Mathematics, delivered in August and September, 1893, at Evanston, Ill.

By Felix Klein. Reported by Alex. Ziwet. Macmillan & Co. 1894.

CSP, identification: MS 1391b (draft). See also: Fisch, *First Supplement*.

Aside from possessing a gift for the verbal expression of what Peirce recognized as a difficult subject, William Elwood Byerly (1849-1935) also possessed great foresight as an educator. He graduated A.B. from Harvard in 1871 and two years later received his Ph.D. there-the second such degree awarded at Harvard. Byerly held a professorship in mathematics at Harvard for many years and, upon his retirement in 1913, was made professor emeritus. Among his many significant achievements, Byerly is remembered as one of the first members of the Harvard faculty to give courses to women when the Harvard annex (later Radcliffe) was founded in 1879.

Notwithstanding its name, so redolent of Helicon, there is mighty little poetry in Spherical Harmonics. The blessed, after a thousand years' performance on harps, may possibly betake themselves to setting one another problems in modern geometry; but to spherical harmonics we may confidently assert they will not resort. This subject might be called the conveyancing of mathematics, since it teaches how to express facts in a form which, though it affords no insight into causes or essences, but on the contrary is blind and bewildering, is for all that quite indispensable for making the mathematician master of his data. The usual problem is this: A certain quantity has a value at every point of some surface--most usually, that of the earth. This value--it may be elevation above or depression below the sea level, or the distance of the sea level from the centre, or the force of gravity, or a magnetical constant, and so on--has been ascertained at many points, and is assumed to vary continuously. (Most experts will say no such assumption is made.) Then, spherical harmonics shows us what we may presume to be the approximate values at points where the quantity has not been observed. Moreover, it affords a general expression for the value; still further, it shows how to cut up the quantity into parts,

each of which is susceptible of further mathematical treatment. It is, thus, a theory (for so mathematicians use the word theory) of great utility; and, like other utility-mathematics, is tedious, difficult, disagreeable, and unbeautiful. This is a circumstance which breeds many loathers of mathematics, because these disagreeable branches are taught first.

The present treatise is undoubtedly the best in our language upon this subject. Its only rival, that of Todhunter, always an unnecessarily dry book, is now pretty antiquated likewise. Mr. Byerly adheres to one point of view pretty consistently, exhibits the doctrine under its best aspect, and leads us into it by the easiest road. It is a branch which nobody but a practical mathematician will care for, and which every practical mathematician has to master.

When we turn from this book to Klein's lectures, we seem to be passing out from a tremendous, rattling factory, with its grimly earnest, unlovely economy, into the pure meadows with the really vastly greater, but infinitely calm, agencies of sunshine, breeze, and river. Here, in only a hundred pages, the moving impulses of modern mathematics are set forth in a way in the highest degree instructive and interesting to every mathematician, without any tax upon his energies. Felix Klein, we need hardly say, is generally considered as the most interesting, if not the greatest (certainly *not* in all respects), of living mathematicians. For such a hundred pages as these the mathematician may search in vain. The small compass renders the process of mathematical cogitation all the clearer, and strips it of details which in other books obscure it; and particularly of details of demonstration that are often wrongly taken to be the soul of mathematical thinking. Such a lesson as this book affords of the conduct of mathematical research the younger student (it is not for beginners) will not easily find. Those who know Klein need hardly be informed that the lectures range over a large part of recent mathematics. The following passage (in which we take the liberty twice to put *experience* in place of "conception") is interesting:

"We are forced to the opinion that our geometrical demonstrations have no absolute objective truth, but are true only for the present state of our knowledge. These demonstrations are always confined within the range of space experiences that are familiar to us; and we can never tell whether an enlarged experience may not lead to further possibilities that would have to be taken into account. From this point of view, we are led in geometry to a certain modesty, such as is always in place in the physical sciences."

Appended to the lectures are ten pages on the history of modern mathematics in Germany.

58 (26 April 1894) 316-317: CAJORI'S HISTORY OF MATHEMATICS

A History of Mathematics.

By Florian Cajori. Macmillan & Co. 1894.

CSP, identification: MS 1393 (draft). See also: Burks, *Bibliography*.

A brief sketch of the course of human activity in a way that has been of more avail than many a more showy trade in moulding men's daily lives and sentiments into what they are to-day, this book will attract the attention of every youthful student of any mathematical science or of engineering. For any man in such a line, who has the ambition to be more than the merest specialist, needs to be familiar with as much of the history of mathematics as can be crammed into one moderate volume. Others, students of humanity, to whom nothing can be more interesting and even entertaining than mathematical history, will find profit in this work unless it is too small for their purposes. We noticed a few years ago a volume on the same subject by Walter W. Rouse Ball, entertaining enough, but not a careful nor competent work. That contained perhaps 900,000 words; and the present compendium has, we estimate, about two-thirds of that number. It is too brief, but economizes space pretty severely.

The mathematics of the nineteenth century was treated by Ball not too seiously [[sic]]. His chapter on the subject, sooth to say, had an old-fashioned English flavor, and about as much thoroughness as one would expect in a collection of Lives of Eccentric Personages. Prof. Cajori, on the other hand, treats recent mathematics worthily, giving to it, as was proper, a quarter of his whole space. In this part at least his work is distinctly of a higher grade than Ball's and has evidently cost him original labor. While excessively condensed, as the fair proportions of his whole history required, it fulfils well enough the needs of students before the fascination of a deeper study of the history of thought has drawn them further into its vortex. We will not say an extraordinary, but a satisfactory, degree of discrimination and accuracy marks this section of the work.

Room could be made for this expanded account of the achievements of this century only by an extra turn somewhere upon the hydraulic press with which the rest has been baled. The part which seems to have been chosen to undergo the extremest compression is the period from Descartes to Euler. One cannot but regret this, for in some respects it is the most important period of all; and no other, certainly, calls more for those elucidations of the historian which show the inexperienced student in what the extraordinary advance of different steps in the progress of thought consisted. The main facts are given by Cajori, and often more correctly than by Ball; but there is no room to suggest reflections, and reflections make the worth of history. Take, for example, that truly gigantic reasoner, Fermat. One of his exploits was a method of ascertaining that value of a variable that renders the value of a given function the greatest or least in that vicinity--a method which, in the opinion of some of the greatest judges, constitutes Fermat the author of the differential calculus. Cajori states the matter so very briefly that the young reader will fail to perceive that the method is almost exactly the same as that pursued in the best modern treatises. The following are the steps of the process: (1) to the variable is given an increment which is assumed to be such that the value of the function before the variable is thus increased can be equated to the value after the increase; (2) by development and transposition, the increment is shown to be a factor of the whole equation; (3) the equation is divided by this increment, which will, in general, leave some parts of it independent of the value of the increment and some parts not so; (4) the increment is put equal to zero, and the value of the variable is then deduced. This is open to the objection that if the increment equals zero, we have no right to divide the equation by it. This objection is flanked in modern works by supposing the increment *is not* zero, but diminishes so as to have zero for its limit.

How very clever Fermat's method was is a thing the beginner needs to have pointed out. It was an idea sufficiently clever to change the whole face of the globe, with our daily mode of living and of philosophizing, to such an extent that the Egyptian under Khufu lived and thought more like a sixteenth-century man than the latter like us. For that all modern science has grown out of the germinal idea of the differential calculus we hold to be most surely true. The {ageömetrëtos} as Plato used, it appears, to call the twaddler in thought, may laugh, not seeing how a prolific conception can assume a guise so humble. He would have a great idea essentially warm, picturesque, colored. He refuses to take seriously a pure intellect, whose forms are as applicable to a game of dominoes as to the struggles of world forces. We instance these as examples of the sort of reflection which, whether finally accepted or rejected, a good history of any wide branch of human thought ought to suggest, if not to discuss.

The above was not Fermat's greatest contribution to the art of reasoning. The greatest was the Fermatian inference, characterized by Prof. Cajori as "an inductive method," but that is quite to miss its essential peculiarity. We find Fermat's not publishing his work ascribed to an "uncommunicative disposition"; but this is thoroughly unjust.

We meet reiterated here for the thousandth time that tasteless objection to Pope's monumental couplet on Newton, to the effect that the important steps of discovery do not take the world by surprise, but were led up to so gradually as to be made almost unawares. It is the worst of German taste to criticise such a couplet because it does not accord with profound historical researches, so long as it expressed what seemed to ninety-nine out of a hundred of Newton's comtemporaries [[sic]] to be the truth. Moreover, the theory of intellectual development on which the objection proceeds is in silliest conflict with psychology and with history. No doubt a very large part of the progress of science is accomplished by industrious hammering away at plain jobs. But there is a part which cannot so be accomplished. To say no great strides are ever taken is in flat contradiction to the record. It is by no means as yet proved that even biological evolution always advances by almost imperceptible differences. It is a recognized principle of modern biology that the nature of the history of the evolution of the race may be judged from that of the evolution of the individual. Now, besides the daily growth of individuals, we see them passing through wondrous and sudden transformations. Of course, there is no absolute breach of continuity, but for that no man contends.

Be the matter as it may in the natural world, the development of thought being open to study, from both outside and inside, from the world's history and from the individual's experience, men of sense and observation ought by this time to have reached some settlement of opinion on this point; and so, perhaps, they have. Doubtless there are men who are never surprised, but whether the wise count

them among their number is not so clear. At any rate, he who attempts to expound the history of science, and à fortiori the history of mathematics, without recognizing that great, startling, and revolutionary discoveries from time to time get made, will have but a wrenched, unjointed, and enfeebled account of it to offer to his readers. We sometimes hear such facts as the knowledge of the intoxicating property of ether before Morton, of a few observations of spectra before Bunsen and Kirchhoff, and the like, put forward to show that no great discoveries are startling. But these instances, if they prove anything, prove just the reverse. They show that, as Whewell said, observations are nothing to a mind unstored with appropriate ideas, and thus emphasize the importance of originality of mind, of which novelty is the product.

Prof. Cajori furnishes a list of one hundred works on the history of mathematics that will be very welcome. These are the works of which he has made use. We notice he has not had the advantage of using Boncampagni's *Bulletino*, nor the *Bibliotheca Mathematica*, as he would have done if he had worked in New York, where it is for the interest of the public that men devoted to the history of mathematics should be stationed. The Astor Library is particularly rich is this direction. We do not find on his list Weissenborn's book on the introduction of the Arabic numerals by Gerbert (1892). We remark, by the way, that one class of events to which some attention might well have been paid is that of the establishment of the several journals of mathematics.

Prof. Cajori's transliteration of Arabic is irregular and puzzling. Speaking of the celebrated algebra of Muhammed ibn Musa, the Khivan, he says: "The name of the author, *Hovarezmi*, has passed into *Algoritmi*, from which comes our modern word *algorithm*." The appellation he refers to as the "name of the author" is *al-Khwarizmy*, meaning 'the native of Chorasmia, or Khiva.' He writes *al-jebr* (algebra) in the form "aldshebr," putting *dsh* for the letter *jim*; but for the native of Khojend, or the Khojendy, he writes *Al Hogendi*, using a soft *g* for the same purpose; and in writing *Abul Gud* and *Abu Gafar Al Hazin* (where "Gafar" is the Ja'afer so familiar in the Arabian Nights) he makes a hard *g* serve. He also writes *Ulug beg* where the English *g* stands in place of a second and a third letter of the Arabic alphabet (the last being the Persian gaf). This cognomen of Muhammed Taraghay is best written Ulugh Beyg. It means "the great lord." We read: "Creditable work was donce by *Fahri des Al Karhi*, who lived at the beginning of the eleventh century." Now, there was an Abu Ghalib at that time, who was surnamed Fakhr al mulk, "glory of the realm." After this high personage, the mathematician Abu-Bekr-Muhammed-ibn-alhusain, called Al-Karkhy, named a book 'Al-Fakhry,' where the final y is equivalent to our termination -*ian*. It amounted to a dedication. As for Cajori's "*des*," we can see in it nothing but the German genitive article.

But these are trifling faults. What we have a right to expect in such a handbook is an agreeable narrative of the most material events in the history of mathematics, and this Prof. Cajori incontestably supplies. The book was much wanted.

58 (3 May 1894) 335: Total Eclipses of the Sun.

[Columbia Knowledge Series.] By Mabel Loomis Todd. Boston: Roberts

Bros. 1894.

CSP, identification: MS 1512 (draft). See also: Fisch, *Second Supplement*.

Mabel Loomis Todd (1856-1932) was the wife of the American astronomer David Peck Todd, whom she accompanied on two expeditions to Japan to observe solar eclipses. Her book *Total Eclipses of the Sun*, reviewed here by Peirce, was written from data collected by her husband, and it served as a standard work for many years. Among other achievements is her edition of the poems and letters of Emily Dickinson.

Joy is hardly too strong a word to express the surprised state of mind of the reviewer who finds a work upon a subject sharply cut treated thoroughly upon a plan sharply cut. True, an easy subject has been selected. Professor Ranyard some years ago digested in a thick quarto all that had up to that time been observed in relation to solar eclipses, and those astronomers whose attention has been turned that way have kept subsequent observations under continued comparison with those recorded by Ranyard. That, therefore, facilitated Mrs. Todd's task. Yet it was not easy to boil Ranyard's huge volume (together with all that has been observed since) into a couple of hundred pages of duodecimo. This has been effected with such skill that the most learned astronomer may profit by reading these pages, though any novice can profit by them.

Of all the phenomena of nature, a total solar eclipse is incomparably the most sublime. The greatest ocean storm is as nothing to it; and as for an annular eclipse, however close it may come to totality, it approaches a complete eclipse not half so near as a hurdy-gurdy a cathedral organ. Few people who do not make journeys on purpose ever see a total eclipse; consequently, it is difficult to imagine how such events could ever have been predicted before the theory of them was understood. The Babylonian bricks have shown that the most scientific people of antiquity often made mistakes in their predictions that solar eclipses would occur; and they never seem to have attempted to predict their totality. It is true that in the period of fifty-four years eclipses come round with some approximation to their former conditions. It may possibly have been that Thales, who is said to have predicted an eclipse 585 B. C., made use of this period; but several considerations cast great doubt on the prediction. It is a curious fact that the records of ancient eclipses, solar and lunar, do not gibe with modern observations. To account for the discrepancy, Oppolzer and other European astronomers suppose a gradual change of the motion of the moon; but our American authority, Newcomb, finds evidence of a sudden change such as might have resulted from some cataclasm within the interior of the earth, which should have altered the length of the day by a minute amount that might become appreciable in the course of many centuries.

Mrs. Todd gives full information about the observations of the sun's corona and about the various theories of it, all of them unsatisfactory. She seems to regard it as certain that the light of it is largely ultra-violet. If this be the case, fluorescent spectacles ought to assist the observation of it. Mrs. Todd makes the curious if not very probable suggestion that the wings of the winged sun-disk which the Egyptians imported along with the god Ra represent the sun's corona. They are certainly similar to some appearances of the corona. At the end of the book is a valuable chart of the eclipse-tracks of the next fifty years. It would have been better if the quincuncial projection had been employed for this purpose, as recommended by Oppolzer. But the book is so pretty and so interesting that we need say no more of it; our readers will find it out for themselves.

58 (31 May 1894) 415-416: **New Light from the Great Pyramid**.

By Albert Ross Parsons. New York: Metaphysical Publishing Co. 1893.

CSP, identification: MS 1394 (draft). See also: Burks, *Bibliography*; MSS L 159.55-56.

The author's long, rambling preface conveying no clear promise of what this book is intended to contain, we turn to the first chapter for light. This opens with four pages of insignificant quotations from unimportant authors--unimportant, at least, in regard to the pyramid--followed by another page of texts from the Bible, such as the parable of the two houses and other passages equally far-fetched. Next comes a section headed "The Pyramid

Explained by the Fall of Lucifer." Glad of something like a definite position, we ask how we are to be convinced that this is true. The author first cites Isaiah xix., 19, 20, which, after referring to Heliopolis, prophesies that an altar to Jehovah shall be set up in Egypt. Upon this the author argues as follows: The Hebrew word for altar (*mizebeha*) comes from the root of the Sanskrit *mri*, to die, and is identical with the second syllable of the word *pyramid*. The first syllable of this word, he avers, is the "Egyptian *pur*, fire." Hence, "a pyramid is an altar signifying death by fire." Now, Mr. Parsons informs us that one Wilson, in 1856, demonstrated that the pyramid interprets an ancient theory of gravitation applied to a body falling from a planetary distance; wherefore he asks us to regard it as proved that the pyramid bears witness to the fall of Lucifer.

This is not convincing. The passage of Isaiah appears to have nothing to do with the pyramid. No evidence is adduced to show that the Hebrew word for altar has any connection with the Sanskrit verb to die; and all we know of the two languages renders this extremely improbable. No evidence is adduced to show that the middle syllable of "pyramid" has anything to do with either the Hebrew or the Sanskrit word. No evidence is adduced to show that the first syllable of "pyramid" means fire. It is true that it might mean fire in Greek, but of the etymology of the word we are quite ignorant.

In the next place, Mr. Parsons must not presume that the world will take his word for it that the writer Wilson proved in 1856 that the Great Pyramid interprets an ancient theory of gravitation. On the contrary, nigh thirty years having elapsed without any man of any character for learning having accepted Mr. Wilson's theory, the burden of proof is upon Mr. Parsons if he wishes to convince any judicious part of the public that that theory is true. *Prima facie*, it seems quite absurd. How could there by anything like a mathematical theory of gravitation in ancient times without its affecting ancient thought very powerfully? And what sort of reasoning can prove two propositions so strange as, first, that there was such a theory in such primitive times, and, second, that the great pyramid was built with reference to that theory?

But even if, for the sake of argument, we grant, first, that the word "pyramid" means altar of death by fire (in which case, it is not an Egyptian word at all), and also that the Great Pyramid was built to illustrate a theory of gravitation, there still yawns a chasm vast and deep between these premises and the conclusion that the Great Pyramid bears witness to the fall of Lucifer. The world will think that bad reasoning; and there is no use in bringing before the world an argument that nobody who wishes to preserve the reputation of a sane head will admit. The book contains about 400 pages, and seems to pile such arguments one upon the top of another to make such a Babeltower as would not be secure even were every stage of it laid with the utmost caution and solidity.

59 (5 July 1894) 17: Johnson's Universal Cyclopaedia: A New and Enlarged Edition.

Charles Kendall Adams, Editor-in-chief; Assisted by a corps of thirty-six Editors, composed of the ablest and most distinguished scholars in the United States, etc. Vols. I.--IV. D. Appleton & Co. 1894.

CSP, identification: MS L 159.71. See also: Fisch, *Second Supplement*.

Charles Kendall Adams (1835-1902) was an educator and historian. From 1885 until 1892, he served as the second president of Cornell University. Later, he became president of the University of Wisconsin. Adams received honorary degrees of LL.D. from the University of Chicago in 1878 and from Harvard in 1886.

Johnson's Cyclopædia was first published in 1874, and was the first small cyclopædia which adopted the plan of many departments with a very eminent specialist at the head of each. There are advantages and disadvantages in such a plan. The great end which it is possible to bring about in that way is a broad and comprehensive view of human knowledge, by men who are able to inform us what is the present state of ideas in their several departments. The great danger lies in a want of unity, proportion, and consistency. Moreover, a large part of the substance of such a work, *e. g.*, the biographies, is not amenable to effective control by specialists. Hence the scientific portions of the original Johnson were much the best, and were peculiarly within the province of the editor, F. A. P. Barnard. In the new edition the Cyclopædia preserves a good deal of its spirit of life, and has been brought up to the times. We must say, however, that the last volume which has appeared, the fourth, while it is an improvement in some respects on those that went before it, is less like the original edition. It is a great mistake to suppose--if anybody does suppose so--that no great literary art is called for in a short cyclopædic article. There is no art in which success requires more peculiar original powers nor a more severe or longer training. It is safe to say there are not thirty real masters of it in this country to-day. But there are 300 contributors to the fourth volume of this Cyclopædia. Some of the least meritorious articles it contains, dreary and antiquated, bear very eminent and deservedly famous names.

Comparing the different departments of the work, if we must select one part as distinctly inferior to any other, it will be logic, which, though written by different hands, is pretty uniformly out of date and feeble. But incomparably the most interesting articles are those relating to linguistics. Those which carry the signature of Prof. Wheeler are all excellent, and there are others which really convey something worth remembering. The legal articles are very clear. The articles on physics are well enough; but there is nothing one will not find everywhere. The mathematics (excepting the debris of former editions) is not nearly so well done as the physics. The biological papers are of various degrees of merit. Some are very learned, others very instructive. The plan of distributing the biography among the thirty-six editors does not work well, especially in the case of living persons. The illustrations, which are confined to wood-cuts in the text, are of a wide range of merit. The maps will not bear comparison with those of Brockhaus.

This new edition, being in eight volumes and costing $48, can no longer be reckoned among the small encyclopædias. It has about as much matter as the new edition of 'Brockhaus's Konversations-Lexikon,' in sixteen volumes, and the total price is the same. There is still room for a methodical encyclopædia, not too large to be read through by an industrious person, say, in a year, and readable, withal; a work which should aim to convey information of large truths rather than of minute facts, and which should be written with something like the Peter Parley art, so as to be read with pleasure by old and young--the whole to be the product of not more than half-a-dozen masterly pens.

59 (12 July 1894) 34-35: **Basal Concepts in Philosophy**.

By Alexander T. Ormond, Professor in Princeton. Scribners. 1894.

Attributed to Peirce by Fisch in *First Supplement* (internal evidence). Ritchie's book, *Darwin and Hegel*, is mentioned; Peirce was definitely the author of that review, which appeared at 57 (23 November 1893) 393-394. This piece is unassigned in Haskell's *Index to* The Nation, vol. 1.

Alexander Thomas Ormond (1847-1915) was educated at Princeton, and, in 1883, he accepted a post there as professor of mental science and logic. In 1898, he became McCosh professor of philosophy, which position he resigned in 1913 to become president of Grove City College.

The purpose of this work is to propose a certain modification of the Hegelian system, suggested by studies in philosophical theology. The modification cuts pretty deep, being no less than the identification of Being and Spirit, two of the primary categories of the Hegelian logic. It is to be accomplished by some alteration of the Hegelian dialectic which is not by any means rendered clear, far less justified. In fact, clearness and conclusiveness are the two qualities this book can boast the least. An ordinary person, should he be condemned to read it, might fancy he caught a glimpse of meaning here and there; but one who has devoted a year or two to the study of philosophy, upon a modern method, will find it utterly unprofitable. An accomplished thinker may make some little use of the work, notwithstanding its grievous faults of logic.

Attempts to reform Hegelianism ought to be encouraged and treated benevolently. All things are to be known by their fruits; and certainly never idea had a fairer, fuller trial than the Hegelian system. All Germany went into it for a generation, and the fruit was disappointment and disgust. How could trial be fairer? And if any question is ever to be regarded as pretty nearly set at rest--set at rest until some very unexpected new evidence presents itself--must not this question be so regarded among the first?

At the same time, very few minds that have examined the matter with attention look upon Hegelianism with exactly the same eyes as upon astrology, or even as the majority of the very small number of persons who have conscientiously examined the evidence about ghosts look upon that belief. On the contrary, although historic experience condemns Hegelianism as a method and a ruling tendency, it does go to show that where Hegelianism, while having a certain influence, has not been strong enough to turn men from common-sense humility in regard to external nature, its influence has many a time been beneficial. So it is probably safe to say that the indefinite congeries of minds that partake of the real spirit of investigating science (not mere readers of scientific results), but at the same time are well versed in the world's philosophical thought, are inclined to think that Hegelianism, however wrong on the whole, yet contains something not without value if it could be singled out. It is easy enough to point at elements of the position of Hegelians and other metaphysicians that are quite irreconcilable with the position of scientific men. All scientific men (except some pure mathematicians) have a very unaffected reverence for nature--so genuine, indeed, that it is often subconscious--which causes them to surrender their dearest beliefs at nature's hest. But the idealists, as a body, as well as some other metaphysicians, look upon things external with a sort of contemptuous disdain, and regard nature as rather a bore. This self-poise deprives them of the only means whereby they could proceed from falsehood to truth. As long as philosophers are bred in seminaries where any species of infallibilism is taught, or imitate others so bred, such will be the main result. Theirs will be systems founded on egotism, embodying whims of more or less artistic symmetry, but destitute of the power of growth. On the other hand, it is not difficult to indicate general features of Hegelianism in respect to which it harmonizes better with the general principles of science than do most of the other philosophical systems. Here there seem to be ideas of real power. Here is something which in time must grow and choke the parasitical errors of the system. The difficulty is to give the thing the first start. Whether that can be done by any attempt to improve the agreement of Hegelianism with theology (such is the aim of this volume), may very well be doubted. We should be more inclined to look for the first buds of regenerate Hegelian life on such a tree as that of Mr. Ritchie, the author of 'Darwin and Hegel.'

We shall not advise many of our readers to try to solace their leisure by reading Prof. Ormond's not too perspicacious, nor too vigorous, nor too agreeably written volume. Two things are specially urged in it which may make some persons curious to look over the work--one the idea that personality is essential to being, the other than Nothingness has a sort of reality. We simply mention these, without saying that we think they are skilfully defended.

59 (19 July 1894) 52-53: **The Animal as a Machine and a Prime Motor, and the Laws of Energetics**.

By R. H. Thurston. John Wiley & Sons. 1894.

CSP, identification: MS L 159.66. See also: Fisch, *First Supplement*.

Robert Henry Thurston (1839-1903) was a mechanical engineer by trade, and an 1859 graduate of Brown University. During the Civil War, he served with the Federal navy, where he gathered information on steam boilers and practical engineering for his later publications. Thurston was a frequent contributor to *Popular Science Monthly* and *Science*.

In this little book Prof. Thurston pursues that way of attacking the Second Law of Thermodynamics (or, as he prefers to call it, the Law of Carnot) which is his own. That law is that heat flows from hot bodies to cold, as water runs down hill; so that when bodies are all cooled down to one level of temperature, the heat in them above the absolute zero is no more available to run an engine than is the height of the sea above the centre of the earth available to turn a water-wheel. At one time great ingenuity was expended to discover some exception to the Second Law of Thermodynamics; but the only exception which withstood examination was the hypothesis of Maxwell. Maxwell supposed a diaphragm in a vessel to separate two portions of air; and that in this diaphragm was a little sliding door at which should sit a tiny doorkeeper who should look out for the very fast-moving molecules coming one way and for the very slow-moving ones coming the other way, and open the door for these and for no others. In that way the air on one side would become heated, and that on the other side cooled. It was the first time an advantageous route for science had been found through fairy-land; and when it was further remarked that, by replacing the little door by a lobby with two doors and putting a fan-wheel in the lobby, an engine could be run directly, the analogy to the running of a water-wheel by the gravitational energy of the ocean became striking. Prof. Thurston considers our inability to use the great heat above the absolute zero of ordinary objects to be a shocking waste; and one of the chief purposes of this little book is to adduce evidence that living animals are machines in which the Second Law of Thermodynamics is "evaded." Inasmuch as he says the Law of Carnot "asserts the necessity of waste," and further says this waste does not take place in the living machinery, perhaps "violated" would have been a clearer word than "evaded."

Prof. Thurston certainly succeeds in showing that, accepting extant experiments upon animals (and they are both elaborate and numerous) for what they may be worth, they distinctly point towards some violation of accepted laws of energy. Thus the careful experiments of Hirn showed that more heat by a third part was generated by the human body than the combustion of food would account for. Moreover, when Hirn compared the amount of work a man performed with the reduction of the heat generated while he was at work below what was generated at the same time while he was at rest, he obtained a result which Prof. Thurston argues is contrary to accepted laws of thermodynamics. "The animal system," he says, speaking of it as a motor, "conceals some secrets that science has still to discover." Dr. Pavy's well-known experiment on two pedestrians is also cited to show that "the body as a heat engine is capable, apparently, of performing more work than the food would seem competent to do." Dr. Austin Flint failed to explain from known physical principles the results of his experiments upon the pedestrian Weston in a walk of 310 miles.

Prof. Thurston's conclusion is, not that there is any inaccuracy in the law of the conservation of energy, which rests with him upon metaphysical grounds, but that the Law of Carnot is somehow "evaded." If the mechanical conception of the universe, that all that exists is expressible in terms of mass, space and time, upon which Helmholtz rested his celebrated enunciation, be accepted, it is known that the "Law of Carnot" follows as a corollary. By the majority of physicists the alternative will be felt to be either, on the one hand, to suppose that all the observations that have been made upon animals are subject to a common error, due to the same cause, whatever that may be; or, accepting the experimental evidence, to conclude that the law of the conservation of energy is not exactly fulfulled [[sic]] in living animal bodies. The Law of Carnot was enunciated long before the law of the conservation of energy, and, if it be regarded as the assertion that the average motions of the different parts of a system tend to equalize themselves or to approach final ratios, is incontestably quite as certain as the law of energy.

Prof. Langley has recently taught us how one of the great wonders of the animal world--that of the soaring bird--is performed. Now, if we are to take his successful explanation as a model by which to explain other animal marvels, it must be confessed that the way the bird turns this way or that way to take advantage of the lulls and puffs of the wind, is not unlike the opening and shutting of the doors of Maxwell's devils, thus affording some comfort to Prof. Thurston. The book has, at any rate, the merit of calling attention to one of those residual unexplained phenomena in the patient study of which, not in blind denial of them, the progress of science consists.

59 (23 August 1894) 144-145: ALCHEMY AND CHEMISTRY

The Alchemical Essence and the Chemical Element: An Episode in the Quest of the Unchanging.

By M. M. Pattison Muir. Longmans. 1894.

Law and Theory in Chemistry: A Companion Book for Students.

By Douglas Carnegie, sometime Scholar and Demonstrator in Chemistry of Gonville and Caius College, Cambridge. Longmans. 1894.

CSP, identification: MS L 159.68. See also: Fisch, *First Supplement*.

Two very eminent chemists have devoted a considerable part of their lives to the elucidation of alchemy, Kopp and Berthelot. Their genius and skill as chemists do not at all come into play in this undertaking, although, of course, some proficiency in chemistry is requisite even to read alchemical treatises, not to speak of appreciating them. Kopp's is a solid compendium of two octavo volumes, mostly of fine print, which gives the titles of an enormous number of writings, and a good deal of general information, but really little about the inside of

alchemical treatises. It dwells particularly on the alchemy that just preceded modern chemistry. It was published in 1886. Berthelot's labors, mostly more recent, are of a totally different and higher kind, consisting in the transcription, translation, and elucidation of a number of Greek papyri about as old as the Christian era, together with a few mediæval manuscripts which throw some light on the papyri. He has thus completely opened up to us the Egyptian alchemy, while leaving that of western Europe untouched. We have, besides, a work which is one of the greatest psychological curiosities of literature. Its date is 1869. It is considerably longer than Green's 'Short History of the English People.' Its peculiarity is that its author, Dr. Gottlieb Latz, professes himself to be a believer in the doctrine. There would be nothing surprising about that were it not that the tone is decidedly critical and even sceptical. He drops such expressions as, "Alchemy is not an inquiry but a speculation." What he calls "the goldmaking swindle," and by other contemptuous designations, is suggested as the explanation of every boast of the adept. He sees in alchemy a general mystical doctrine with applications to man, to plants, to metals. It has had, he tells us, various forms, but all are interpretations of the Smaragdine Table. This is certainly in part true, and Latz makes the mediæval alchemists more intelligible to us than anybody else has done, even if we do not believe the Smaragdine Table has played the part he attributes to it. But it is astounding to find a medical man, able to throw light on a difficult problem, who yet attributes wonderful powers to seven "arcana," which are, (1) sulphuric acid, (2) iron, (3) carbonate of soda, (4) Chili saltpetre, (5) solution of sulphide of ammonium, (6) *Pulver solaris ruber, i. e.*, a mixture of red oxide of mercury with pentasulphide of antimony, (7) *Pulver solaris niger, i. e.*, a mixture of red oxide of mercury with tersulphide of antimony. We mention the work because it is not very well known.

But all three books hardly afford us a living mental portraiture of the madiæval [[sic]] alchemist. Of the externals of the man we have several contemporary pictures; of modern psychological studies perhaps those of Dumas and Balzac are not quite worthless; but as to the state of the mediæval alchemist's intellect we have had no popular delineation before this sketch by Mr. Pattison Muir, and this is very true to nature. But more we cannot say for his work.

Mr. Carnegie's is a little volume of two hundred and odd pages of small octavo about the history and philosophy of chemistry, from the alchemists to this day. It is not intended for beginners, but to "recapitulate and coordinate the more important principles of the science, before proceeding to more detailed and advanced works." Certainly so small a volume must be inadequate to the needs of adult and serious students; but there are a good many practical chemists who are too negligent of the marvellous progress that chemical dynamics has made of late years, and of its growing practical importance, and this volume will serve to show them that they must lose no time in studying Ostwald's 'Allgemeines Lehrbuch,' from which a large part of Mr. Carnegie's volume has been abridged, and which, by the way, ought to be translated entire. Moreover, there are others, bright students, even boys, and a few general readers, to whom, if they know how to read a book they cannot entirely comprehend, this volume may lift a veil, and

display, if only confusedly to those not well acquainted with chemistry, what a vast field is being worked and with what success.

Alchemy is treated by Mr. Carnegie in the slight, we had almost said the facetious, vein which is inevitable when history is made subservient to doctrine. Indeed, the whole first chapter, which carries us to phlogiston, is of small accuracy. Thus, having finished with the alchemists, the first sentence following is: "With the Benedictine monk, Basil Valentine, begins the period of iatrochemistry, . . . during which transmutation was in abeyance." It is true that the author of the 'Triumph-Wagen' calls himself "Basilius Valentinus, ein Bruder des geschwornen Ordens S. Benedicti." But this cannot be true, because the Emperor Maximilian instituted inquiries in 1515 to find out who Basil Valentine was, and completely failed. Now the general catalogue of the order, kept at Rome, was open to him, and therefore it is impossible there ever should have been a Benedictine of that name. The author of the books was in all probability living in 1515. Dr. Latz will not admit that any of the other writings which pass under the name of Basil Valentine are by the author of the 'Triumph-Wagen,' because they do not accord with the cryptic interpretation which he traces out for this; but to ordinary men they seem wonderfully like that in style, and certainly in strong contrast with earlier alchemical works, so that they must at any rate be classed with the works of the "spagiros," as they call themselves.[†*] Now these writings have much to do with transmutation. Mr. Carnegie's next sentence states that "heretofore the apothecaries had prescribed purely vegetable preparations only, but now we find mineral specifics contesting the field with them and partially replacing them." This is a mistake. To give no other refutation of it, Avicenna had been translated by Gherardus Cremonensis in the twelfth century, and that translation still in use, with all the corrections noted, gives as medicaments antimonium, ammoniacum, alaunoc et alahabar (*i.e.*, graphite, or possibly MoS(2)), asius, alæ, argentum vivum, atrimentum, arsenicum, azul, ambra, aranea, argentum, alumen, adeps, acetum, aurum, etc.

Van Helmont is passed by with bare mention in a footnote, albeit he did much toward causing gases to be conceived as corporeal substances, and invoked the balance to decide a chemical question, a stupendous idea had he only not unfortunately overlooked the possibility that substance concreted from gases might weigh something. Coming down to Robert Boyle, Mr. Carnegie speaks of his style as "language which knows nothing of the mystic and rococo style of alchemistical literature." Now it is true that alchemists are all mystics, and that the iatrochemists are often bombastic; but to say that the style of the mediæval alchemists is rococo, or florid, is exactly like making the same accusation against the Cook's Own Book. Their writings are as dry as possible. Everybody knows what Boyle's style is, because Swift's 'Meditation on a Broomstick' imitates it to perfection, except in one particular, his quite inimitable longwindedness.

The second chapter is devoted to the Phlogistic Period. In the third, the historical arrangement is abandoned, and the treatment of mixtures is considered in a manner which fails to leave a very definite impression on the reader's mind. In the fourth, on the Atomic Theory, there is a return to the first method, and a most interesting, though not quite full, narrative is given of the various waves of opinion which swept over the chemical world from Dalton to Williamson. Mr. Carnegie says it seems as if Berzelius were *lucky* about his atomic weights. *Luck* is a self-contradictory idea; but there is perhaps a sagacity whose reasoning is too subtle and nice for its possessor ever to give a satisfactory account of it. Dalton and others seem to have been possessed by such inspiration-like insight when they accepted the Atomic Theory. It is now eighty-six years since men to be listened to have been very clearly and very pertinently asking those who hold that the facts of chemistry support the atomic theory, why it is that 1,234,567 atoms of hydrogen might not form a saturated compound with 617,283 atoms of oxygen, unless by virtue of a special law which would give rise to simple compounds though there were no atoms? And this question, incessantly asked, has never been answered. By this time facts non-chemical have caused almost all physicists to believe in atoms. But it would aid the chemical philosophy, and perhaps throw much light on it, if the secret reasons which led chemists to accept the theory before the physical proofs were known could be ascertained. This chapter ends with a discussion of molecules which is hardly in continuity with the first part, but is worthy of attention by itself.

The remainder of the volume sketches rather too slightly the most modern theories by which chemists are laying siege to their great problem. That problem is this: The periodic law makes all the properties of the elements to be dependent upon their atomic weights alone. It is required, then, to produce an hypothesis from which may be deduced from the atomic weights all the physical and chemical properties with mathematical strictness. It is also requisite that this hypothesis should explain why the atomic weights are precisely what they are--that of oxygen, for instance, 15.94 instead of 16, etc. A few years ago the idea of setting up such an aim, even as a remote one, would have seemed chimerical, and it may seem so to-day to many chemists; but in a few years more it will be in full sight of all as the Alp which our army has to mount.

One question which it is very desirable to settle as early as possible is, whether the forces existing between atoms are polar or not. At first blush, it seems as if they must be so, because of the fact of saturation. But Van't Hoff suggests that the surfaces of atoms are level surfaces, with points of maximum attraction. In that case, saturation consists in having all the places where atoms would be strongly attracted occupied. This theory seems to involve the conception that an atom has a surface and a size. Yet the Boscovitchian idea may be true just the same, for that theory virtually gives to the atom a surface, namely, the surface of the space within which the central force is repulsive, while outside of that it is attractive. Prof. Mayer made some years ago a very beautiful experiment by which little objects floating on water were made to repel one another if very near together, but attract (or rather, all were attracted towards a common centre, which is the same in effect) when at greater distances. The result was that they arranged themselves in regular clusters strongly suggestive of chemical molecules. If we

add the hypothesis of their being in constant motion without resistance, we seem to have all the elements requisite for a foundation for chemical dynamics without polarity in the forces. Prout's law would also be accounted for in this way; for if two Boscovitchian atoms have attractions which increase inversely as a high power of the distance with no repulsion for smaller distances, one may easily encounter another in such a way as to move in narrowing spirals around it for ever; and nothing would be likely ever to separate them. The inexactitude of Prout's law is a fact calling for a distinct hypothesis.

The manner in which Mr. Carnegie has set forth the tridimensional graphs of Wislicenus, Le Bel, Van't Hoff, and Guye, and Horstmann's application of thermodynamics to chemical processes, is extremely clear, and calculated to set the student to work to learn more of these matters. The author is thoroughly versed in all the modern ideas, thinks with rare clearness, and writes with peculiar simplicity, grace, and charm.

59 (13 September 1894) 191-193: HELMHOLTZ

CSP, identification: Haskell, *Index to* The Nation. See also: Burks, *Bibliography*; *List of Articles*; MSS L 159.41-42, L 159.47-48, L 159.50; MS 1395 (draft). This obituary was reprinted in Gustav Pollak, ed., *Fifty Years of American Idealism: The New York* Nation *1845-1915* (Boston: Houghton Mifflin Co., 1915), 348-355, and is there attributed to "C. S. Peirce."

Dr. Hermann Helmholtz, as his contemporaries have called him, the acknowledged and worshipped head of the scientific guild, is gone. He was born on August 31, 1821, at Potsdam, where his father was professor of the gymnasium. His mother's maiden name was Caroline Penn; she came of a branch of that family settled in Germany since the religious troubles in England. From childhood Hermann had a passion for science; but the nineteenth century came near missing this great light, for the circumstances of the family were such that no road to science was open to him except that of studying medicine in the Military Institute of Berlin. He took his degree of M.D. in 1842, and his inaugural dissertation, the only Latin publication of his life, related to the nervous systems of invertebrate animals. He was at once attached to the service of charity, and began without delay to study putrefaction, upon which in 1843 he published a memoir maintaining its purely chemical nature--an opinion subsequently surrendered. He soon returned to Potsdam a surgeon in the army. In 1845 he was employed with good reason to write articles on animal heat in a medical encyclopædia of high character, and in the yearly report upon the progress of physics. The same year he printed an original investigation of the waste of substance of a muscle in action.

After that, for about two years, he produced nothing. It was one of those periods of seeming idleness to which the most productive geniuses are subject, and which afford mediocrity matter for carping. Other young scientists filled the journals of 1846 with the records of their industry, but not one syllable came from Helmholtz. He was not heard from until 1847, and not till July 23, when he read a paper before the Physical Society of Berlin. This paper was entitled "The Conservation of

Force." In the judgment of many of those who have examined the matter, it was the epoch-making work from which alone the greatest scientific discovery that man has ever made must date. Certainly it was the argument which produced the intense conviction with which the world has held that doctrine ever since. It is fair to say that other excellent critics, and Helmholtz himself among them, award the merit of the first enunciation of the great law to Robert Mayer, who, in 1842, had published a paper which attracted no attention whatever, and of which Helmholtz in 1847 was as little aware as the rest of the world. But, in any case, there is no doubt that

Helmholtz was the first to conceive the proposition from the point of view which made it so attractive to all accurate thinkers and so wonderfully fecund in new truth.

According to his statement, nothing exists in the outer world but matter. Matter *in itself* (*an sich*) is capable of no alteration but motion in space, and these motions are modified only by fixed attractions and repulsions, and this is true everywhere, even in the actions of animals and men. It was an amazingly bold assertion, utterly opposed to almost every kind of philosophy, certainly to Kantian and all post Kantian idealism, as well as to the nominalistic idealism of the English school, which such writers as Ernst Mach have taken up. But the implicit faith with which it has been received is a singlular [[sic] psychological phenomenon, for the theory that all human actions are subjected to a law having no teleological character, when we know (or seem to know) that our actions are adjusted to purposes, has obvious difficulties; and the experimental evidence of the correctness of the law as applied to animal physiology is very slender. Indeed, some of the most careful researches (as those of Fick and Wislicenus) have led to results directly opposed to it. Yet the physiologists, one and all--the judicious Michael Foster, for example--simply treat those results as absurd. In this aspect Helmholtz's great doctrine appears as the pet *petitio principii* of our time. Its truth was unquestionable, in the only sense in which anything based on induction can rationally be admitted as true, namely, its *close approximation* to exactitude. Nobody can deny that it is at once the crown and the key of physical science. In that memoir, by the way, Helmholtz first displayed his facility in applying the calculus to unaccustomed problems--a facility very surprising in a man of twenty-six whose studies had been supposed to lie in the direction of anatomy and physiology. Surely, in the company at that memorable meeting of the Physical Society there must have been some who were able to discern that they were in the presence of one of the most stupendous intellects that the human race had yet produced.

Of course, a reward was due from organized humanity to the man who had thus lifted man's mind to a higher vantage ground. And this reward came, for the next year he was created no less than assistant in the Anatomical Museum of Berlin. He now began to occupy himself with the physiology of hearing. In 1849 he was appointed supplementary (or extraordinary) professor of physiology in the University of Königsberg (without salary), and in 1850, on July 19, he communicated to the Physical Society of Berlin an elaborate memoir breaking ground in the interesting field of the measurement of the duration of nerve-actions. In 1851 he invented the ophthalmoscope, for which many and many a human being has owed him his eyesight. This year he began an original study of electrodynamics. In 1852 he was

promoted to a regular chair in the university. His discourse upon his installation dealt with peripheral sensations in general, especially those of sight and hearing. It was a comparison of the relation existing between the vibrations that excite a given sense, and those existing between the sensations themselves. We remark that while the memoir on the Conservation of Force fairly bristled with repetitions of the philosophical phrase *an sich*, "in itself," it is in this discourse carefully avoided. It would seem that something must have happened in the interval which made Helmholtz dread "*an sich*" as a burnt child does fire. In this paper, such ingenuity is used to avoid it that but once does it slip in, and then in a negative phrase. But since the idea was there, we cannot praise Helmholtz for not giving it its proper dress.

In giving the substance of his lecture, we need not imitate his circumlocutions to avoid this natual [[sic]] phrase. His point was this: vibration-systems essentially different give rise to precisely the same color-sensations. There are three fundamental color-sensations, which, being mingled in different amounts, give rise to all others; but there is nothing corresponding to this tri-dimensionality in the vibrations themselves. On the contrary, the sensations of a color-blind person for whom one of the three fundamental sensations is non-existent, much better correspond with the facts in themselves. Sounds, on the whole, correspond more accurately to the vibrations. But, to the ear, the difference between one rate of vibration and another is hardly perceptible until two different sounds are compared. If a melody is transposed to another key, the effect is nearly the same; but a painter who should transpose red to yellow, yellow to green, green to blue, and blue to violet, would make a nightmare of his painting. These are certainly striking facts; but still more interesting is it to note what lesson it was that his typical nineteenth-century understanding drew from them. Other minds as clear as his might have read here the incommensurability between mind and matter, and have found a refutation of materialism in the circumstance that mind here acts as matter could not do. But the conclusion of Helmholtz is that the sense-qualities distinguish the things in themselves about as well and about as arbitrarily as the names Henry, Charles, and John parcel out human kind.

Besides this "Habilitationsvortrag," a "Habilitationsschrift" was expected from the new professor, and this last set forth his theory of the mixture of colors. It was, at bottom, the doctrine of Dr. Thomas Young; and only the careful comparison with observation, and the application of it to explain effects of mixing pigments and the like, were new. In 1854 he attended the meeting of the British Association at Hull, and there read a fuller account of his theory of colors, which no doubt induced Maxwell to take up this study, who soon made it even more lucid and beautiful than Helmholtz had done. In 1855 he became professor of physiology at Bonn. In 1856 he began the publication of his great treatise on physiological optics, which was not completed till ten years later. On May 22 of the same year, he announced to the Berlin Academy his discovery of combinational tones, which are musical sounds resulting from the interferences of the vibrations making two other sounds.

In 1858 he became professor in Heidelberg, at that time the ultimate goal of a German professor's ambition; and in the same year he astonished the mathematical world by his great memoir on eddies, or vortices, a matter of fundamental importance in hydrodynamics. It was a very great and fruitful idea which he there advanced, and which he wonderfully developed. Much has already come from it, but its full harvest yet remains to be gathered in. No mathematician will dispute that this was a work only second in importance to the cataclysmic essay on the Conservation of Force. During the next two years Helmholtz's acoustical researches were very prolific, and at the same time he published remarkable papers upon colorblindness and upon the contrasts of colors. In 1860, on April 12, he read to the Vienna Academy a paper giving measurements by his pupil, Von Pietrowski, of the viscosity of fluids, with a mathematical discussion by himself. Although the subject was not quite new, Stokes's masterly work dating from 1851, still Maxwell's researches were not yet begun, and this memoir constituted another important contribution to hydrodynamics and to the general conception of matter. Helmholtz himself very soon began to apply these ideas in acoustics.

We next find him engaged upon the difficult problem of the horopter and the motions of the eye. One of the next subjects to engage his attention was the musical note which is emitted from a strongly contracted muscle. In 1862 appeared his great work on Sensations of Sound and the theory of music, and with it the main work of his life was accomplished. Since that time he has indeed produced enough to make another man famous; it is little only in comparison with his earlier achievements. He has written, for example, papers upon the facts underlying geometry which were substantially anticipated by Riemann's great work, with which Helmholtz would seem not to have been acquainted. To produce independently that which was the proudest laurel of one of the most original mathematicians of the ages was a great feat, but it was needless. There were also a series of memoirs in which Helmholtz discusses all the principal systems of formulæ which have been proposed by different physicists as laws of electrodynamics. He gave the first mathematical explanation of the formation of ordinary waves upon water--an explanation which not only enables us to see why certain forms of waves which might exist are not produced in nature, but also throws much light on other subjects. In 1871, he was appointed professor of physics, no longer of physiology, in the University of Berlin. Twenty years later he was made president and director of the Physikalisch-Technische Reichsanstalt, a foundation under the control of the Imperial Department of the Interior, for the experimental furthering of exact natural inquiry and the technics of precision.

Not the slightest allusion to any moral or religious problem ever dropped from the pen of Helmholtz. Though no reference to Hegel or Hegelianism appears in his pages, he more than any other namable person caused the downfall of that kind of speculation in Germany, and brought in the present admiration for the English style of philosophizing which his own so much resembled. The temper of the man was admirable. He never indulged in one of those reclamations of priority into which scientific vanity is sure to be betrayed, but several times published notes to show that his own results were not so new as he and the

scientific world had believed them to be. He did much to bring into notice the works of other physicists, among them the Americans Rowland and Rood (his visit last year to this country is freshly remembered). He found himself several times engaged in controversies with redoubtable antagonists, Clausius, Bertrand, perhaps we may so reckon Land. In every case he so conducted himself as to bespeak an imperious desire to find out the truth and to publish it; and every approach to personality was avoided or flung away from him as a pestilential infection. The world owes much to the intellectual clearness and integrity of Hermann Helmholtz, M.D.

59 (27 September 1894) 237-238: FOUR HISTORIES OF PHILOSOPHY.--I.

A History of Philosophy. With especial reference to the Formation and Development of its Problems and Conceptions.

By Dr. W. Windelband. Authorized translation by James H. Tufts, Assistant Professor in Chicago University. Macmillan & Co.

History of Modern Philosophy.

By Richard Falkenberg. Translated with the author's sanction by A. C. Armstrong, jr [[sic]]., Professor in Wesleyan University. Henry Holt & Co.

An Historical Interpretation of Philosophy.

By John Bascom. G. P. Putnam's Sons.

A History of Modern Philosophy.

By B. C. Burt. 2 vols. Chicago: A. C. McClurg & Co.

CSP, identification: Haskell, *Index to* The Nation. See also: Burks, *Bibliography*; *List of Articles*; MS 1396 (draft).

If history is to be conceived neither as mere narration nor as the study of obsolete politics, but as an account of man's development, then the history of the mind is surely the main thing, and that of thought must stand at its head. It is odd how different to different sciences is the importance of their own chronicles. Mathematics can boast of a long and interesting past; but it is neither needful nor usual for mathematicians to know much about it, except only in certain special branches--the theory of elasticity, for example--which are regularly studied in the discussions that gave them birth. An examination of the beginnings of mechanics throws, as is well known, no feeble light upon the science itself; while physicists hardly concern themselves about early optics or early meteorology. Certainly of no natural science can it be said, as might very well be maintained of metaphysical philosophy, that its history is of more consequence than its doctrine. The University of Paris, and every mediæval university, required a student to be fairly well trained in the history of philosophy before they would allow him to teach it. The metaphysics of those days was theology, and theology was metaphysics. Now Charles Thurot and many good authorities understand that in Paris, after a man had taken his hood of master of arts, he had to study fourteen years

more before he could be admitted to the degree in theology. That study was, of course, almost entirely historical. Denifle understands that only fourteen years *in all* were required; but what a contrast even that to modern practice.

Dr. Windelband is a German professor of high reputation. His manual history of philosophy, now presented to English readers in such English diction as can emanate from the most modern, if not (after the flesh) the most wealthy, of earth's seats of learning, is well known everywhere; his larger work is celebrated. Throughout Germany, this scholar is extolled for his accuracy, for the energy of thought with which he compresses a whole philosophy into a paragraph, and for his crystal clearness. The most faithful student must tremble at the idea of criticising such a work. We will suppose that the college "senior" for whom the book is intended, reads upon p. 342 about the *Parva logicalia*, which are a series of mediæval treatises explaining how the forms of everyday thought are to be made amenable to strict logical rules--explanations highly useful in times when formal accuracy of logic was exacted in all those scholar's disputations which filled most of the scholar's waking hours. Windelband, speaking of that one of these *Parva logicalia* which relates to "supposition," i.e., the logical denotation of nouns, remarks that the importance attributed to this subject was "not without its precedent in antiquity"; to which he somewhat cruelly adds that "the reader need only be reminded of the investigations of Philodemus on signs and designations." Our student, not being blessed with Teutonic phlegm, blushes at this as a snub; for the investigations of Philodemus with which he is expected to be so familiar are contained only in a somewhat recently transcribed and fragmentary papyrus from Herculaneum, of which Dr. Windelband in this history has only recorded the bare title, {peri sëueiön kai sëmeiöseön}, which he now translates, "On Signs and Designations." Stung to the quick by the imputation of unusual ignorance, our ingenuous youth rushes to the college library, gets the transcription of the papyrus by Gompertz, and proceeds to dig out the Greek until he has mastered the substance of it. Having done this, he finds to his amazement that the title cannot, agreeably to the contents, be understood to mean, "On signs [*i.e.*, words and the like] and designations," but, on the contrary, must be rendered "On signs [*i.e.*, facts symptomatic of other facts] and their significance [*i.e.*, their inferential value]"; and further that the substance of the treatise bears not the remotest affinity with the "supposition" of nouns, but is a discussion of the philosophy and value of inductive reasoning! In short, he discovers that the superlatively learned Windelband can certainly never have opened the volume of which he talks so glibly. After that, the poor fellow will begin to doubt whether Dr. Windelband has so much as read Henry of Ghent (whose works are downright *rare*), though he talks of him as an intimate; and he will almost be tempted to extend that doubt to Richardus de Mediavilla, whose name is printed in this volume Mediavia.

The above is, perhaps, not the worst of swarms of amusing blunders of detail with which the book abounds, and which the translator had better have obtained leave silently to rectify. Roger Bacon is spoken of as a product of the Franciscan order, which is as if Marie Antoinette were called a product of the French

Revolution. Albertus Magnus stands shoulder to shoulder with Roger Bacon--a worse error yet, exposing defective perception of the calibres of men, and at the same time naïve want of acquaintance with the spirit of natural science. Besides, in truth, Bacon never tires of satirizing Albert. True, he does not name him--that were unnecessary and coarse. But there is no mistaking the characterization, which, so aptly fitting the most prominent man in the learned world of that day, cannot be meant for some indiscoverable nobody, when Bacon plainly says he means a person of universal celebrity. Errors of a more important description are equally rife. Windelband speaks of the hæcceity of Scotus as a *form*, instead of a *formality*, or *formal principle*, a widely different thing. This error has been committed by others, but it none the less argues a terrible misapprehension of the central idea of Scotus, as well as a total obliviousness of the literature of the dispute between Thomists and Scotists. We notice, too, that Prantl's unfounded theory of a "Byzantine logic" is spoken of as if it were beyond all doubt. This hypothetical "Byzantine logic" is represented by a single book, the Greek of which is fishy to the last extreme. It is full of phrases which can only be explained by the Latin, and its ideas are even more Latin than its language. In short, it is a manifest translation from the Latin.

Dr. Falkenberg's history is eminently modern in its methods and preferences. It furnishes what may be called the conceptions best received in Germany to-day of the different systems of modern philosophy. It is little colored by personal views, is luminous, sensible, and accurate. Not its least recommendation is that it has been translated by a man able to write an agreeable English style. It is to be feared that unfortunate presswork, quite painful to the eyes, may detract from its usefulness.

Dr. Bascom, who has hit upon an expressive title for such a sketch of history as his, is well known as a dualistic intuitionalist. The business of his life has been to fit out young men destined to practical pursuits with a kit of ready-made opinions and to train them in not criticising the same. Everywhere throughout this book he is occupied with the good or bad practical results of the different philosophies. What has been taught is the question that interests Dr. Bascom, and on that head he makes many shrewd observations, forcibly expressed; but as to what train of thought it was that led to any doctrine he does not care, and teaches his scholars not to care. The most important knowledge, he holds, "admits of no further explanation than is involved in the very act of knowing" (Bascom's 'Psychology,' p. 13). Or, as he says in another place, "sound philosophy gives no entrance to the feeling that, knowing a thing once, we need to know it again, in some other way, in order to know it" ('Historical Interpretation,' p. 390). This is declaring war on criticism in general. Provide yourself, young man (is his general tone), with a full set of sound beliefs, and then take good care of them, and don't indulge in idle longings to know things in two different ways: so you will have a successful and happy life, and go to heaven when you die.

That a history of philosophy ought to be written in the country where it is to be used, is a maxim that gains in weight the more one reflects upon it. Mr. Burt's pretty volumes, besides being written by an American to meet the wants of

American students, present several excellent and original features. In the Kantian volume, one-third of the space is devoted to English philosophy. The corresponding ratio in Falkenberg, though the translator of that work has entirely rewritten and greatly enlarged the English section, is only 1-10. Among German authors, such intense concentration of thought, such subtilty, such fine edge upon ideas as Scotus, Ockham, Hobbes, Hartley, Berkeley, James Mill, Clifford exhibit, will be sought in vain. Everybody knows that there is a German history of philosophy, admirably translated, that has been of very great usefulness in this country; we mean Morris's Überweg. But now that work, more than twenty years old, is growing out of date. Besides, it states only the conclusions of philosophers, not their reasonings, and then, it is written from a foreign point of view. We need an American history of philosophy, upon an encyclopædic scale. It does not matter much whether we have good summaries of Aristotle, Descartes, Kant, nor even of the second grade of philosophers, but it is for information about writers whom few can find time to read, Samuel Parker, Toland, Arthur Collier, Jean Senebier, Tetens, Rosmini, and hundreds of other third-rate philosophers, that such a work is greatly needed. Mr. Burt devotes a section (sometimes several) to each writer, who is taken up, his life briefly narrated, his doctrine stated, and his influence estimated. If Mr. Burt does not display an exceptional power of comprehensive statement, the clear, sensible, and logically excellent arrangement of his work lights it up very much in more senses than one. We could wish he would take more pains to satisfy our curiosity as to just how each metaphysician came to think as he did.

59 (4 October 1894) 251-252: FOUR HISTORIES OF PHILOSOPHY.--II.

A History of Philosophy. With especial reference to the Formation and Development of its Problems and Conceptions.

By Dr. W. Windelband. Authorized translation by James H. Tufts, Assistant Professor in Chicago University. Macmillan & Co.

History of Modern Philosophy.

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CSP, identification: same as 59 (27 September 1894) 237-238.

In the following review, one learns a great deal about Windelband and his *History of Philosophy*, but very little about the translator. James Hayden Tufts was an 1884 graduate of Amherst College, and took his Ph.D. from Yale in 1889. He was a teacher of mathematics and philosophy and a contributor to Baldwin's *Dictionary of Philosophy and Psychology* on topics of ethics. Tufts was himself an author on the history of philosophy.

John Bascom (1827-1911) graduated from Williams College in 1849 and from Andover Theological Seminary in 1855. For nineteen years, Bascom held the post of professor of rhetoric at Williams College. In 1874, he left Williams to become president of the University of Wisconsin and chairman of that school's department of philosophy. After twelve years, however, he returned to Williams, where he lectured on sociology and political science. Bascom authored twenty-two books and more than two hundred articles.

In order that the reader may be able to compare the style of the four books, we will quote a part of what each says about Berkeley, selecting this subject as familiar and as capable of brief treatment. Of course, there is not room for two opinions regarding Berkeley's place in history. What Windelband says is distributed in five different places, although Berkeley's system is as clearly "all of a piece" as can be. In one of these places we find this characteristic specimen of English:

"As the ambiguous, indeterminate nature of Locke's psychology unfolded itself in the antithesis in the following developments, so, too, this epistemological metaphysics offered points of departure for the most varied transformations. The very first of these shows an audacious energy of one-sidedness in contrast with the indecisiveness of Locke. Berkeley brought the ascendency of inner experience to complete dominance [why not say he brought the dominance of inner experience to complete ascendency? One phrase seems to mean as much as the other] by putting end to the wavering position which Locke had taken [not that he influenced Locke, who was dead and gone; but he put an end to the position which had wavered while Locke was in it] upon the question as to the knowledge of bodies. This he did with the aid of his extreme Nominalism and with a return to the doctrines of Hobbes. He demolished the conception of corporeal substance. According to the distinction of primary and secondary qualities, it was held that a part of that complex of ideas which perception presents us as a body should be separated out [he means eliminated] and another part retained as alone real; but this distinction, as Hobbes had already taught, is in the nature of the case erroneous. The 'mathematical' qualities of bodies are as truly ideas within us as the sense qualities, and Berkeley had demonstrated exactly this point with analogous arguments in his 'Theory of Vision.' He attacks the warrant of the distinction of Descartes (and of Democritus). [This reference to Descartes and Democritus has nothing to do with Berkeley.] But while, according to this view, all qualities of bodies without exception are ideas in us, Locke has retained as their real supporter a superfluous unknowable 'substance'; in a similar way others speak of matter as the substrate of sensible qualities" (p. 469).

Now let us see how Falkenberg expresses precisely the same ideas:

"Berkeley is related to Locke as Spinoza to Descartes. He notices blemishes and contradictions allowed by his predecessor to remain, and, recognizing that the difficulty is not to be remedied by minor corrections, goes back to fundamental principles, takes these more earnestly than their author, and, by carrying them out more strictly, arrives at [attains] a new view of the world. The points in Locke's doctrines which invited further advance were the following: Locke proclaims that our knowledge extends no further than our ideas, and that truth consists in the agreement of ideas among themselves, not in the agreement of ideas with things. But this principle had scarcely been announced before it was violated. In spite of his limitation of knowledge to ideas, Locke maintains that we know (if not the inner constitution, yet) the qualities and powers of things without us, and have a sensitive certainty of their existence. Against this, it is to be said that there are no primary qualitites, that is, qualities which exist without as well as within us. Extension, motion, solidity, which are cited as such, are just as purely subjective states in us as color, heat, and sweetness. Impenetrability is nothing more than the feeling of resistance--an idea, therefore, which self-evidently can be nowhere else than in the mind experiencing it. Extension, size, distance, and motion are not even sensations, but relations which we in thinking add to the sense-qualities (secondary qualities), and which we are not able to represent apart from them; their relativity alone would forbid us to consider them objective. And material substances, the 'support' of qualities invented by the philosophers, are not only unknown but entirely non-existent. Abstract matter [this is not very good English. "Material substance" is Berkeley's expression] is a phrase without meaning, and individual things are collections of ideas in us, nothing more. If we take away all sense qualitites from a thing, absolutely nothing remains. Our ideas are not merely the only objects of knowledge, but also the only existing things--nothing exists except minds and their ideas. Spirits alone are active beings, they only are indivisible substances and have real existence, while the being of bodies (as dependent, inert, variable beings, which are in a constant process of becoming ["forever changing," "in a perpetual flux," are Berkeley's expressions. 'Siris,' §§ 344 *et seqq*.]) consists alone in their appearance to spirits and their being perceived by them. Incogitative, hence passive, beings are neither substances nor capable of producing ideas in us. Those ideas which we do not ourselves produce are the effects of a spirit that is mightier than we.

"With this a second inconsistency was removed which had been overlooked by Locke, who had ascribed active power to spirits alone and denied it to matter, but at the same time had made the former affected by the latter. If external sense is to mean the capacity for having ideas occasioned by the action of external material things, then there is no external sense.

"A third point wherein Locke had not gone far enough for his successor concerned the favorite English doctrine of nominalism. Locke, with his predecessors, had maintained that all reality is individual, and that universals exist only in the abstracting understanding. From this point Berkeley advances a step further--the last, indeed, which was possible in this direction--by bringing into question the possibility even of abstract ideas. As all beings are particular things, so all ideas are particular ideas."

The above two presentations of Berkeley are as alike as two peas or as two synoptical gospels, and illustrate what advantages and disadvantages the Germans derive from thinking gregariously.

The following is about one-fifth of what Bascom has to say about Berkeley, and we select the passage in which he has the most to say about methods of reasoning:

"Bishop Berkeley stands quite by himself. Idealism has played a very secondary part in English philosophy. The idealism of Berkeley did not arise from magnifying

mental processes, and displacing with them the physical phenomena disclosed in the senses, but sprang from the dualism of Descartes and from the weakness involved in empiricism itself. Empiricism becomes uncertain in its affirmation of any exterior reference of sensations. The mind is so robbed of its native powers as to be able to make no primitive assertion with certainty. Sensations, as simple phenomena, overmaster the mind and hold it in subjection to themselves. Mill gave this tendency full expression in regarding matter as only the possibility of sensations. The correct and firm reference of our ideas became impossible. Berkeley, much impressed by the empiricism of Locke, and escaping the fracture in the universe involved in the system of Descartes, affirmed the true origin of sensations is the divine mind."

The following is about a third of Mr. Burt's account:

"To the query 'whether a man born blind and then made to see would at first give the name distance to any idea (object of consciousness) intromitted by sight,' Berkeley's answer is that he would 'take distance that he had perceived by touch to be something existing without his mind, but would certainly think nothing seen was without his mind.' He would come to perceive distance by sight, only as he learned to interpret visual impressions by impressions of touch and bodily movement. By experience he would become able to 'perceive' distance at once by sight: every visual impression would instantaneously receive an interpretation in the language of touch and movement. But, this being the case, all vision would, in a very important sense, be prevision; visual perceptions are, unconsciously to ourselves, created for us beforehand by experience; and every idea or object of (visual) consciousness would presuppose a subject of consciousness or mind. What is true of vision is true of all forms of sensible experience. Why the sensations of one sense thus receive interpretation in the language of another, and why certain impressions of different senses are uniformly conjoined to consitute [[sic]] the idea of a fixed object, we do not know, any more than we know why words in English, Greek, or any other language have the significations they have for us. Certain it is that we find in experience ideas or objects existing in regular coexistence and succession, or in an order--which order we know, from the manner in which we get these ideas, and from the fact that they form an order, to be inseparable from mind. Such being the case, the traditional notions of matter, substance, and the like which suppose a real existence apart from mind, are 'empty metaphysical abstractions,' a 'dust raised by metaphysicians that prevents their seeing clearly.' The notion of matter is self-contradictory; 'matter is something that is not, and yet at the same time *is* for consciousness,' since we cannot attach any meaning to the term 'matter' without giving matter an existence for the mind, or 'bringing it within the mind.' The very being of all objects for us consists in the 'being perceived and known.' What does not exist in *my* mind or that of some other mind or spirit, finite or infinite, cannot have existence. The self-contradiction inherent in the notion of matter [misprinted, water] does not appertain to that of spiritual substance. The words *I* and *you* have certain intelligible meanings which warrant our speaking of spiritual beings, though they be not exactly phenomenal."

This is perhaps not quite so forcible a presentation of Berkeley as the Germans give; but it is thought out by the author for himself, and presents the subject in the fresh light of a new morning.

59 (25 October 1894) 312-313: **Essays in Historical Chemistry**.

By T. E. Thorpe, Professor of Chemistry in the Royal College of Science,

South Kensington. Macmillan & Co. 1894.

Attributed to Peirce by Fisch in *First Supplement* (internal evidence). Peirce definitely reviewed the later edition of this book at 75 (21 August 1902)

153-154. Peirce was a Boyle enthusiast. This piece is unassigned in Haskell's *Index to* The Nation, vol. 1.

Sir Thomas Edward Thorpe (1845-1925) was a distinguished British scientist and educator. He held the post of professor of chemistry at Andersonian College, Glasgow, from 1870 until 1874, at which time he went to Yorkshire College of Science, Leeds. In 1885, Thorpe began an association with the Royal College of Science at South Kensington which continued for several years. He was knighted in 1909.

In this volume Prof. Thorpe has collected ten addresses and lectures which he has delivered at various times, and to audiences of very different type, during the past eighteen or twenty years, and three articles which have appeared in as many different periodicals. Making no pretension to be a history of chemistry, even during the period covered by its narratives, the book presents a series of biographical sketches which are "put together with the object of showing how the labors of some of the greatest masters of chemical science have contributed to its development." The essays are arranged in historical sequence, beginning with a lecture on Robert Boyle, continuing with sketches of Priestley, Scheele, Cavendish, Lavoisier, Faraday, Graham, Wöhler, Dumas, Kopp, and Mendeleeff, and concluding with an address on the "Rise and Development of Synthetical Chemistry."

The great variety of occasion for which these essays were originally prepared has left its mark upon them; the discussion of details of chemical accomplishment which forms the principal part of the longer essays on Graham and Kopp being in striking contrast with the more popular treatment which renders most of the others so readable. The chemist will not quarrel with the essayist on this account, however, but will rather be grateful to him for so full and adequate a summary of the labors of these eminent investigators. In reading these admirable and scholarly essays one is unavoidably impressed with their resemblance, in ease and clearness, to the felicitous biographical sketches which Hofmann gave us from time to time as memorials of the scientific worthies of the Continent. Prof. Thorpe has in most instances well preserved the human interest in his treatment. The men appear not merely as chemists and investigators, but also as citizens of the world. The estimate of their work is, on the whole, discriminating and just.

In the "Honorable Robert Boyle, seventh son of the Great Earl of Cork," born in 1626, the year of Bacon's death, the "sceptical, inquiring, reforming spirit" of the age found its expression in the domain of natural science. Under the title of the 'Sceptical Chymist: or Chemico-Physical Doubts and Paradoxes touching the Experiments, whereby vulgar Spagyrists are wont to endeavour to evince their

Salt, Sulphur, and Mercury to be the true Principles of Things,' Boyle published anonymously in 1661 a book which attracted immediate and eager attention not only in England, but on the Continent, where no less than ten Latin editions of it appeared. "In its revolt against mere authority, in its disdain of old-world notions, and in its ill-concealed contempt for the schoolmen, it so exactly caught and expressed the spirit of the time that it instantly arrested the attention of the learned world, and . . . of that infinitely larger public of thinking men who felt a growing impatience of the dogmas of the schools." Boyle, with his disciples Hooke and Mayow, founded the first school of scientific chemistry, and was a member of the so-called "Invisible College, an assembly of learned and curious gentlemen who applied themselves to the study of experimental science," out of which grew the Royal Society of London, incorporated by Charles II. in 1663. "The growth of the new philosophy excited the jealousy and anger of those who affected to see in the ascendancy of the Baconian method the subversion of everything that was orderly and of good repute.... Bishops anathematised; Hobbes... thundered; Butler lampooned." But in spite of rough usage the Society continued to grow and prosper, and science even became fashionable.

Prof. Thorpe closes his summary of the 'Sceptical Chymist' by saying: "I have purposely quoted very largely from it, for I wished to show you, in Boyle's own words, how wonderfully near much of the philosophy of the seventeenth century is to that which we are too apt to regard as the outcome of the nineteenth. It is impossible to exaggerate the importance of Boyle's labors.... The work exhibits in an eminent degree Boyle's character as an investigator," his strength and his weakness. "But to say that Boyle is only inferior to Bacon and Newton is to assign him one of the first niches in the Walhalla of the heroes of science." "The 'Sceptical Chymist' sealed the fate of the doctrine of the *tria prima*, and before the close of the century the Paracelsians were as much out of date as a Phlogistian would be to-day."

Priestley, theologian and chemist, the "Father of Pneumatic Chemistry," the keen and forcible controversialist, is a notable figure in the history of chemistry. Indefatigable and successful in his experimental work, he failed to see the important bearing of his chief discovery.

"The discovery of oxygen was the death-blow to Phlogiston. Here was the thing which had been groped for for years and which many men had even stumbled over in the searching, but had never grasped."

"The knowledge which Priestley . . . imparted to the French chemists was used by them with crushing effect against his favorite theory.... Priestley, however, never surrendered.... When age compelled him to leave his laboratory, he continued to serve the old cause in his study, and almost his last publication was his 'Doctrine of Phlogiston Established.' His own life, indeed, affords an exemplification of the truth of his own words, that 'we may take a maxim so strongly for granted that the plainest evidence of sense will not entirely change, and often hardly modify, our persuations; the more ingenious a man is, the more effectually he is entangled in his errors, his ingenuity only helping him to deceive himself by evading the force of truth.'"

"That he was content to rest in the faith of Stahl's great generalization . . . is the more remarkable when we recall the absolute sincerity of the man, his extraordinary receptivity, and, as he says of himself, his proneness 'to embrace what is generally called the heterodox side of almost every question.'"

Of Scheele, Prof. Thorpe says:

"An obscure apothecary, living . . . in a small town on the shore of a Scandinavian lake, hampered by poverty and harassed by debt, hypochondriacal, and, at times, the victim of the most depressing melancholy, he yet succeeded, by the sheer force of his genius as an experimentalist, and under the influence of a passion which defied difficulty and triumphed over despair, in changing the entire aspect of a science. No man ever served chemistry . . . with more interested devotion than Scheele. 'Diese edel Wissenschaft,' he wrote to his friend Gahn, 'ist mein Auge.' . . . When every legitimate deduction has been made, Scheele's work . . . stamps him as the greatest chemical discoverer of his age."

"Cavendish, a scion of a great house, was cold, retiring, reticent, passively selfish, a confirmed misogynist, a hater of noise and bustle. It was said of him that he probably uttered fewer words in the course of his fourscore years than any man who ever lived so long--not even excepting the monks of La Trappe.... Mr. Cavendish rarely did violence to his love of solitude by asking any one to his house. If a friend chanced to dine with him, he was invariably treated to a leg of mutton, and nothing else. We are told that on one occasion, three or four guests being expected, he was asked what was to be got for dinner. He replied with the customary formula, 'A leg of mutton.' 'But,' said the servant, 'that will not be enough for five.' 'Then get two legs,' was his answer."

These few extracts will serve to show something of the quality of these essays, which many besides chemists will find very interesting and enjoyable reading.

59 (8 November 1894) 343: NOTES

--The last number of the Proceedings of the Psychical Research Society contains, in its report of the Committee on the "Census of Hallucination," the solidest and heaviest piece of work which it has yet published; and if dryness and dulness can make a thing scientific, then these 400 pages are as scientific as the most fastidious intellect can desire. Stories of apparitions, however flesh-curdling they may be when told singly in the twilight, are terribly monotonous when cast into the form of printed depositions and taken in bulk. The Census of Hallucinations was, as is well known, an idea of the late Edmund Gurney, who thought that the proportion of frequency of apparitions-coinciding-with-death to other apparitions might decide whether the former were or were not something more than accidental coincidences. The present committee has had 17,000 persons interrogated as to whether they have had, when awake, etc., an hallucination or not. Of these, 1,684 answered yes, and the details of the experiences form a body of data which the committee go over and discuss in every possible way. As regards the main

question, the committee's verdict is *tout ce qu'il y a de plus* affirmative. Their calculation of probability is based, for obvious reasons, on one sort of "veridical" phantasm, that, namely, in which on the day of a person's death a distinct visual hallucination of his presence appears to some one at a distance. Their statistical argument against chance is as follows: According to the registrar-general's report, 1 person in 19,000 dies daily in England. If there be no causal connection between apparitions and deaths, and if apparitions of persons strike persons at random (so to speak), being no more likely in advance to strike a dying one than a living one, we should expect them to strike the living 19,000 times more frequently than the dying, because living men are 19,000 times more frequent facts than dying men. In other words, the antecedent probability that a person will die on the day on which his apparition is seen is only 1 in 19,000. Now the figures of the returns give a much smaller fraction than this, even when corrected so as to be on the safe side of every objection that can be urged against their accuracy. They give, even with these corrections, 30 apparitions on the day of the death amid an aggregate of 1,300 apparitions of recognized living persons, being at the rate of 1 in 43, or 440 times the most probable chance number, of 1 in 19,000. In fact, since many of the apparitions seem to have occurred not merely on the day, but at the very hour, of the death, the improbability of the explanation by chance is really much greater than this figure. Disbelievers in occult causes cannot well attack the reasonings of the committee, which, by the way, is headed by Professor and Mrs. Henry Sidgwick. They will have to attack the premises, and say either that the narratives are so essentially inaccurate that no results can be pinned upon them, or else that the figures are still too small to reach a "law" by, and that the Census must be continued elsewhere. The American continuation of the Census is not yet published. Meanwhile, it is fair to say that this ponderous contribution to the subject has once for all redeemed the theory of an occult connection between apparitions and deaths from the status of a "superstition," and has captured for the "telepathic" hypothesis which the committee professes, the right to a patient and respectful hearing before the "scientific" bar.

59 (8 November 1894) 344-345: SPINOZA'S ETHIC

Ethic.

Translated from the Latin of Benedict de Spinoza by W. Hale Wright; translation revised by Amelia Hutchinson Stirling, M.A. (Edin.). Second edition, revised, with a new preface. Macmillan, 1894.

CSP, identification: Haskell, *Index to* The Nation. See also: Burks, *Bibliography*; *List of Articles*.

Although this purports to be only a second edition, yet as it has a new "preface" of over a hundred pages and the translation is improved and perfected, and as the book is immortal and ever fresh, our readers will be glad to have some account of the volume. Let us first lament that the work has to be translated at all. A scientific man

may get along without German, a waiter in a restaurant without French, a traveller in the Levant without Italian, but a metaphysician without Latin never. Moreover, Spinoza's language is beautiful, and crammed with meaning that no version can convey. Then, too, a student of philosophy who does not know Latin cannot understand the older English writers, whose terminology is based on that of scholasticism--indeed, we may say he can have no accurate knowledge even of the less modern literary English. Needed, however, without doubt, however unhappily, the English version was; but we cannot help thinking that those who will read it would have been helped in mastering the thought if the original had been printed on the opposite pages. At the very least, the Latin of important phrases should have been added in parenthesis. For instance, the opening words are "By cause of itself, I understand," etc. Now, this expression, like many others of the treatise, is frequently quoted by philosophical writers, and almost invariably in the language of the original.

Spinoza, we need not say, is rated very high by all the idealist schools--that is to say, by all who go very deep into metaphysics; and he is without question one of the most, if not the most, difficult of such writers. His thought is so high and abstruse that nobody who has not reflected long and to good purpose can appreciate it. But that is not all, nor the worst of it. Paradoxical as it may seem, it may be maintained that none of the very great philosophers understand themselves. Crystal clearness, such as we justly require in mathematics, in law, in economics, is in philsophy the characteristic of the second-rates. The reason is that the strongest men are able to seize an all-important conception long before the progress of analysis has rendered it possible to free it from obscurities and difficulties. If Kant had waited, before he wrote the 'Critic of the Pure Reason,' until the ideas with which it chiefly deals had been accurately dissected, he might, had he lived, have been pottering over it to-day. But of Spinoza this is true in a much higher degree. Not only has he not mastered an altogether distinct apprehension of his own thought, but he has a positively mistaken view of it. He thinks that he reasons after the style of Euclid, and perhaps there is some truth in that; but he thinks that his reasoning has the form which Euclid understood his own to have, and that is a complete delusion. This appratus of Definitions, Postulates, Axioms, Problems, and Theorems is in geometry itself merely a veil over the living thought. Hence it is that Euclid's manifold slips in logic have scarce cast a shadow of doubt over the substantial truth of his propositions. The history of mathematics justifies the presumption that just in proportion to the importance of a theorem is the demonstration of it likely to be fallacious--or, at least, it would be so were the proposition stated in the absolute style of Euclid. Thus, the fruitfulness of Cauchy's work is intimately connected with its logical inaccuracy. Dirichlet's principle, which powerfully aided the development of modern mathematics, is well known to be logically unsound; and much of the foundations of the theory of functions which has never been called in question--even, for example, the passage from one branch of a function to another--cannot sustain cross-examination.

Some mathematical results, doubtless, could have been worked out with Babbage's analytical engine; but did anybody ever suppose that the subject could at all

be really advanced by such a machine? Yet the current notion is that syllogistic reasoning is wholly mechanical, and that mathematics proceeds by syllogistic reasoning. Neither proposition is true. Even syllogistic reasoning in its higher varieties as they appear in the logic of relatives, requires a living act of choice based on discernment, beyond the powers of any conceivable machine; and this sufficiently refutes the idea that man is a mere mechanical automaton endowed with an idle consciousness. Moreover, the real procedure of mathematical thought is not merely syllogistic even in this loftier sense. Mathematical thought advances chiefly by generalization; and the generalized conclusions are made rigorously logical by the device of correspondingly generalizing the premises. But mathematical generalization is not the infantile process which the logic-books describe; for they think of no relations between individuals except those which consist in those individuals having common simple predicates. Let the predicates be relational, and generalization means organization, or the building up of an ideal system. Mathematical reasoning consists in thinking how things already remarked may be conceived as making a part of a hitherto unremarked system, especially by means of the introduction of the hypothesis of continuity where no continuity had hitherto been thought of.

It is difficult to find an illustration of these assertions suited to our columns, because it would need to embrace a whole sequence of theorems--such a sequence as mathematicians term a "theory." Nevertheless, we will essay it. Euclid, or rather pre-Euclidean geometers, easily saw that the three angles of a plane triangle added up, very nearly at least, to two right angles, while those of a triangle drawn on a level (and therefore spherical) surface were greater. The question therefore arose: Are they exactly 180º in the former case? They drew a triangle with a horizontal base and a higher vertex. From the left-hand angle they drew a line bisecting the opposite side, and then conceived this line to be doubled. They assumed that the right hand extremity of this doubled line would be higher than the base of the triangle. There was no logical proof of it. They knew it would not always be so upon a sphere. To assume it to be so was therefore to beg the very question at issue, namely, whether a triangle on a plane was like a triangle on a level (and therefore spherical) surface, or not. Syllogistically, it was illogical. Considered as mathematics, it was merely the ordinary procedure whereby something is added to the original hypothesis. Considered as physics, it was quite unjustifiable to assume that their idea of space corresponded precisely to the space of the real world.

Here is a better example: Boole discovered that if he simply assumed 1 to signify what is, and 0 what is not (and any other two numbers would have equally answered the purpose), he could without any further assumption express the premises of a syllogism as two equations from which, by ordinary algebraical rules, the conclusion could be deduced. This was a genuine, living thought, and as such is quite beyond the appreciation of seminary logicians. Its value consisted in its bringing the conceptions of being and nothing into relation with the system of numbers, and especially exhibiting them as the mere punctual terminations of the continuous quantity between them. This last part of the idea coincides with that of Hegel's Becoming, though this latter, besides its inconvenient lugging in of Time, is less useful as being less diagrammatic. However, Hegel's reasoning and Boole's were essentially the same, and this was nothing but an example of the ordinary mathematical proceeding. Boole's form of statement can easily be made a theorem, and can be furnished with a demonstration of the usual degree of irrefragability, or the reverse, as you will. But such demonstration completely overlooks all that there was of life and of value in the thinking.

All this is eminently pertinent to Spinoza. It is more than pertinent--it is indispensable to the comprehension of him. His 'Ethics' (which these translators call "ethic," following the ignorant corruption of *ethica* from a neuter plural to a feminine singular) is likewise drawn up in theorems, with demonstrations which have always furnished a laughing-stock to mathematicians. But you must penetrate beneath these if you would enter the living stream of Spinoza's thinking. You then find that he is engaged in a somewhat mathematical style in developing a conception of the absolute, strikingly analogous to the metrical absolute of the mathematicians. He thus appears as a mathematical thinker, not in the really futile, formal way in which he and his followers conceived him to be, but intrinsically, in a lofty, living, and valuable sense. But whether or not this ideal absolute which he brings us to conceive has anything at all in the real world corresponding to it, is a problem which simple thinking cannot solve. That must be brought somehow to the bar of experience, or remain a pure ideal. Yet, even so, it would not do to assume the speculation to be useless; for one might by the same reason conclude mathematics useless, that being only the study of ideal constructions. Spinoza himself of course reposed in childlike faith in the objective truth of his ideas. The 'Ethics' was written in the midst of those discussions about the principles of dynamics which for many years occupied the attention of the whole learned world, and which were brought to a conclusion in Newton's 'Principia.'

Now these discussions related to matter of fact; and yet their method invariably was to develop the writer's instinctive notions. Thus, Galileo, seeing that a falling body evidently falls faster and faster, only stops a moment to show that there would be serious difficulties in the way of supposing the velocity to be proportional to the distance fallen from rest, and then at once adopts the correct idea that the incremental velocity is proportional to the time that increment occupied. Spinoza's reasoning is precisely of the same nature. But what he and all his school fail to remark is, that the conclusions of the students of mechanics are sure to be brought to the test of experiment in various ways and without any public remark, unless the experiments fail. It is just that quiet verification that makes all the difference in the world. A hypothesis of any kind has no positive support until it has predicted something capable of being observed and that prediction has been verified. Nevertheless, it must be conceded that as soon as they had learned to introduce the idea of continuity, a very considerable proportion of the instinctive ideas of students about forces turned out to be just, and the rest needed only slight correction.

Spinoza's ideas are eminently ideas to affect human conduct. If, in accordance with the recommendation of Jesus, we are to judge of ethical doctrines and of philosophy in general by its practical fruits we cannot but consider Spinoza as a very

weighty authority; for probably no writer of modern times has so much determined men towards an elevated mode of life. Although his doctrine contains many things which are distinctly unchristian, yet they are unchristian rather intellectually than practically. In part, at least, Spinozism is, after all, a special development of Christianity; and the practical upshot of it is decidedly more Christian than that of any current system of theology.

There are at least three good translations of the 'Ethics' into English--that of R. Willis, M.D., affixed to his interesting Life of Spinoza (Trübner, 1870); that of Daniel Drake Smith, published separately (Van Nostrand. 1876); and the present one, which originally appeared in Trübner's "English and Foreign Philosophical Library." The latest is distinctly the best of the three translations. None of them prints the Latin. The long "Preface" is occupied chiefly with an analysis of the development in Spinoza's mind of the doctrine of the 'Ethics' as shown in his earlier work, 'A Short Treatise upon God and Man's Well Being,' and in his correspondence. This is interesting and throws some light upon the doctrine itself. There is no really thorough book about Spinoza in our language, though there are several that contain much that is valuable, especially those of Frederick Pollock and of Caird. The best edition of his works is by Van Vloten and Land (The Hague, 1883). In regard to the relation of Spinoza to the philosophers who went before him, much has been done in special directions, one writer urging his indebtedness to Descartes, another that to scholasticism, a third that to the Jewish philosophy, a fourth that to Giordano Bruno. But no really good comprehensive view has ever been published; nor, singularly enough, has anybody remarked, as far as we are aware, the very obvious indebtedness of Spinoza to Hobbes, to whose wooden mechanicalism he was naturally inclined.

59 (22 November 1894) 381: HALLUCINATIONS

This letter is a reply to the anonymous note at 59 (8 November 1894) 343.

TO THE EDITOR OF THE NATION:

SIR: Permit me, as the spokesman for a number of persons who carefully examined and rejected the evidence about phantasms of the dying presented in the book called 'Phantasms of the Living,' to say that we do not consider the new Census of Hallucinations as satisfactory, nor the conclusion from it legitimate; and that our objection is to the committee's logic. It is true that for any ordinary case one might let the evidence go as sufficient, but, the conclusion being so revolutionary, in our opinion an exacter proof is necessary.

We object to your assumption that whoever rejects the reasoning of the committee is necessarily a positive disbeliever in the reality of the phantasms. I, for my part, in my attack on that book, fully admitted that it ought to be regarded as sufficient to silence any pooh-poohing of the belief in ghosts.

C. S. PEIRCE.

ARISBE, MILFORD, PA., November 13, 1894.

59 (22 November 1894) 383: NOTES

This note is probably by Peirce because he had translation rights to this book at one time. Also, Peirce was an admirer of Wundt's work. This item is unassigned in Haskell's *Index to* The Nation, vol. 1.

--Prof. Wundt's 'Vorlesungen über Menschen und Thierseele,' published more than thirty years ago, can be named with E. H. Weber's articles, with Fechner's 'Psychophysik' and Helmholtz's works on sight and hearing as one of the pioneers of that experimental method of study in psychology which has become so powerful since. In 1892 Wundt published an edition of it with alterations and omissions, which we have now done into English by Professors Creighton and Titchener of Cornell, and published by Macmillan. There is much to be said against issuing an altered edition of a book that has marked a moment in the development of a science. You deprive it of its value as an historic document, and you fail to make it a genuinely modern work. Prof. Bain's recently published fourth edition of his 'Senses and Intellect' is a case strikingly in point. The translators of the present work, moreover, tell that they "have aimed to furnish a literal, as distinguished from a verbal (*sic*), rendering of the German text, . . . even at the occasional cost of literary effect." These two facts made us open the book with some alarm; but happily it proved causeless. Prof. Wundt's unexampled cleverness in bookmaking has practically led him to write a new work altogether, in which passages from the earlier one have been inserted only where they would still serve; and the translators have not fulfilled their awful threat, but have given us English that is quite unbarbarous even though it may at times be slightly heavy. The result is one of the most available text-books for use in colleges which our language now possesses. It is needless to say that the doctrines of the earlier work the author has not left many unchanged. In particular the original explanation of so many sensations and judgments by "unconscious judgment" has of course been wholly abandoned, and mechanism and logic are no longer affirmed to be the same. Many of the results of Wundt's later thinking, as expressed at length in his larger books, are here to be found in compendious and popular form; and for a reader who wishes to make some acquaintance with the distinguished Leipzig philosopher, these lectures are decidedly the most advantageous channel of approach.

59 (29 November 1894) 409: NOTES

--Regarding the review of Spinoza's 'Ethic,' in No. 1532 of the *Nation*, a correspondent writes:

"Your reviewer ought to have been aware of an American translation. Had he been, he would not have mentioned only those made abroad, as if none had been made at home. He blames his translators for their title *Ethic* as 'an ignorant corruption of *Ethica* from a neuter plural to a feminine singular.' The American

translator did not make that mistake, and his work seems for many reasons more worthy of reproduction than that reviewed. This translator was Henry Smith, who, in 1836, was a university student in Germany, who, in 1844, published a translation of Crusius's 'Homeric Lexicon,' and for twenty years, as professor and president at Marietta, did much for that study of Latin which the reviewer so extols. At Lane Seminary Dr. Smith for twenty years lectured on Spinoza, and had completed his translation of the 'Ethics' before he had ever seen any other in English. Though his style is terse, his prolegomena fill 179 pages, embracing a minute analysis of every point in the 'Ethics,' as well as the relations of that work to other philosophic systems, *e. g.*, those of Bacon, Descartes, Kant, Coleridge, and Hamilton. His work 'Spinoza and his Environment: A Critical Essay, with a translation of the Ethics,' was ready for the press in 1877. Its publication, delayed by *res angusta domi* and the author's death, was in 1886, by Robert Clarke & Co. The 'Ethics' was his pet theme, and so his students brought it out *in memoriam*."

Another correspondent, writing in praise of Elwes's translation of the 'Ethics,' calls our attention to the fact that he was well aware of Spinoza's acquaintance with Hobbes, and cites a passage to that effect.

59 (6 December 1894) 430-431: **Modern Scientific Whist**.

By C. D. P. Hamilton. New York: Brentano's. 1894.

CSP, identification: MS 1388 (draft). See also: Fisch, *First Supplement*.

Charles Dingee Pennock Hamilton (1851-1940) was a self-made man, having risen through the ranks of small time manufacturers to become the head of one of the largest manufacturing firms in the United States, the International Shoe Company. During World War I, Hamilton was chief of the boot and shoe section of the War Industries Board. He was widely known as a great chess player. He excelled at whist, and when that game was at the height of its popularity, he was considered the world's greatest whist authority. Hamilton's *Modern Scientific Whist* was accepted as the rule book of the game.

This is a volume of 600 pages, the largest and incomparably the most thorough of all treatises on its subject. Our readers need not be informed that the invention in 1883 of the "American leads" has elevated whist to a new position and dignity. A strong light is thrown upon the mathematical (if a little dry) quality of the American mind by the great access of popularity of the game that resulted from those improvements, although they imparted to it a severer character than it had had before. Formerly good players were to be met with in only half a dozen clubs in all the land; now they swarm.

The analysis of the play of the Third Hand occupies no less than eighty of Mr. Hamilton's pages. The illustrative examples are of a very high order of excellence. We will quote one specimen, although it must be without the diagram given in the book, and without the lucid explanation of the principles it involves. But the reader should take the cards and look at this. The players A, C, B, D sit in this order of play. A holds: Spades, K, 9, 8; Hearts, 5; Diamonds, 8. C holds: Spades 4; Hearts, K, X; Clubs (which are *trumps*), X, 6. B holds: Spades, X, 5;

Hearts, 9; Clubs, J, 7. D holds: Spades, 7, 3, 2; Diamonds, 5; Clubs, 9. Two rounds of trumps (Clubs) are supposed to have been played; and it has been made apparent that C and B each hold two more, and that B probably has a tenace over C. C is known to have the best Hearts. A knows that D holds the 5 of Diamonds. A has the lead. Here is the play:

1st Trick--Spades K, 4, X, 2.

2nd Trick--Diamonds 8; Hearts X; Spades 5; Diamonds 5.

3rd Trick--Spades 8; Clubs X, J; Spades 3.

4th Trick--Clubs 7, 9; Hearts 5; Clubs 6. This is fine whist.

It could not but be that a few of Mr. Hamilton's propositions are open to dispute. He is among those who are least favorable to playing for cross-ruffs. He says it is "cheap whist." So much the better: there is nothing so elegant as economy. He says: "It is *never* right to play a false card." This refers apparently to some standard of right and wrong too lofty to consider winnings. He says further:

"The vast majority of American whist-players play whist purely for the intellectual pleasure it affords--there is no other incentive. With such players the mere making of tricks is a secondary object, and to either make or lose a trick through deception is equally unsatisfactory. If the right to play false is recognized, there is then no limit to its pernicious and disintegrating practice."

This comes perilously near to silly pedantry. Of course we play for pleasure, and trying to make tricks is secondary to that pleasure; but there would be no pleasure in it if we had to have a schoolmaster put over us to make us play exactly as *he* likes, whether his way be conducive to winning or not. People who are too virtuous to wish to succeed by the aid of a *ruse*, even in a game, are (thank Goodness!) not long for this world. How such people can be so truculent as to wish to conquer their opponents at all puzzles us. It may be remarked that the whole practice of modern whist is a development of the Blue Peter, which was originally a sharper's cheating device. Our national horror for everything like deception and guile is well shown in our childlike game of poker.

As whist is largely an art of expressing one's self, we should expect to find whist-players fastidiously precise in their style of writing. If it be so, it cannot be proved from this book. Too many sentences are arranged like this: "You must always recollect, when planning any finesse, that if your finesse loses, the immediate loss is nearly always modified, provided, of course, that your finesse was justifiable, or turned into a gain in after-play." Or this: "In the last stages of the hand you are sometimes put to discard, holding a winning card of two plain suits, the opponent holding a losing card in one of them, but uncertain which." Or this: "Suppose partner opens the hand with knave of trumps; you have not one; your discard is from your weakest suit, but it may happen that it is injudicious to do so, owing to the unusual character of your hand." Injudicious to suppose partner so opens the hand, or injudicious so to open it?

Notwithstanding the bulk of the volume, we find nothing in it we should be willing to spare, except four pages of maxims written in the style of morals to

German fables, a style abominable in itself and absurd as applied to whist. On the other hand, there are several omissions we should be glad to have supplied in appendices to a new edition. Firstly, we think the work would hold its ground longer if it contained a discussion from the point of view of a wicked whist-player of the questions of cross-ruffs and false cards. Mr. Hamilton's loathing of deception is so great that we fear we can hardly hope for a quite accurate statement of the case from his pen. Such strong feelings are not favorable to scientific truth. We should also like some instructions how to play with an old-fashioned partner against two modern opponents; and in these, too, we demand a real low, sordid, trick-taking spirit--the moral tone of a man who would not stickle at playing blind-man's buff itself. We should also like chapters on dummy, single and double. Finally, an extensive collection of calculations of chances would be instructive. Hamilton's 'Modern Scientific Whist' must for a long time be the leading treatise, and consequently all whist-players must desire that it should be complete.

59 (27 December 1894) 476-477: DESCARTES AND HIS WORKS

CSP, identification: Haskell, *Index to* The Nation. See also: Burks, *Bibliography*; Fisch and Haskell, *Additions to Cohen's Bibliography*; MS L

159.70.

An edition of all the writings of Descartes really does not exist. Every collection is incomplete, and gives only translations either of the French writings into Latin or of the Latin writings into French. The French ministry of public instruction has now assumed the duty of bringing out a worthy edition. M. Charles Adam, in the *Revue Internationale de l'Enseignement* for November 15, considers what researches have to be made and how much can be expected from the new edition. In regard to Descartes himself and his philosophy, extremely little is to be expected. He is the subject of one of the best biographies that ever was written, that of Baillet, which has totally eclipsed an earlier one in Latin by Pierre Borel, containing some valuable information not in Baillet. The frontispiece of Baillet's book, by the way, is engraved from the admirable portrait, familiar to everybody at first or second hand, by Franz Hals, in the Louvre. That engraving, though not remarkable otherwise, is interesting as showing details which to day can hardly be made out in the original without something to call attention to them. As for Descartes's writings, it would not matter much now if they were all destroyed. He himself called his philosophy an application of his geometry; but rather the achievement lay in showing how mathematical reasoning was to be applied to philosophy. It was an idea so simple, so statuesque, that we all get the full benefit of it though we never look into the original expression of it. Every educated man reads the 'Discours de la Méthode'--most educated men the 'Meditations'-at least, it was so when a college life was a literary, and not an athletic, life. But few philosophers are less pored over. A metaphysician will devote more time to the histories of Cartesianism by Bordas-Demoulin and Bouillier than to the simple words of its author.

Of course, whoever wishes to understand the history of science will pay a more minute attention to the details of the writings of Descartes, and also to his correspondence. And it may be that diligent research will yet bring to light letters by him and to him not altogether unimportant for the study of the intellectual status of his times. But not very much can be expected, even in that direction--certainly nothing comparable for an instant with the mine of historical information with which the new edition of Huygens is enriching the world. After all, it is no great hardship to have to read Descartes in the Elzevirs, and in the various volumes of *Opera posthuma* and *inedita* which have been dribbling down to us during the past two centuries and a half.

Descartes's three dreams, which mark the beginning of his philosophical career, were dreamed November 10, 1619, and he died in February, 1650, during a visit to Queen Christina in Stockholm. He had carried with him to Sweden a chest containing those of his papers which he considered interesting, and while there had busied himself, at the Queen's command, with arranging them. After his death the papers in the chest were inventoried, and so were, separately, those which he had left behind in Holland. The inventory of the chest, given in brief form by Borel, was found not many years since in its original bad French among the papers of Christian Huygens. The other inventory we know nothing about, but it could have contained nothing valuable except letters; and the best of his letters were in the chest in Sweden. M. Adam clearly proves that there are only a few pages in that chest whose loss is to be regretted. The head of the house of Descartes, the elder brother of the great philosopher, looked upon him as the Squire du Perron, who was demeaning himself in an eccentric and scarcely dignified way in busying himself with science; and he readily made a present of the chest and its contents to the French ambassador in Sweden, M. Chanut. Chanut in turn gave it to his brother-in-law Clerselier, who published two volumes of posthumous writings. He intended to bring out a third, but died in 1684 without having done so, leaving the papers as a legacy to J. B. Legrand, together with 500 livres for whoever would undertake their publication. Legrand, in 1690, writes to a correspondent: "Je vous diray pour votre consolation, Monsieur, que tous les manuscrits de M. Descartes qui n'ont point été imprimez sont en ma possession, outre 120 lettres que j'ai recueillies de diverses personnes, sans parler des mémoires qui me sont venus de la part de sa famille." There was a niece who seems to have conceived the idea that uncle Rene was rather a man to be proud of than otherwise; the more so as she looked upon his fame as an appanage of the family, a sort of addition to the coat of arms. The Abbé Legrand allowed Baillet the use of all the manuscripts. M. Adam suspects, on slight grounds, that Baillet's celebrated biography may be "at least in part" (which seems to us particularly weak, for it has certainly not been worked over unless possibly to render it more catholic) the work of Legrand. Legrand, dying in 1704, left the papers and the 500 livres to a M. Marmion, who died the following year, bequeathing the papers and the 500 livres to the mother of the Abbé Legrand. Nothing more is known of their history.

One thing there was in that chest that human curiosity cannot but desire to see. Incidentally we may mention that the chest on its way to Paris was sunk in a river-boat, was recovered, and all the papers as well dried as might be, and that from them were printed some of the invaluable correspondence which is contained in the edition of Cousin (to mention the least rare book which contains the bulk of it). But the curiosity we mean is a blank book which Decartes began "Anno 1619, kalendis Januariis." It began with a mathematical essay of eighteen leaves, entitled "Parnassus." The conditions favorable to mathematical genius--the stuff of which it is made, was just at that instant in its most plastic state. One would be glad to see what it was that the man who, in a few months, was about to invent analytical geometry, then thought worthy of that ambitious title. Then "après six feuillets vides est un escrit qui contient autres six feuillets, en prenant le liure d'un autre sens, les discours intitulés: *Olympica*." Among those pieces was the narrative of the three dreams, not so very extraordinary in themselves, but producing an extraordinary impression upon Descartes.

The inventory says that on the margin was written "11 Novembre cepi intelligere fundamentum inuentimirabilis." But, according to Baillet, who had the book before him, there was a date, 10 Nov. 1619, in the text, and 11 Nov. 1620, in the margin. This affords an excellent touchstone for the precious simplicity of commentators. The writing, if we had it, would disclose how the marvellous discovery appeared to its author at its first conception, and before he began to think how to put it into a shape acceptable to the general public. Then, "reprenant le liure en droit sens," two leaves of "quelques considerations sur les sciences." Next, half a page of algebra. Then a few lines of *Democritica*. Then, again turning the book, five and one half leaves of *Experimenta*. Finally, four pages entitled, "Præambula, initium sapientiæ est timor Domini." What a study for the psychologist would be there!

1895

60 (14 March 1895) 208: Logic.

By Dr. Christoph Sigwart. Second edition, revised and enlarged. Translated by Helen Dendy. 2 vols. London: Swan Sonnenschein & Co.; New York: Macmillan & Co. 1895. 8vo.

CSP, identification: MS L 159.50. See also: Fisch, *First Supplement*.

The second edition of Sigwart's Logic was substantially a new book; at least, it seemed quite fresh at its appearance a few years ago. The opinions expressed in it are recognized as among the sanest and best-considered. But whether it is a particularly suitable book for translation is not quite so clear. It is to be remembered that no very extensive or important part of logic stands accepted without dispute on all hands. Nor are the differences confined to points of theory and arrangement; on the contrary, they are practically decisive as to what inference is to be commended, what rejected, as well as for methods of inquiry. Beginners in the study of logic certainly ought to be apprised of the more important divergences. Most logicians when they approach a disputed doctrine exhibit manifest symptoms of it. Even if they name nobody, the breath comes quick, and the glare of the eyes is reflected in the type. But Sigwart goes on his way with such calm serenity, giving his own lucid and, to an advanced scholar, luminous views, that the young student would never dream that anybody else ever thought otherwise.

Since the author quite omits, or treats with strictest compression, all those parts of logic about which there is no dispute, one might wonder how two goodly volumes of a thousand pages of four hundred words to the page have been filled up. They contain chiefly the *ex-parte* presentation of Sigwart-ish views, very well worth reading certainly. This is enlarged by a great deal of acute criticism on points of little importance, many of them not truly relevant to logic. Many topics, although subjects of divergent opinions, are passed by without mention.

Historical statements are sparingly made; and of the few, there is a considerable proportion that cannot support criticism. Thus, Sigwart (vol. ii, p. 308, footnote) attributes to Jevons the remark that induction is an inverse operation (mistranslated a "reverse operation"), related to deduction as division is related to multiplication. But this had been said long before Jevons's day. Indeed, the moment deduction is regarded as a mathematical operation, it follows from Aristotle's conception of induction that this is the inverse operation. The assimilation was made by Leibnitz, and was expanded into two eloquent volumes by Pere Gratry. Indeed, the latter writer apprehends it more accurately than either Jevons or Sigwart, for he shows that mathematical differentiation, depending on the inverse operation of subtraction, while integration depends on the direct operation of summation, is in that sense an inverse operation, and he defends the older opinion that differentiation is the instrument of inductive reasoning, integration that of deductive reasoning. Thus the laws of mechanics are expressed by differential equations, and the applications of them are performed by integrating those equations.

Sigwart treats logic chiefly from the point of view of methodology. From that standpoint probability and probable inference appear as its most prominent topics. These are subjects upon which we cannot expect a German treatise to attain the English standard of accuracy and subtlety of thought, for two generations of the most powerful minds the English-speaking countries have produced have had them constantly under scrutiny, while in Germany they have been little studied. Sigwart's account of the philosophy of probability is inferior even to that of Locke. Take this sentence: "F. A. Lange rightly saw that the theory of probability is based upon the disjunctive judgment, and is in this way connected with logic." This is hardly doing justice to that able thinker. But, passing that by, very long before Lange every English logician had seen, what must appear early in one's reflections upon the subject, that the theory of probability is not only "connected with," but is a part of, logic--one might almost say is the major part of it. The pedantry of supposing that "the disjunctive judgment" can shed a vivifying light upon the philosophy of probability is deplorable, while it provokes a smile.

Yet upon subjects closely connected with this Sigwart is most instructive. We will select by way of illustration a point which shows at the same time how near the abstract theory of logic may come to matters of practical concern. Our readers will understand that we are expressing no opinions whatever concerning the subject matter; we intend only to point out how divergent theory may lead to divergent practice. Sigwart remarks that when we investigate anything whatever, we go upon the presumption that the truth about the matter in hand is capable of being expressed in general terms, and further that the proposition that events happen according to laws and are determined by causes is a special case of such presumption. No proof, he says, of the impossibility of a lawless happening has ever been brought forward, and thus he is led to hold that such postulates are "not so much laws which the understanding prescribes to Nature" (which was Kant's opinion), "as laws which the understanding lays down for its own regulation in its investigation and consideration of Nature." Correct or not, this is a pregnant suggestion. In modern whist there are certain presumptions, especially towards the end of a hand, which we make, not because we possess any evidence of their truth, but because if such a proposition be true, there is a way of winning a trick that, under the supposition of its falsity, there is no way of winning. This is the sort of presumptions which German logicians call "regulative principles." Now it is plain that such a regulative principle cannot, *as such*, be universal, because it applies in that capacity only to the single case in hand. If, then, Sigwart be right in holding the law of causation, the principle of the uniformity of nature and the like to be "regulative principles," all ground for asserting them to be absolutely without exception is removed. Now that would profoundly modify not only our theory of the universe, but also our philosophy of life and death. All this would or might come from a particular answer to an abstract, obscure, and apparently insignificant question of pure logic. When we turn over the pages of such a book as that before us, we may be tempted to wonder of what use or application is all that. But it is always possible there is a "live coal behind the thought" that long blown on may some day flame. The instance adduced shows the possibility of this.

The translator writes an easy un-German English, and the accuracy of the rendering is vouched for by the author, who has read all the proofs. Nevertheless mistakes are to be found in it.

60 (21 March 1895) 226-227: **Philosophy of Mind: An Essay in the Metaphysics of Psychology**.

By George Trumbull Ladd, Professor of Philosophy in Yale University. New York: Charles Scribner's Sons. 1895. Pp. xiv, 414, 8vo.

Although the evidence here is completely internal, this is surely by Peirce. Peirce had started a review of Ladd's *Psychology* in 1894 (see MS 1386), and the term "Phenomenism" was one of the entries Peirce wrote for the *Century Dictionary*. This piece is unassigned in Haskell's *Index to* The Nation, vol. 1.

George Trumbull Ladd (1842-1921) not only wrote on topics of psychology, but achieved great fame as an author in the field of theology. His *Doctrine of Sacred Scriptures* (2 vols., 1883) has been called one of the six great theological works of the nineteenth century. Ladd's early education was at Western Reserve College (1864) and Andover Theological Seminary (1869). He accepted the chair of philosophy at Bowdoin College and subsequently became professor of metaphysics and moral philosophy at Yale. From 1895 until 1896, Ladd taught a graduate seminar in ethics at Harvard University. He was one of the founders of the American Psychological Association, which he served as president from 1893 until 1894.

There were but few, we venture to think, who read Prof. Ladd's 'Psychology, Descriptive and Explanatory,' issued last year, that did not feel the presence of an obscure background of opinion which in part, at least, was determining the author's course at every step, but found no adequate expression in his argument; and the like is true of the 'Philosophy of Mind.' Neither book contains its own final premises. Those of the 'Psychology' are now first published in the 'Philosophy of Mind,' and those of the 'Philosophy of Mind' lie hidden, as the author intimates (page 81), in "certain points of view, and even certain conclusions" which he has not yet made accessible to the public. The result may be imagined. The series of which this volume forms an instalment is composed on the plan of a Ciceronian period, and purposes to move in all the pomp of suspended intelligibility till the last word. The "secret of Ladd" retires before one like the boxes in a Chinese puzzle, and every volume hands on its mystery to the next. This is, of course, a matter of exposition, simply; and Prof. Ladd unquestionably has a right to keep his secret through as many volumes as he may choose; but he presents meantime too often the ungraceful attitude of sweeping in the stakes without showing his cards.

With this drawback, or rather in spite of it, the 'Philosophy of Mind' is a full, rich book, with that amplitude of by-remark which fattens the writings of middle aged men. It opens with two chapters on the relation of Psychology to Philosophy. Their general purport is, that in spite of the failure of its professors consistently to treat it so, Psychology is properly a natural science like any other, and proceeds upon assumptions which it is not its own business, but the business of metaphysic as the general science of final assumptions, to criticise. There is much in them to remark, but as they stand in no vital connection with what follows, we pass at once to the rest of the book.

Prof. Ladd is interesting among psychologists of note at the present day in his holding by the soul, and the 'Philosophy of Mind' is in the main devoted to his declarations on that head. The reader will find them very new wine in an old bottle. The soul, according to this new dispensation, is terribly up to date; it differs from the soul of tradition in pretty much everything but the name. It is not an entity over and above consciousness. It is not an unchanging core of reality--a perdurable substratum of the conscious states. It is not simple, it is not indivisible, it is not in its essence immortal (the "possibility" or "perhaps the probability" of immortality is at the utmost but "suggested"), it is not even continuously existent in the past. In every deep dreamless sleep, in every case of fainting or other loss of consciousness, the soul ceases utterly to exist. The self we are conscious of--the "agent" we observe in introspection actively presiding over consciousness--is the only human soul there is. It begins with the appearance of consciousness in the body; it dies, so far as the weight of the evidence goes, with the death of the body; its existence is broken into blocks by every gap in consciousness, and between these blocks there exists no unity but a felt unity--the later block remembers the earlier and looks upon itself as a continuance of it. For the trustworthiness of memory, however, we have but the guarantee of a tender bit of dialectic which will not, we may suspect, outlast the strain of close inspection. It is as follows: We can't doubt the memory, for unless we remember correctly the premises of our argument to disprove it till we reach the conclusion, the conclusion does not follow. But we apprehend that premises and conclusion are never in different moments of consciousness; we don't conclude that Socrates is mortal because we remember that we *formerly* believed that all men are mortal and that Socrates is a man; we conclude it only on the ground that we believe this *now*. If premises and conclusion are not in the same moment of consciousness, there is no conclusion.

One wonders, in these days of Hegelian formulæ, how the pews in most of the churches would "sit up" if they ever got a notion what the pulpit is saying. Prof. Ladd's doctrine of self is very good doctrine, and the ten chapters of which it forms the backbone are in many places models of sustained argument and masterly exposition, but one wonders why he calls it *soul*. His statements differ not at all from those of the least hirsute (*baldest* is his word) phenomenism; unless it be in the term "agent." But an agent who is not an entity over and above consciousness is an ego that Hume himself might have accommodated. Phenomenism, however, Prof. Ladd regards with unconcealed aversion, sometimes on the ground that it is "shallow," sometimes on the ground that is is "bald," sometimes that it is self-contradictory and absurd. It would seem a pity, if this last be true, that he should have argued himself into it. Happily his reasons for regarding it in that disparaging light may not prove inexpugnable--so far, at least, as he has yet suggested them. (There may no doubt be better ones in that dark background of assumption to which allusion has been made.) We can examine but one of them before we close, but it may be taken as a sample of the rest. It may be stated as follows: That one knows something, one cannot doubt--one assumes it even in the argument to disprove it. But knowledge of phenomena is not knowledge; knowledge by its very terms imports reality. Therefore, the fundamental doctrine of phenomenism, that we know nothing

but phenomena (of consciousness), amounts to this, that we know that we know nothing.

We apologize for the harshness of the terms (it would be a mistake to speak of Prof. Ladd in semi-tones--he does not deal in them himself), but this bit of reasoning reminds one of a celebrated syllogism mentioned by Moliere, in which the major was inept, the minor ridiculous, and the conclusion impertinent. For if (as Prof. Ladd grants for the purposes of argument) one can, on the assumption that one knows something, have a proof that one knows nothing, that assumption, like any other which issues in a contradiction, is sufficiently discredited. That a knowledge of phenomena is not knowledge sufficiently discredits itself. And that the conclusion is not pertinent will be plain to anybody who reflects on the bare meaning of the proposition that one knows *nothing* but phenomena. It means above all that the one thing we cannot know about consciousness is, that it is a phenomenon of something else--that consciousness is the only reality.

These, however, are but blemishes in a book which is strong enough to carry even worse ones. We have touched on its central and most interesting topic, but it treats incidentally and often at some length of many things besides. "The self, . . . not simply as known to itself, but also as scientifically known in its relations to the bodily organism," is the author's own statement of his subject, and under the latter head he discusses monistic materialism, monistic spiritualism (that monism of the "unknown" and "absurd" which regards both matter and mind as common manifestations of a single *tertium quid*), the law of causation, the conservation of energy, etc., etc. The three monisms mentioned he rejects for the time being in favor of something very like a plain man's dualism, which he assumes provisionally until the publication of the "certain points of view" and "even certain conclusions"--the undivulged premises of the 'Philosophy of Mind.' They will amount in their entirety, it seems, to an Idealistic Monism, not otherwise defined than that it issues in a personal Absolute, the One Ground of all interrelated existences and activities. The positions of the present book the reader is left to adjust to this as best he may.

60 (4 April 1895) 265: **Nicolai Ivanovich Lobachevsky**.

By A. Vasiliev. Translated from the Russian by George Bruce Halsted. Austin, Tex.: The Neomon, 2407 Guadalupe Street. 1894.

Attributed to Peirce by Fisch in *Second Supplement*. Peirce had reviewed similar books produced by Halsted, and had been in correspondence with Halsted about these matters. This review is unassigned in Haskell's *Index to* The Nation, vol. 1.

A good many interesting particulars about the non-Euclidean geometry and its author can by searching be picked out of this ill-arranged pamphlet. Lobatchevsky was born in 1793 in the town of Makarieff, some forty miles above Nizhni-Novgorod, on the Volga. His father was a peasant (Ch. Rumelin says an architect, forgetting, apparently, that a *Bauer* is not usually a *Baumeister*). He was entered at the newly founded University of Kazan in 1807. He must have impressed his parents with his genius. Prof. Vasilieff tells us he was a passionate boy. His behavior was always reputed bad. Later there was some terrible storm of passion which left him for the rest of his life taciturn and stern. He took the degree of master in 1811. After graduation he entered the Observatory and studied practical astronomy under J. J. Littrow (whose name Dr. Halsted spells Lettrov). In 1814 he was made adjunct professor of mathematics, and in 1816 full professor. But the University of Kazan was hardly a regular university, and he had to teach astronomy also; and for some time physics and chemistry besides. "One of these lectures," the only one on chemistry, Prof. Vasilieff tells us, "was accompanied by experiments." In 1825 Lobatchevsky added to his duties those of librarian to the University, and so continued for ten years. It was about this time that one Magnetsky was rector of the University, and the dire administration of this religious fanatic and barbarous foe of science is evidently a familiar tradition of horror in Kazan to this day. In 1827 Lobatchevsky himself was elected to the rectorship, which he held for nineteen years. In 1846 he retired from the University, and went to live in a village which belonged to him, Belovoliskaya Slobodka, on the Volga, near the mouth of the Kama River. He was now appointed curator, or assistant curator, of the district of Kazan. He interested himself vastly in agriculture, becoming the president of his branch of the Imperial Economical Society, and taking the medal of the Imperial Agricultural Society for his improvements in the treatment of wools. Finally, he became blind, and in 1856 died.

Kazan was not the *milieu* for a man of genius, especially not for so profound a genius as that of Lobatchevsky, and he did little mathematical work beyond writing text-books. What little he did publish was received with derision and contempt. Who in Russia in 1834 could possibly see any sense in the contention of Lobatchevsky that it was one thing for a curved line to be continuous, and quite another for it to have definite tangents?

The mathematicians of Western Europe did not become aware of the distinction until nearly 1880, when Weierstrass suggested that a line might be wavy, and these waves carry smaller waves, and those still smaller waves, and so on *ad infinitum*. Down to this day there is but one text-book on the differential calculus (that of Camille Jordan, in its second edition) which introduces the distinction. All of Lobatchevsky's writings are marked by the same high strung logic, and there is nothing a semi-civilized people respects less than extremely accurate thought.

Lobatchevsky made some experimental researches in terrestrial physics. He was also one of the first carefully to observe and call attention to the solar corona. It is a marvelous instance of man's stupendous power of shutting his eyes to plain facts that this phenomenon, one of the most startling, not to say thrilling, in nature, was not enough noticed to receive a name until 1851. But it was observed by Lobatchevsky at the eclipse of 1842, July 8,[†*] and described with care. He also gave a

theory of it which, as reported by Vasilieff and translated by Dr. Halsted, seems pretty puerile.

According to the evidence adduced by Prof. Vasilieff, it appears that both the reputed authors of the non-Euclidean geometry, Lobatchevsky and Bolyai, probably derived their first knowledge of it indirectly from Gauss. A letter is extant, written in 1799 by Gauss to Bolyai's father, which contains a very plain hint of the thing. And Vasilieff now informs us that Lobatchevsky's teacher of mathematics was J. M. C. Bartels, who had been the teacher and devoted friend of Gauss, from 1785 to 1807, when Bartels went to Kazan. It is next to impossible that, coming then into very intimate relations with Lobatchevsky, he should not have mentioned Gauss's studies in the non-Euclidean geometry. However, Gauss was not the first discoverer. Lambert in 1785, in a printed book, spoke plainly of a space where the angles of a triangle should sum up to less than 180 degrees, and mentions one of its most remarkable properties. Gauss most likely knew of this. Nor was Lambert first in the field, for the Jesuit Saccheri had discovered the thing before 1734.

60 (11 April 1895) 284-285: Comte, Mill, and Spencer: An Outline of Philosophy.

By John Watson, LL.D. Macmillan & Co. 1895. Pp. 302.

CSP, identification: Haskell, *Index to* The Nation. See also: Burks, *Bibliography*; *List of Articles*.

This is a sketch of philosophy by a well-known idealist of the school of Green and Caird, based upon a criticism of the most famous of the experientialists of our century. What is the standing to-day, among those who devote themselves to philosophy, of Comte, Mill, and Spencer? To judge the pertinence and strength of this book, we must recall to mind what their reputation is, not among men who read half-a-dozen works of philosophy and then stop, but among those who are qualified to judge of them. Spencer's books are supposed to contain an immense number of acute and valuable ideas, belonging to the marches between positive science and a more comprehensive theory; but as for his philosophy as stated by himself in sixteen propositions, it is valued by very few, if by any, although there are two or three of its statements which are pretty widely accepted. Spencer's 'First Principles,' though it contains one idea, at least, which has been generally taken up, is as a whole the work of a man wholly ignorant of modern philosophy, and therefore entirely incompetent to address modern students. Spencer, as a philospher [[sic]], is much more respected by those who write against him than he is by the bulk of students, although their general attitude of thought is nearer to his. Mill's 'Logic,' a work going on to two generations of age--generations the most active in the direction of logic mother earth has looked upon--was written by a man without any reading in philosophy outside the nominalist school. It was animated by the purpose of overthrowing the ideas of Whewell, which are the ideas that, before Whewell and since, have prevailed among those physicists who have endeavored to take a philosophical

view of the history of physics, and who have had no extreme bias towards any particular school of philosophy. Moreover, it is now generally recognized that Mill's exposition suffered from his struggles to throw off the narrow bonds of the school in which he had lived. The result was to make him wavering and inconsistent to a degree that the reader would not at first deem possible. Jevons had begun to show this, in a series of papers now reckoned important by scholars, when he was cut off, to the world's loss. Comte, in the judgment of modern students, was a man whose spool of intellectual piano-forte wire was vastly greater. Of Mill there is nothing to ask except whether his doctrine is correct or not; but Comte certainly made mighty contributions to the world's stock of ideas, over and above what he got from St. Simon, whether they squarely drive the truth into the hard skull of Humanity or not.

Such, as well as we can make it out, is (not *our* opinion, but) the centre of gravity of the judgments of real students to-day. As compared with its place thirty years ago, it has moved much away from Mill, much toward Comte.

But not in a direct line. No doubt, opinion has swerved toward realism (in the scholastic sense). For example, thirty years ago thinkers were generally very averse to innate ideas. But Spencer (July, 1865) used the phrase "hereditary ideas," and men began to think it was a question of evidence. Experiments were made; and the upshot is there is not a psychologist to-day who does not hold the doctrine of innate ideas, whether he *calls* them so or not, substantially as it was held by Descartes and Leibnitz. Now the doctrine of innate ideas is in general harmony with realism as opposed to nominalism. Pure mathematics, too, burst out into extraordinary activity; and pure mathematics is a natural friend of idealism. Then an exacter analysis of the logical question showed that realism was the doctrine of modern science. The advanced school of Formal Logic found in their algebra potent reasons for a conception of logic having a general similarity with that of Hegel's objective logic. The idea of evolution naturally inclined men the same way. Accordingly, when Mr. Watson proposes to found an idealistic philosophy on the study of the three great empiricists of whom our time talks, he is taking advantage of a powerful current of thought.

He begins with mathematical reasoning and Mill's views about it. During the last two generations the fundamental mathematical conceptions have been the object of most exact and profound study by the subtlest and exactest mathematicians who ever illuminated the world; and those studies have resulted in a substantial settlement of opinion among mathematicians themselves concerning most of the questions involved. After this history, it is interesting to find that Mill's main contentions are now regarded by mathematicians as attempts to formulate what is definitively acknowledged to be true. It is not surprising that Prof. Watson should be able to show very clearly that Mill is in error; and up to a certain point he goes on with great success. But it soon becomes apparent that he knows nothing of modern mathematics, and has not sufficiently (if at all) studied the analyses by modern mathematicians of their own thought. He is not aware how much those works do toward reconciling idealistic proclivities with the spirit of positive science. After the usual style of Kantians, he allows the question to be confused by not separating geometry as a branch of physics from geometry as a branch of pure mathematics. He calls no attention to the remarkable circumstance that, after mathematicians have deliberately severed their moorings to the real world--as in the Theory of Functions, for example, which is all about {sq rt}-1--instead of their bark being tossed upon a wild sea of arbitrary hypotheses, as one might expect, they reach a certain destination, different investigators upon widely different lines being led to the same conceptions and to concordant results, quite as surely as different chemists experimenting upon the same substance. Such facts afford comfort to idealists; and it will generally be found that modern pure mathematicians are as idealistic about ideal geometry as they are decidedly empiricist about physical geometry.

Nor does Prof. Watson expose, as he should, the falsity of the common idea that mathematics advances by means of demonstrations. No doubt demonstrations are more important in mathematics, than in most sciences; but, for all that, mathematics advances, just as the physical sciences do, by observation and generalization. Its observations are, it is true, only observations of the mind's own constructions, but they often have that *startling* quality which indicates that they *are* observations. The generalizations are so complex in construction as not at once to be recognized as generalizations. When mathematics is written in the style of Euclid, both the observations and the generalizations are hidden in the crush of demonstrations from student and writer alike. But modern mathematicians recognize the operations of attentive observation and generalization as their chief engines, and give them due prominence. This at once favors the moderately positivist conviction that science is the fruit of observation and generalization, and at the same time the idealistic belief that the world of ideas is a real world, that can be observed and reasoned about, and not a world of whimsies. It is really high time that those who write about the philosophy of mathematics should have a familiar acquaintance with modern mathematics, and should have attended to the reflections of modern mathematicians concerning their own science.

Prof. Watson's view of causation will seem to Anglo-Saxon minds a little wanting in meaning. He denies that there is anything "occult" about causation, but in the same breath attributes it to an *identity* which he does not make clear. Thus, two bodies at a given distance have a given gravitational acceleration towards one another. If we ask why, and are told there is an occult unity in nature producing the uniformity, we can understand that, expecially [[sic]] if the occultness is only relative. But when we are told that it is because of an identity not occult, we cannot say that that is perspicuous.

In three successive chapters on Biology, its relations to Philosophy, and the Philsophy [[sic]] of Mind, the question of whether everything is to be levelled down to matter or levelled up to mind is discussed with great clearness and no little force. The idealistic position is argued against the school of Spencer and Huxley in a manner deserving attention. The strong hold of idealism to-day lies in reflections upon the question of how consciousness came into the world. Students of to-day form their opinions on a basis of observed fact. Their quarrel with the old "experientialists" is that their own doctrines were not so formed, but were the mere expression of their intellectual inclinations. If observed facts lead to belief in ghosts, in ghosts

they will believe. If they lead to the belief in the active reality of ideas, in that they will believe. But in accepting idealism, the older generation of idealists must not flatter themselves that scientific men--and modern philosophers have upon their hands the stains of the laboratory--are going to embrace the notions, the sentiments, and the mode of being which, heretofore accidentally associated with idealism, have their home in the theological seminary.

60 (30 May 1895) 431-432: **Herbart and the Herbartians**.

By Charles DeGarmo. Scribners. 1895.

Attributed to Peirce by Fisch in *Second Supplement*. Peirce owned a copy of this book. Peirce was an admirer of Herbart, but not of the Herbartians. This item is unassigned in Haskell's *Index to* The Nation, vol. 1.

Charles DeGarmo (1849-1934) was a leading exponent of the Herbartian theory of education. He received his Ph.D. from the University of Halle, Germany, in 1886, and, in that year, was appointed to the chair of modern languages at Illinois State Normal School. In 1890, DeGarmo became chairman of philosophy at the State University of Illinois, leaving that school in 1891 to become the third president of Swarthmore College.

This volume is entirely occupied with the relations of Herbart and his followers to teaching, and furnishes a good deal of welcome information about Ziller, Stoy, Rein, Karl Lange, Frick, and others, their work and their doctrines. The philosophical part is excessively general and sketchy, and though a hundred pages are given to Herbart, there is no exposition of his system, nor anything on which any beginning of an opinion of the merits of his theories could be built. The Herbartians in this country have founded a club, which seems to be designed for a propaganda, not to say a party. There are those who think this one of the many indications of over-positiveness in the Herbartians. Herbart himself was very absolute in his opinions; but there was this half-excuse for him, that he conceived them to be rigorously demonstrated. The best of his followers, being quite alive to the strides that have been made in logic since Herbart's time, no longer insist upon the metaphysical proofs he-offered, and it would therefore become them to temper the boldness of their master's conclusions.

The *apperception* which Herbartians always have on their tongues is nothing in the world but another development of the idea familiar to us of English culture under the name of the association of ideas; and many of the Herbartian applications of apperception to teaching might have been borrowed from Hartley. Hartley's 'Observations on Man' was published in 1749, while Herbart's writings were all in our century. It would have been a shame if the later development of the idea had shown no improvement on the earlier. That the theory of teaching should be based upon the law of association of ideas, in a generalized form, and upon the law of fatigue, is evident, if those are recognized as the sole laws of mental action. (Do not the heavy tomes of some of the modern Herbartians sometimes leave the second law out of sight?) To that extent every student of pedagogy must be a Herbartian. But when they assume that, in order to make

"apperception" the sole law of mind, it is necessary to sweep away all entitative faculties, they fail to carry with them those teachers who believe in the guidance of ordinary good sense. One might as well say that the law of mechanics, being the autocrat of the physical universe, requires the denial of all special forces. Human faculties may be supposed to have originated through the action of association, but there is not the slightest necessity of denying that human powers are, in fact, specialized.

Another idea upon which the propaganda of the Herbartians is designed to wage war is the gymnastic conception of education. Dr. DeGarmo sneers at "our favorite doctrine of mental discipline." "This conception assumes, first, that the mind can be well trained with a minimum of concrete knowledge; and, second, that the power gained in one department of knowledge may be transferred to any other." This is certainly the favorite doctrine of our teachers; is it not also a good description of the principles upon which modern physical science has been built up? Namely, physicists have made mathematics the basis of their work, which is "training their minds with the minimum of concrete knowledge." They have learned their mathematics by working fictitious examples, because they hold that the power so gained may be transferred to any other problem. In fact, the whole progress of physics is due to applying to one department the training gained in studying another. It will be, we guess, vain to try to persuade our teachers that this principle is not a sound one in the main. There is no part of instruction in which the Herbartians are weaker than exact science. Dr. Frick's course for a gymnasium, taking the boy at ten years of age and dismissing him at nineteen, includes no mathematics whatever, and teaches physics, as well as the brief account in this volume enables us to judge, in the absurdest manner. The Herbartian books upon geometry betray a total ignorance of the subject, as it appears to a modern mathematician. The truth is, the only way the exact sciences have ever been successfully taught is by the despised method of "mental discipline."

Abandoning, as the Herbartians do, the theory of mental discipline, it is no wonder they have been greatly puzzled how they should unify instruction. For what they call the "core" of instruction for children, Ziller and others have adopted fairy tales, a cruelly mutilated Robinson Crusoe, and so forth. This seems a poor substitute for our "Rollo books" and their like, which are based on the principle of training the powers.

The Herbartians carry to great lengths the embryological theory of learning, according to which the individual student of a science must reproduce in his own course of thinking all the mental operations which in history have brought that science to its present state. This is more true of some subjects than of others. When Dr. DeGarmo tells us that "if a man would advance in art, he must master its past to start with," and mentions improvements in the dynamo as an instance of what he means, he has experience all against him. American inventors should, in that case, be singularly unsuccessful, for their method is to make themselves thoroughly acquainted with the present state of the art, so as to know exactly where the shoe pinches, and to collect from various quarters (generally by applying

training gained in one department to another) suggestions which may be applicable to improvements. As in other cases, the Herbartian notion has much merit, but it is pushed too far, regardless of the protests of good sense and of experience.

61 (4 July 1895) 14-16: SOME STUDIES OF REASONING

A Critical Account of the Philosophy of Lotze: The Doctrine of Thought.

By Henry Jones, author of 'Browning as a Philosophical Teacher.' Macmillan & Co. 1895.

Die Grundbegriffe der ebenen Geometrie.

Von Dr. V. Eberhard. Erster Band. Leipzig: Teubner. 1895.

Riemann and his Significance for the Development of Modern Mathematics.

By Felix Klein [Translated by Alexander Ziwet. In the Bulletin of the American Mathematical Society for April, 1895.]

Elements of Inductive Logic.

By Noah K. Davis. Harper & Brothers. 1895.

CSP, identification: Haskell, *Index to* The Nation. See also: Burks, *Bibliography*; *List of Articles*.

Sir Henry Jones (1852-1922) became master of Ironworks School, Brynamman, in 1873, and he received his M.A. from Glasgow University in 1878. Jones was influenced toward the philosophy of Edward Caird. He taught philosophy and political economy at the University College of North Wales (1884); logic, rhetoric, and metaphysics at St. Andrews (1891); and moral philosophy at Glasgow (1894-1922). Jones was knighted in 1912.

Noah Knowles Davis (1830-1910) was an innovator in American education. In 1868, he was elected president of Bethel College, where he established the first department of English in the United States. In 1873, Davis assumed the chair of moral philosophy at the University of Virginia. Davis wrote extensively on philosophy and logic.

We may dispose of the last book first. It has the signal merit of being small--a double merit considering its quality. It plunges the "tyro," as the student is politely called, at the very outset, into perplexing disputes of which he will be utterly incompetent to form the least opinion, thus tending to pervert him into not the least nocuous of mental vices. It jumbles wholly irreconcilable theories. Its power of thought may be judged from a single specimen. The proposition "A scotched wheel does not revolve" is pronounced unscientific and inaccurate, because "in strictness a cause is essentially positive." It is a pity that those who have for the last three hundred years been trying to improve the condition of the world by the study of forces were not told this in time; for they have always been regarding forces as acting continually, whether balanced by others or not, and have always treated friction, which is (we are forbidden to say the cause) the devil of the scotched wheel, as a force. It would be a grievous thing that young men should be taught such triviality instead of the methods by which useful reasoning is really performed, were it not that fortunately it runs off of them like water from a duck's back.

One of the leading mathematicians and mathematical philosophers of our age, Klein, gives another of those instructive comments upon mathematical procedure of which we have enjoyed a number from his pen. He again dwells, as he had already done, upon the importance of attentive intuition--in other words, of the *observation* of diagrams and the like--as an essential element of mathematical reasoning. Considering that mathematicians have long held that mathematics covers all exact reasoning, quantitative or not, it will be seen that Klein is going over to a logical doctrine which has had defenders in this country and in England. According to this, our assurance that (2.4.6.8.10) &unk; (1.2.3.4.5) = 2^5 is of the same nature as our assurance that sulphuric acid is precipitated by baryta, we having satisfied ourselves in each case that a single experiment is sufficient; only in one case we observe Nature, in the other our own construction. Dr. Klein remarks that although pure mathematics deals with a purely imaginary world, yet the course of its development is not arbitrary. He seeks to explain its orderly growth by historical continuity, and by the fact that as older questions get solved new questions "naturally" arise. No doubt they do, but that is precisely what needs explanation. We should like to know in what this "natural" succession of ideas, without any external *nature* to guide it, consists. Is there, for example, some Hegelian dialectic, or is there a different general law for this growth of pure concepts, or is it lawless?

The paper contains so many points of interest that we can only recommend it to the reader. The logical importance of Riemann's memoir on Trigonometric Series is pointed out. Klein says (in the absence of the original, we have taken the liberty of rubbing down some of the angles of the English translation):

"Riemann's collected works are neither numerous nor extensive. They are comprised in an octavo of 550 pages, and but half of that matter was published by him. Yet his sway over the minds of mathematicians was the most potent of his time; nor is it even yet come to an end. This is owing to the *originality* and the *penetration* of his mathematical thought.... Passing by the latter character, I desire to point out that Riemann's originality lies in a unifying central idea, the source of all his achievements.... He devoted much time and thought to physical theories.... These are preserved in fragmentary form in his posthumous papers. All have in common the hypothesis (since made prominent by confirmations of Maxwell's theory of light) that space is filled with a continuous fluid serving as a common medium for the propagation of optical, magnetic, and gravitational phenomena.... Those physical ideas are the mainsprings of his investigations in *pure mathematics*.... The mathematical work of Riemann is the counterpart of the physical researches of Faraday."

He remarks, too, that Riemann's growing influence is illustrated by two new French books, Picard's 'Traité d'Analyse' and Appell and Goursat's 'Théorie des Fonctions Algébriques.'

Dr. Eberhard's work is a welcome mathematical treatise on plane point-systems. We mention it because some thirty pages of the long preface are occupied with the basis and purpose of geometry. As in some other recent German work in the same direction, while the need is for a purely logical analysis, we are furnished with semi-psychological and epistemological reflections which would not answer the purpose even if they were beyond criticism. It illustrates, however, the purely empirical ground upon which geometry is now placed by mathematicians, considered as a science of actual space, although the very same men may be high idealists when they come to geometry of *n* dimensions or other pure mathematics. From generalities which might almost have been taken from Bain's 'Senses and Intellect,' or some such psychological treatise, Eberhard passes at once to the intersections of planes and straight lines, in entire forgetfulness that in order to exhibit the foundations of geometry he ought first to have treated topology, or that branch of geometry which relates to lines and surfaces whose exact forms remain undefined--a branch which includes the theory of knots, Euclid's formula for the number of summits, edges, and faces of a polyhedron (which need not have plane faces), etc. This plainly underlies the optical doctrine of straight lines and planes.

For those who like their philosophy drawn particularly cool and mild, Lotze's system is just the thing; and the 'Microcosmos' (would there were a handy imprint of Misses Hamilton and Jones's translation) would be a good a book to begin with as those could have for whom one big but easy tome on philosophy will be all they have the courage to undertake. Yet whoever perseveres to the end of that work will most likely want more such reading, and may probably next go on to the 'Logic' and 'Metaphysics' of the same author. Those twin treatises mastered, the reader will certainly find it to his advantage to examine the refutation of them of which the volume that heads our list is the first instalment. It deals with Lotze's 'Logic,' and another is promised to make mincemeat of the 'Metaphysics.' Lotze is somewhat in sympathy with modern physics; he might be said to be one-third imbued with the scientific spirit. He had a medical student's initiation into the outer courts of science. His chief master was Gustav Theodore Fechner, a many-sided man like himself, say three-quarters scientific; but Lotze was not half as much so. That which more than all else distinguishes the thoroughly natural-scientific philosopher from the theological is that, in the former, all other passions are swallowed up in the passion to *learn the truth*, while the latter is fired with the impulse to *teach the Truth*. It is amazing what disparate conceptions of truth and of Truth those opposite attitudes carry. As for Lotze, it was teaching and not learning that engaged his energies. The life of the man was devoted to curing fellow-countrymen of the malady of Schellingism. Now, to a superficial glance, Lotze, though not untheological, is high in the ranks of the scientific among metaphysicians; while Schelling, with his eye fixed on Deity, was a babe in exact science. Nevertheless, a scientific specialist may well feel nearer to Schelling than to Lotze, because Schelling seems to really desire to find out the truth, ready at a moment's notice to dump all pet dogmas for her sake. The two revolutions his opinions underwent, though they are in many critics' eyes his shame, are his honorable scars in those of the physical experimenter. Lotze, on the other hand, having embraced Kantian nominalism at his first confirmation in the church of philosophy, fought for it doughtily all his life long, with that sort

of fidelity which, extolled in seminaries, sets the man of the world to looking out for other signs of immaturity or arrested development.

Prof. Jones has his attention far too closely riveted upon a single question (though it is undoubtedly the central question of Lotze's philosophy) to give a very good all-round account of Lotze's logic. For example, toward the end of the book he remarks in a foot-note:

"The doctrine repeatedly advanced by Lotze, that our ideas can be regarded as objectively valid, and that the process of thought leads to objective results merely because every one, on account of the constitution of the human soul, must arrive at the same results, does not seem to me to be worthy of serious discussion. Error would not cease to be error though all should commit it. It would, probably, not be recognized as error."

Now not "repeatedly" merely, but on every proper occasion Lotze advances that doctrine. It is, not certainly the most prominent, but the basic principle of his system, and if it is not worth serious discussion, why write a somewhat lengthy book about that system? But, after all, considering that Lotze holds that it is impossible for anything like the real to be thought, if all mankind were immovably fixed in a belief, so as to entertain no suspicion of it, in what would its erroneousness consist? It could not picture the real as different from what it is, since it cannot, on Lotze's theory, picture it at all. It could not represent things as like when they are not alike, if all things are somewhat alike, nor as unlike when they are like, if all things are somewhat unlike. Where, then, would be the error? It is difficult, not to say impossible, to see how, in Lotze's view, any judgment can be in error, except in the sense that it is destined to be reversed. Such reflections are, by this time, the very commonplaces of philosophy; but Prof. Jones seems not to be aware of them. As for what he can mean by saying that any error all men shared would *probably* not be recognized as error, the reader must answer that without any guess from us, for we can make none. The only plausible reply to Lotze's principle would seem to consist in denying that men ever can be immovably fixed in any error; but that is the principle stated in other words.

Prof. Jones is a half-idealist. He conceives himself to be a complete idealist; but he will find himself forced to pursue that path much further, for he has not yet emerged from nominalism. "No one," he says, "can assert that things in general exist." Can one not? A pendulum has been drawn to one side 86,400 times daily for twenty years, and every time it has returned to its position, and that at almost the same rate of speed. Was that chance-coincidence? If not, there was a really operative *law*. That law is general. It is not only general itself, but it applies to a general class of things; and if the law is real, the class is real. If for "assert" in the quoted sentence, we read "deny," we come nearer the truth. The idealist, to be consistent, will be forced to deny that individuals really exist, as such; and he will be ultimately led to hold that, while there are, of course, fictitious generals, yet some generals not only *exist* but *live*.

Lotze's 'Logic' was published twenty years ago. It is choked up with the eccentric notions with which the German logics of earlier date abounded. Thus, Lotze holds that every true hypothetical proposition is convertible; that is to say, from

the proposition, "If attraction varies inversely as the square of the distance, the orbit of one particle round another will be a conic section," it follows that "If the orbit of one particle round another be a conic section, attraction varies inversely as the square of the distance," although, in point of fact, it may vary directly as the distance. Again, he holds that our parts of speech are essential to thought, although it does violence to the majority of languages to classify their words in that way. A hundred such whimseys might be pointed out. But then logic was at a low and unpleasant ebb in Germany for the greater part of our century. Schröder's treatise, if the Germans will only go through it before they write, as they probably will, must render such nonsense impossible for the future.

No very close thinking was needed to refute Lotze's logic, at least in great part. Had it been required, and had Mr. Jones been able to supply it, it would have been impossible to set it forth in an easily readable book, in the literary way still expected in a philosophical treatise. An accurate logical discussion can no more be drawn up in such form than could a well-made balance sheet. The more elegant and perspicuous the style, the less clear would the statement be. However, in this case, nothing of the sort was needed; and the style is agreeable enough. Occasionally, points are made with remarkable neatness. Yet long successions of pages are diluted with such washings of insignificant words as we marvel to encounter from the pen of a student of poetry. Nevertheless, for the substantial result of the engagement, idealists of all stripes--and they make up the great majority of thinkers, nowadays--will opine that such deadly holes have been made in the sides of Lotze's frigate that no patching can render her seaworthy. At the same time, a goodly number will think Mr. Jones has himself some lessons to learn in idealism yet, and he certainly has much to learn in logic.

61 (11 July 1895) 34-35: **The Source and Mode of Solar Energy throughout the Universe**.

By I. W. Heysinger, M.A., M.D. Philadelphia: J. B. Lippincott Co. 1895.

CSP, identification: MS L 159.62; MSS 1512, 1513 (drafts). See also: Fisch, *First Supplement*.

Dr. Heysinger writes charmingly, and has made a better book than any mere crank could ever make. Nevertheless, we do not read far in it before we find he has not the knowledge his difficult problem requires. The following undiscriminating sentence, which we come upon at the beginning of the third page, is conclusive as to that:

"The authorities cited in this work include many illustrious names: Proctor, Tyndall, Helmholtz, Langley, Huggins, Newcomb, Young, Flammarion, Balfour Stewart, R. Kalley Miller, Herschel, Nichol, Lord Rosse, Urbanitzky, Crookes, Fraunhofer, Ball, and many others, all of whom are known throughout the world as among the master minds of science."

The theory is not perspicuously stated, but some features of it are plain enough. Dr. Heysinger, while not utterly rejecting the condensation theory of the sun's energy, thinks that there are only slight condensations and expansions of the sun. These

answer the purpose of a fly-wheel or governor to keep the radiation uniform. He holds that the instrument for the collection of energy in the sun is an electrical instrument. The total amount of energy spent is only that absorbed by the planets, for the rest is put in bank--that is, the thermal radiation into vacuous space becomes converted into electrical energy. How does that portion which is spent get replenished? We should suppose by the retardation of the orbital motions of the planets, since Dr. Heysinger argues, and with no small force, they they must be influence machines. But he seems to think influence machines work at the expense of the energy of the air, though everybody who has ever turned the handle of one is conscious of doing more work as the machine comes into action. However that may be, the electricity thus collected flows, says Dr. Heysinger, to the sun, and its energy is there converted into heat.

This conflicts at several points with opinions that are backed by pretty solid considerations. It supposes, for example, the planetary spaces to be filled with ordinary vapors. Why, under those circumstances, the sun does not withdraw the atmospheres from the planets, is a problem the author ought to have worked out before coming before the public. That the resistances, mechanical and electrical, would not be too great to be admitted, is not shown. The calm way in which aqueous vapor at the temperature of the planetary spaces is talked of is startling. Its pressure might be something like that of Crookes's tubes--what are called "high vacua." In fact, the difficulties are multiform. At the same time, there is hardly room for doubt that electricity does play a part more or less important in the solar system, and speculations of the same complexion as Dr. Heysinger's, but more exactly reasoned, will be much heard of in the near future. The book is certainly not without interest as suggestive of points some of which may be sustained.

The last chapter but one is occupied with a minute verbal examination of the Hebrew form of the Chaldæan cosmogony.

61 (22 August 1895) 139-140: Mental Development in the Child and the Race: Methods and Processes.

By James Mark Baldwin, M.A., Ph.D., Stuart Professor of Psychology in Princeton University. With 17 figures and 10 tables. Macmillan. 1895. Pp. xvi, 496.

Attributed to Peirce by Fisch in *First Supplement* (internal evidence). Peirce reviewed other books by Baldwin. There are references to Bain and to color perception which suggest Peirce's hand. This review is unassigned in Haskell's *Index to* The Nation, vol. 1.

Few American writers on philosophy and psychology have enjoyed greater fame and success than did James Mark Baldwin. He graduated A.B. from Princeton in 1884, and remained there to take an A.M. in 1887 and his Ph.D. in 1889. Baldwin was professor of metaphysics and logic at the University of Toronto from 1889 until 1893, professor of psychology at Princeton from 1893 until 1903, and professor of philosophy and psychology at Johns Hopkins University from 1903 until 1909. His

Dictionary of Philosophy and Psychology was his best-known work. With James McKeen Cattell, he co-founded the *Psychological Review* in 1894. Baldwin was awarded an honorary D.Sc. degree from the University of Geneva in 1909, and an LL.D. from the University of Glasgow in 1901. As a tribute to his great achievements, he was given an honorary D.Sc. degree by Oxford in 1900, the first time that degree was ever given by that university.

It is not to be concealed that this book is not well written. The author himself makes no effort to do so--he recognizes it rather with a charming frankness. He speaks of the conflicting aims he had in writing it, of his inability to hit upon an arrangement of his material that would satisfy him, of the "need of a patient reader before the page." In spite of liberal warnings in the preface, and of statements of the problem and of what the several chapters contain, the perplexed reader about the three-hundredth page is inclined to throw the book aside, or to wish, when he finds the scattered threads at last being drawn together, that it had been written just backwards. The pages are too full, too well-fed. There *is* a central problem, a main discussion, but it lies so imbedded in remarks and considerations by the way that one is perpetually losing sight of it. Prof. Baldwin is essentially an inquirer at first hand, and he carries his method over into his exposition; he begins with his apparatus, he keeps one in suspense, he determines his shortcomings, he goes into side issues, he shows one his uncompleted work. With the best intentions in the world he is bewildering. And his sentences at times are as puzzling as his order of exposition. And yet, when one has vented one's spleen, one's abiding impression of the book is very favorable. Its pages breathe something of the intimacy, the frankness, the personal charm of a clever investigator's note-book. The fragmentary state of some of the experiments, the loose threads and ragged ends, give one the not unpleasant sense of being admitted behind the scenes, of seeing science in undress--in the making. And the substance of it all--of the experiments and discussions themselves--is capital.

'Mental Development: Methods and Processes' is a preliminary volume to be followed under the same general head by a companion sub-entitled 'Interpretations: Educational, Social, and Ethical.' Prof. Baldwin regards it as the fundamental weakness of current sociology that there exists no theory of the *socius*. It is this deficiency which his projected volume will endeavor to supply. The present one, leading up to it, yet possessing an independent value, deals with the evolution in the individual of conduct adapted to enviroment [[sic]] and of voluntary self-control. The hypothesis which has hitherto obtained the widest acceptance is best known in the form Prof. Bain gave it in his 'Emotions and the Will.' Granted, it says (what seems to be the observable fact), that the earliest movements of the new-born individual are quite at random, and that activity which results in pleasure tends to be continued and that which results in pain to be stopped, it may be shown according to mere natural selection how there would arise in the lapse of time habits of pursuing pleasure and avoiding pain, and a connection more or less complete between pleasurability and wholesomeness, pain and harm. This hypothesis, so far as it goes, Prof. Baldwin accepts, but it goes nothing like the whole way.

(1.) It proceeds as if all pleasures and pains came to the organism originally as effects of (random) movement on its part--which is in the majority of instances quite the opposite of the truth--"turns the case completely over," as Prof. Baldwin says, "and stands it on its head." It is the environment (including the living creatures that in part constitute it) that commonly takes the initiative, brings to bear on the organism some fresh stimulus of pleasure or pain--it is to this in the main, if not exclusively, that the "random movements," when they exist, are due.

(2.) It proceeds as if the organism were entirely dependent on the enviroment [[sic]] for the repetition of an agreeable stimulus; whereas the fact is, that the organism possesses the power of *Imitation* (Prof. Baldwin calls it), of taking a hint, of eking out the gaps in a defective environment and securing the repetition of a desirable stimulus by efforts of its own.

These two positions Prof. Baldwin argues with much point, in detail. Of their importance as completions of the Spencer-Bain theory of development, there can hardly be dispute. But whether the second of them is as distinctly an addition to that theory as is the first--whether *Imitation* is an independent factor in the result--is something more doubtful. Adherents of the Spencer-Bain theory have commonly held that, if the hypothesis of random movements and "lucky" chances would account for adapted conduct of any kind, it would account for *Imitation*.

This is the main current of the book, but, as has already been indicated, the discussion is of a richness and breadth of which the foregoing gives but a scant idea. Notably there are some admirable chapters early in the book on Distance-and Color-Perception by Infants, the Origin of Right-Handedness, the Rise of Tracery Imitation and the Origin and Analysis of Handwriting, and on Suggestions.

61 (14 November 1895) 353-354: Studies in the Evolutionary Psychology of Feeling.

By Hiram M. Stanley. London: Sonnenschein; New York: Macmillan. 1895.

CSP, identification: MS L 159.78; MS 1397 (draft). See also: Fisch, *First Supplement*.

Mr. Stanley, in the agreeable essays which compose his chapters (and we are not yet become so German in this country as to hold that their readableness *per se* detracts from their scientific usefulness) limits feeling to pain and pleasure. We had almost said that he limited it to pain; for he makes pain so much the more important that but a single step remains to be taken to pronounce pleasure to be only a specially adjusted pain. The great function of knowledge, upon his theory, is to bring about pain, or its supplement, pleasure. He will have the primitive consciousness to be a "pure pain." Pure pain is not an easy phrase to apprehend; the less so that we are accustomed to expect the first emergence of a new experience that does not burst out with too sudden a shock, to be pleasurable. Throes come later. Mr. Stanley, however, reminds us that both birth and death, to judge by appearances, are states of pain, and of almost undefined pain, at that. His remarks are decidedly worth reading. For the post of primitive emotion he nominates fear, defining it as "pain at pain." Instructive evolutionary analyses of despair, anger, surprise, and disappointment, retrospective emotion, and desire, supply materials for as many chapters.

In "Some Remarks on Attention," the author limits that term to the voluntary sort, and so inevitably fails to bring out its intimate connection with emotional association. He identifies attention with the effort to attend; and when he speaks of attention as essentially painful, he even seems to have in mind a more or less *unsuccessful* effort. For vigorous, effective attention leaves, we conceive, little room in consciousness for pain. Yet here, as elsewhere, we find acute observation.

Self-feeling, including pride, shame, and the like--and we cannot help thinking our author's ideas would have had more systematic unity if he had made pride a special variety of shame--is considered as the main ingredient of self-consciousness. Æsthetic activity is defined as "an independent self-activity of some sense, or of perception, or of imagination, or emotion, impelled by a pleasure; this pleasure, being a distinct and new form, we term æsthetic." The self-activity of a mental faculty! What will Herbartians say?

We are happy to find in one of the last chapters an essay at a partial analysis of literary style. Some of the shortcomings of Herbert Spencer's theory are made manifest. The author does not pretend to afford a catalogue of all the psychological factors of style. Perhaps he might simplify the problem if he would hold fast to one of Spencer's points, namely, that the purpose of style is to convey ideas. Spencer himself, alike in his theory and in his practice, whatever he explicitly says, virtually assumes that to convey an idea is nothing but to state it. But the style of a real master is one which succeeds in immersing the reader in the fleeting part of thought, the liquid menstruum which flows around its rigidly objectified conceptions; and *that*, no statement, however perspicuous, can effect.

Without any astonishing power of thought, this book will do good service for that inquiry into the feelings which now so much engages the psychologists.

61 (28 November 1895) 395: **The Psychology of Number, and its Applications to Methods of Teaching Arithmetic**.

By James A. McLellan and John Dewey. [International Educational Series.]

D. Appleton & Co. 1895.

CSP, identification: MS 1398 (draft). See also: Fisch, *First Supplement*.

The two respected scholars whose names appear upon the title-page of this work, in their eagerness to illustrate the important truth that the teaching of arithmetic ought to be based upon a thorough acquaintance with psychology, seem to have clean forgotten that there are two other legs to the tripod on which it should rest, viz., an exact logical analysis, and a lively appreciation and comprehension of the nature of mathematics in general. As long as nothing but psychology is called for, what they say is admirable--this, for example:

"There are but few children who do not at first delight in number. Counting (the fundamental process of arithmetic) is a thing of joy to them.... When, under the formal teaching of number, that interest, instead of being quickened and strengthened, actually dies out, the method of teaching must be seriously at fault. ... The native aptitude for number is continually baffled.... The child is adjudged to have no interest in number and no taste for mathematics; and to nature is ascribed an incapacity due to irrational instruction.... It is perhaps not too much to say that nine-tenths of those who feel that they have no aptitude for mathematics owe this misfortune to wrong teaching at first."

Excellent; and even more true of geometry than of arithmetic. Again, the authors are admirably alive to the futility of any attempt to teach any branch of mathematics until the scholar has been made to feel with some accuracy what the use of it is. Of course, you cannot convey to young children your own meaning when you say, what all grown people discover, that the happiness of life very greatly depends upon facility in ciphering. Yet you must try to cast some adumbration of the distinction between happiness and misery upon their poor little hearts, and make them associate the one with arithmetical expertness and the other with neglect of numbers. That should be Lesson I. in arithmetic.

Upon such generalities of the art of teaching, too usually neglected, Profs. McLellan and Dewey are perfect. But these things have been said before. When we come down to details, depending upon the nature of mathematical thought, they are more often wrong than right. They have allowed secondary truths and half-truths to run away with them. For instance, their own conception of the purpose and end of numbers is not true either in scientific mathematics or in life. They tell us that numbers are for measurement, for valuation. This is not accurate; but, waiving that for a moment, we ask what measurement and valuation are for? Though we are not explicitly told, we gather that they are supposed to be mainly useful to improve our general conceptions. But that may be disputed. Except casually and secondarily, numbers--arithmetical numbers--do not contribute to our conceptions. Their greatest utility by far--a utility more fundamental than that of all the constants of physics or the tables of the statisticians--is to keep our daily accounts, and so continually to check up our conduct without imparting any definite ideas. For 999 school-children out of a thousand this is destined to be the chief manner in which a knowledge of arithmetic will further their welfare. The corollary is, that the casting up of columns of figures easily, and swiftness in making those mental calculations which will enable them to overreach others in bargaining, are the bull's-eyes to be aimed at. True, man does not live by bread alone; but it must be the chief of his diet, so far as his nutriment is to come from arithmetic. If the Theory of Numbers were studied, that undoubtedly would afford a valuable training in logic, and impart some little exercise in mathematical generalization and even in mathematical imagination. But there is very, very little of all that in Vulgar Arithmetic.

It is a capital error, leading to the worst pedagogical blunders, to suppose that numbers have any significations whatever. Are "Eeny, meeny, mony, mi," etc, significant words? Charles Leland says they are Gipsy numerals. At any rate, they have precisely the force, use, and sense of primitive numbers. The essential use of numbers is that which the tourist in Italy has to put them to when he lugs about with him some baker's dozen of hand-packages, big and little, of all shapes and sorts, with no semblance of equality, but so many detached things, liable to be separately lost, and requiring to be incessantly counted. He might just as well use the words, "Eeny, meeny, mony, mi," etc., as "One, two, three, four," etc. Or he might use the syllables of "Ein feste Burg ist unser Gott," etc. In fact, the system of cardinal numbers may be defined as a scale of vocables, used as intermediary, for the establishment of one-to-one correspondences between different collections. To say, as our authors do, that the idea of measurement enters into the idea of all number, and should never be lost sight of in teaching, is on its theoretical side to confound things usefully distinguished, and on its practical side to fly in the face of the embryological maxim that the development of individual thought must follow the course of the development of thought in history. After we have once got the idea of metrical equality, we can perceive that it is verbally applicable (with little or no meaning) to the units of numeration. But that reflection does not help us to count. Who can doubt that man counted for ages before he began to measure, in the proper sense, and therefore before he had the conception of metrical equality?

It is useful to distinguish the different purposes of numbers, because a distinct system of numbers, each with its distinct modification of logic, is used for each purpose. For *counting*, we must use cardinal numbers; for the *assignment of places in cycles*, we use limited series (the names of the days of the week is such a system of numbers); for *dating* in series running indefinitely both ways (like the years B.C. and A.D.), we require negative as well as positive integers; for *measurement*, we need rational fractions; for *reasonings about continua*, where first the idea of a limit comes in, we need surds; for *comparing functions*, we need imaginaries; for *four-dimensional continuous numerations*, quaternions. It is a truism to say that, in teaching, one idea should be inculcated at a time, and the most rudimentary first. But above all in arithmetic, which, in its most practically important aspect, is a question of physiological habit of rapid, accurate ciphering, the less the operation is interfered with by superfluous reflections, the better. No doubt it is necessary to teach certain simple methods of reasoning; but they must be performed in an instinctive, quasi-mechanical way, without philosophizing. One can, with a microscope, detect traces of mathematics in vulgar arithmetic, but there is so little intellectual value in it, while it is so stupendously important as a practical skill, that it is better to sacrifice the former utility to the latter. To evaluate this work from the former point of view we should need no monstrous number, while from the latter standpoint we fear the datary system of numeration would have to be drawn upon.

61 (19 December 1895) 447: ACETYLENE AND ALCOHOL

This letter by an anonymous author is answered by Peirce at 61 (26 December 1895) 464.

TO THE EDITOR OF THE NATION:

SIR: A great deal has been printed in the papers lately in regard to acetylene gas, and I think it is time that chemists should characterize some of the statements made. It has been represented in the stock reports, etc., and the writer has even seen it stated or implied in the *Nation*, that acetylene could be made at such prices as to confirm its use for the manufacture of alcohol, benzole, or nitrobenzole, and aniline, from which the colors could be obtained.

Now there is just enough chemical truth in this to deceive the ignorant or the partially educated. It is true that acetylene can, to a small extent, be decomposed into benzole, that it can, by a roundabout method, be decomposed into alcohol; but it is utterly false that it can be done on any commerical [[sic]] scale to advantage. We might as well say that because marsh gas, which is the main constituent of the natural gas of Pennsylvania, can be decomposed by chlorine into chloride of methyl, and from that can be made into wood alcohol--we might, I say, as well advertise that alcohol can be made at fabulously cheap prices in this method, as to claim the same for acetylene.

The decomposition of water power into electricity, and that electricity into chemical energy, the use of that chemical energy to decompose a mixture of carbon and limestone, and the subsequent use of the carbide of calcium, is a very interesting, and, for some purposes, may be a useful process; but any one who will for one moment consider the matter will see that the original electricity directly converted into light must inevitably be far more economical than this very roundabout method, implying large losses in lime, in coal, and in electrical energy which are incident to the process.

To chemists who have examined the process critically and without previous bias, it seems a very great pity, if not a shame, that such an interesting chemical discovery should be so exaggerated as to become almost as much falsehood as truth. There is an old adage that "a lie that is half the truth is ever the blackest of lies."--I remain, sincerely yours,

CHEMIST.

61 (19 December 1895) 453: **Great Astronomers**.

By Sir Robert S. Ball. London: Isbister; Philadelphia: Lippincott. 1895.

CSP, identification: MS 1399 (draft). See also: Fisch, *First Supplement*.

Robert Stawell Ball (1840-1913) was a British astronomer and mathematician. He received his education at Trinity College, Dublin, and later (1867-1874) served as professor of applied mathematics and mechanism at the Royal College of Science, Dublin. After a lengthy association with Dublin University as professor of astronomy and royal astronomer of Ireland, Ball became Lowndean professor of astronomy at Cambridge in 1892 and remained in that capacity until his death. He was knighted in 1886.

A book of fourteen essays on the lives and works of as many astronomers, ancient and modern, but all deceased. It would have been a great subject for a great writer, for a scientific Sainte-Beuve. We cannot quarrel with the list of men treated. It was certainly best the writer should choose those whom he liked to write about. They are Ptolemy, Copernicus, Tycho Brahe, Galileo, Kepler, Newton, Flamsteed, Halley, Bradley, W. Herschel, LaPlace, Brinkley, J. F. W. Herschel, Rosse, Airy, Hamilton, Le Verrier, and Adams. Some of them are pretty well-worn subjects. Brinkley, Adams, and Airy are probably the only ones concerning whom any new facts are here made known. Still, for the majority of the others, masterly critical accounts would have been welcome. It would have been better to make half as many essays twice as long.

No one method of treatment is followed, so that to get an idea of the book it is necessary to examine several of the lives. Sir Robert Ball is himself a brilliant mathematician and a very competent astronomer. He does not seem to be aware that several of the personal traits of Claudius Ptolemy can be made out from a diligent study of his writings. He does not even mention the fact that there is an astrological treatise, bearing the name of

Ptolemy, which resists every test of genuineness. It has the great astronomer's method, style, and language; it bears his name and is addressed to the same person as the 'Almagest.' The geography of Ptolemy is only just mentioned, without any account of it. Sir Robert Ball cannot be ignorant that Ptolemy was a great mathematician, that he created spherical trigonometry, and calculated tables for the application of it. Was not this worth saying in a sketch of twenty-three pages? With what are these pages filled? Chiefly with an account of the Ptolemaic system of the world. But-we must suppose for the sake of simplification, for it cannot be that Sir Robert Ball is so ignorant--this account is falsified in such a manner that all that really was contributed to it by Ptolemy is erased, and what is provided is the rough description of the motions which was invented by Hipparchus, and which Ptolemy made his own by one of the most wonderful and profound pieces of mathematical igenuity [[sic]] ever devised. Namely, Hipparchus had two circles, one carried round on the other, the planet being carried round the former. One of these motions is in geometrical truth nothing but the motion of the sun round the earth, or, what is the same thing (since in mere geometry all motion is relative), the motion of the earth round the sun. The other motion is the motion of the planet round the sun. The orbits are really ellipses; but they are so near circles (except for Mercury) that the difference in *shape* was not perceptible to the ancient observers. But the foci of the ellipses are perceptibly remote from the centre; and, moreover, since equal areas are described in equal times, the motion is physically more rapid near one focus than near the other. Now, Ptolemy almost perfectly represented all this by placing the orbit eccentrically from the earth, and then making the body that was supposed to move round the circle describe equal angles in equal times about a centre twice as far from the earth as the centre of the circle, and lying in the same direction. Nor was the motion in the second circle taken to be uniform. These hypotheses gave an essentially different character to the theory. The system of Hipparchus was a mere harmonic analysis; the Ptolemaic system needed only minor corrections to represent the true *relative* motions, or appearances in the heavens (which Ptolemy declared was all he aimed at), with a high degree of accuracy. At any rate, this, and this only, was the contribution of Ptolemy. It is a strange account of the man which attributes to him what was really done by another, and omits all mention of what he really did.

The account of Copernicus is hardly better. There is no allusion to the practical side of his life, his management of the vast estates of Frauenberg, his parliamentary career. His planetary system is not explained at all, although this has been successfully done in several elementary books. The differences between the points of view of Ptolemy and Copernicus are not explained. Ptolemy really only cared to account for the appearances of the heavens; and he clearly saw that it made

no difference, with those appearances, whether the earth moved or not. It was a mere surplusage--a curious inquiry, somewhat metaphysical--to ask whether the earth moves or not. Then, in Ptolemy's time, everybody assumed that there is one substance of things celestial and another substance of things terrestrial. The stars were mere lights. There was no matter in them, such as we see on earth. There was thus no analogical argument to be drawn against their moving ever so rapidly. On the other hand, if the earth moves, then real terrestrial objects are moving with a velocity very many times greater than any of which we have any experience. Copernicus, on the other hand, lived at a time when the discovery of the New World inclined men to think that things remote were a good deal like things near at hand. Moreover, reflection upon Ptolemy's system, just as he had described it, would show that the general conception of it could be much simplified by making all the planets revolve round the sun. Ptolemy would not care for such simplification, as long as the computations of places remained the same, as they would remain. But in Copernicus a deeper philosophical spirit pervading Europe was beginning to awaken.

The life of Galileo is marked by the same inadequacy. Galileo's greatest discovery, that of the uniformly accelerated fall of bodies, is not mentioned. Kepler receives still worse treatment. One of the most interesting personalities that ever lived, and the author of the most wonderful piece of inductive reasoning, with the exception of Mendeleieff's [[sic]] law, ever performed, he is so feebly described that the reader gets but a glimpse into the nature of the man and none at all into the course of his thought. The modern essays are no better. The men, too, are smaller and less interesting. The chapter on Brinkley has two and a half pages about Brinkley and ten and a half about other things. It is modern superficial writing about a subject that would formerly have engaged the best talents, carried to its last point of newspaper-paragraphing.

61 (26 December 1895) 464: ACETYLENE AND ALCOHOL

Attributed to Peirce by Fisch in *Third Supplement*. At this time Peirce was very interested in the commercial aspects of acetylene gas production. This review is unassigned in Haskell's *Index to* The Nation, vol. 1.

TO THE EDITOR OF THE NATION:

SIR: The violent protest uttered by "Chemist" against certain statements about the possible commercial future of acetylene should not be allowed to pass without comment. The question of advantageous production of alcohol from acetylene is not to be settled in any such summary and off-hand fashion. The possibility or impossibility of making wood alcohol from marsh-gas at a fabulously cheap price has no bearing on the matter. It is a problem by itself, and chemistry does not indicate that the solution must be unfavorable. It is simply a question of the cost of production starting with calcium carbide, as against the cost when grain is used, and must be worked out empirically. If the chemical reactions which are involved can be effected on the large scale with something like theoretical results, carbide of calcium at $40 a ton may very well enter the lists against grain as a profitable source of alcohol.

As to the use of acetylene for illuminating purposes, "Chemist" says "that the original electricity directly converted into light must inevitably be far more economical than this very roundabout method, implying large losses in lime, coal, and in electrical energy." If electricity could be converted into light directly, and without loss, as this statement seems to imply, acetylene and indeed all other sources of artificial illumination would be out of all economical consideration. But electricity cannot be converted into light without loss. There is loss from the instant that the current leaves the dynamo--loss in the conducting wires, loss in the transformer; and when the electricity which finally reaches the lamp is transformed into radiant energy, only about five per cent. of this is light. The making of acetylene does not necessarily imply large losses in even such cheap materials as coal and lime, and it is by no means "inevitable" that the light given by acetylene gas must be far more costly than the electric light. The matter is not to be disposed of by dogmatic, assertion; it will be determined by experience, and experience thus far indicates that "Chemist" is wrong.

It is to be expected that one who dismisses the commercial claims of acetylene so abruptly may have rather hazy ideas in regard to the character of chemical reactions and the transformations of energy. That this is the case is certainly suggested by the language which "Chemist" employs. According to him, acetylene is "decomposed" into benzole (benzine) or into alcohol; and marsh gas is "decomposed" by chlorine into methyl chloride. None of these reactions are decompositions; the first two are building-up reactions or syntheses, and the third a substitution. Energy in the form of water power may be transformed into electrical energy, and this in turn is transformed in the electrical furnace into the heat necessary to bring about the reaction between lime and carbon which results in the formation of calcium carbide; but "Chemist" has it that water power is "decomposed" into electricity, and electricity into "chemical energy" which "decomposes" the mixture of carbon and lime.

S.

1896

62 (2 January 1896) 16-18: BENJAMIN'S HISTORY OF ELECTRICITY

The Intellectual Rise in Electricity: A History.

By Park Benjamin. Appletons. 1895.

CSP, identification: Haskell, *Index to* The Nation. See also: Burks, *Bibliography*; *List of Articles*; MS L 159.81.

The present history is, in its two halves (the first down to Gilbert inclusive, and the second from Gilbert's successors to Franklin, inclusive), of very different orders of merit; the last part being much the more valuable. In the first part, in which we miss any reference to the graceful, useful, and beautifully printed translation by our countryman, Dr. Mottelay, of Gilbert on the Magnet, which we reviewed some months ago, every scrap of information has been diligently collected; but our comments will show that the work has its blemishes. In the second half, this work comes into competition with Dr. Priestley's 'History and Present State of Electricity,' which, besides being a thorough and full account of the matter, is also a particularly well-arranged account, which can hardly be said of Mr. Benjamin's. Priestley's is also entirely free from the sensational tone of our *fin-de-siècle* style. But there is enough, both of fact and of well-executed general sketches of historical situations, in the volume before us to establish it as the leading work on the subject in any language.

In the period antecedent to the death of Bacon there is much baseless conjecture. Thus, Mr. Benjamin guesses that Gilbert lived in London in Linacre's house. But he could easily have ascertained that Dr. Gilbert lived in the lane called Peter's Hill, south of *Little* Knightrider Street, while the Linacre house was No. 5 of Knight Rider Street proper, and, we believe, on the north side. While thorough scholarship was not an indispensable qualification for Mr. Benjamin's task, we could wish there were fewer indications of the lack of it. On the second page of the first chapter we read that Homer ("Iliad,Z.[1] 513:T.[1] 398") calls the sun {ëlechtor}. A proof reader familiar with the looks of Greek words would have challenged that. Boesius is the name which Mr. Benjamin gives to the philosopher Boetius. We are familiar with Boethius and even Boecius, but do not remember Boesius. Under the reign of "Aelfred," Mr. Benjamin informs us that Scotus Erigena "began the assertion of the scholastic philosophy." There are three errors here. In the first place, Erigena (whom it is no longer permissible to confound with another Irishman at the court of the Mercian King) was not a subject of Alfred. In the second place, the scholastic philosophy did not consist in any assertion. It was the philosophy taught in the lecture-rooms (*scholæ*) of the mediæval universities. The only philosophical proposition concerning which the scholastic doctors were agreed was the practical infallibility of Aristotle. What marked their teaching was, first, its general form (it was usually either a commentary or a disputation, or both), and, second, the algebra-like formality of its statements. Scotus Erigena was not a scholastic; for, first, he lived over three centuries before the

regular organization of the universities, and in a deeply dissimilar civilization (or want of civilization); second, he is not an Aristotelean; third, the 'De Divisione Naturæ' is neither a commentary nor a disputation; fourth, it is not marked by great formality of statement; fifth, it is in no sense a school-book. The university of Alexandria, according to Benjamin, was "begun by Alexander." We apprehend it will be necessary to take the will for the deed, to make that out. As ornaments of that university are mentioned Archimedes and Hipparchus. The former did study and the latter may have studied there; but Archimedes did the work of his life in Syracuse, and Hipparchus at Rhodes and elsewhere. He did not observe in Alexandria.

Mr. Benjamin's references are not seldom inaccurate. The following is a single specimen: "Vincenti Bellovacensis: Speculi Naturales, etc., tom. ii., lib. ix., c. 19." On one of the first pages there is a faulty reference to a passage in Pliny, which is all the worse because Pliny is not quite accurately reported. Even the scientific statements are often careless. Thus, we are told that the orientation of the Great Pyramid is in error by 19' 58", and that a surveyor "with the best modern compass" could hardly do better. Now, to begin with, the error of orientation is only about 1 ¼', which, being the *minimum visibile*, is as small as the probable error of the best possible naked-eye observation. No modern surveyor, when he wants to do nice work, dreams of employing a compass; and, for that reason, there has been no attempt to develop a compass of precision. But in all magnetical surveys the deviation of the needle is ascertained far more closely than the figure given.

But let us come to the substance of the work. The author has unfortunately a theory. If it were a very broad and instructive theory, especially if it were very solidly founded, this would be no misfortune. But it is neither broad nor solid. It is that the knowledge of the earliest form of mariner's compass came from the Baltic town of Wisby, that it came to Wisby from the Finns, and that it had been, perhaps, an ancient heritage of the great "Turanian" race. Apparently because that theory is sadly in need of support, the author accepts without the slightest reserve the theory of Mr. Terrien de Lacouperie of the Elamite origin of the earliest Chinese civilization. Singularly enough, however, when it comes to accounts of the Chinese possessing compasses before the Europeans, he becomes unexpectedly sceptical. The letter of Klaproth of 1835 is generally supposed to have proved the proposition that the Chinese, some time before A. D. 400, at latest--that is, many ages before the Europeans--knew that a needle could receive directive force from a lodestone. As for the Egyptians, Dr. Benjamin reaches the sane conclusion that they knew nothing about magnets, though the process by which he reaches that result is open to some objection. As for knowledge of the magnet on the part of the Greeks and Romans, it is easily stated. Dr. Benjamin drags in irrelevant matter from Rossignol's essay on the mythology of Greek miners; but, for the matter in hand, the well-known passage in the 'Ion' of Plato gives all the information there is. Namely, the Greeks knew that a lodestone would lift an iron ring, and that another, and so on; but they knew nothing of the polarity of the magnet.

It is next to impossible to prove the negative proposition, that the mariner's compass (in some crude form) was not known at a given date. Such is the stupidity of man that it would be known for a very long time before it came much into use. On an Arabian vessel we first hear of it, Mr. Benjamin assures us, in A. D. 1240. Since the needle was floated on water, and was magnetized then and there (only soft iron being at hand), it would be used only on cloudy nights when the sea was pretty calm. It might go a long time unrecorded in a book; and it might be recorded in numbers of books before it was recorded in one which Western scholars have read. To show how slow progress was in those days, the compass is mentioned (as Klaproth shows) as a familiar thing in the laws of Alfonso X. of Castile dated A. D. 1263; and yet the evidence seems to be (we are indebted to Mr. Benjamin for this) that Spanish galleys were never supplied with it before 1403. The rational conclusion seems to us to be that it was probably known in the Mediterranean before A. D. 1200; but, owing to the choppy seas, it was little used in these waters until it was balanced on a point.

We now turn to northern waters. The Norsemen used to follow the method of Noah, except that they sent out ravens instead of doves. The earliest description of the mariner's compass (in precisely the same form as that of the Arabians of A. D. 1240) which Mr. Benjamin finds is in Neckam's book 'De Natura Rerum,' written about 1180. He gives a flattering portrait of Neckam, and compares his book with the 'Origines' of St. Isidorus. But surely the two greatest merits of an encyclopædia are to be full and to be compressed. The work of St. Isidorus in twenty books has both those merits in an eminent degree. Considered as an encyclopædia, the work of Neckam is contemptible, being both small and garrulous. Within a few years after Neckam, notices of the compass in northern waters multiply. M. Paulin Paris gave in 1842 some verses by Guyot de Provins and some others by another poet. Dr. Benjamin has very prettily translated several of these; but the originals would have been quite worth giving, too. Within fifty years of the first passage in Neckam we know of near a dozen passages referring to the compass. The contrast between this state of things and the single Arabian passage may be attributed to the thorough overhauling of early European literature. The inference is, that the compass could have been very little known, if at all, in Normandy much before the earliest of these quickly succeeding notices. Therefore, although the balance of evidence inclines toward the supposition that the compass was known in the north before it was known in the Mediterranean, it inclines only slightly that way. As far as investigation has gone, there is no evidence whatever of the compass having been known in those early days in the Baltic. True, it is mentioned as of great importance in the laws of Wisby; but it is probable that that law was a late insertion. We should expect that the compass would in its early shape have been used in the Baltic, owing to the fogs and the smooth sea; but positive evidence is altogether wanting.

Mr. Benjamin seems to regard the invention of the early mariner's compass as an exceedingly difficult one. If that be just, then decidedly the probable hypothesis about its introduction is that of Klaproth, that the Arabs got it from the Chinese, and that from them the knowledge was carried through, or crept round,

Europe to the north. But it may be doubted whether the invention is so difficult that it might not, without improbability, be supposed to have been independently invented in different places. Is it incredible that a man playing with two lodestones should find out their polarity, and then magnetization, and then the directive virtue of the needle?

The latter half of Mr. Benjamin's history, after taking leave of Gilbert, is, on the whole, much the more interesting. To be sure, no startling discovery was here possible. The succession of discoverers was Von Guericke (Hauksbee?), Gray, Du Fay, Watson, and Franklin. Mr. Benjamin modifies a little here and there our notions of what each did. It appears that that Sagredo who takes the leading part in Galileo's dialogues, not only was a living person, like the personages of Aretino's dialogues, but also probably discovered the secular change in the variation of the compass. He mounted a lodestone of five pounds so that it would support twenty pounds. It was in experimenting with that lodestone that Galileo found out the effect of the armature in causing the magnet to grow in strength. The Jesuit Nicolaus Cabæus is another old physicist whose achievements, as Mr. Benjamin states them, are of quite another order of importance from what we had supposed. To make our meaning clear, let us say that there are five departments of work in any branch of pure physics, like electricity; namely, (1), the phenomena have to be brought out and seen; (2), suitable instruments have to be invented for their study; (3), the process of experimental analysis, or cross-questioning of Nature, must be applied so as to produce statements of the laws of the phenomena; (4), measurements have to be made (though, of course, there was little of this in the pre-Franklinian ages); and (5), hypotheses, mechanical or other, must be constructed and experimentally verified to show the inward nature of the phenomena. What we have hitherto been told about Cabæus was that he extended the list of electrics; that is, he slightly increased the range of a known phenomenon. But it now appears that he observed that when little bodies are attracted to an electrified body and strike it, they are at once thrown off from it. Now this observation was the first step necessary in the experimental analysis of the phenomenon, ultimately leading to a knowledge of its laws. Nor was that all. For it seems that Cabæus was the first to plunge a lodestone into a mass of iron filings and notice the result; and, further, that he made an analogous experiment by plunging electrified amber into a quantity of sawdust. Here he took a step of the second kind, in our enumeration; for these things were instruments of observation of high importance.

In many places, Mr. Benjamin fills up the gaps of history in this way. Nor does he neglect the historian's more difficult tasks. He pictures the fad for experimentation that was caused by Charles II.'s interest in it. He shows that that interest was pretty deep, too, and that it had a most stimulating effect upon experimental science in England. In France, on the other hand, the hollowness of Louis XIV.'s endeavor to interest himself in science, combined with the total absence of interest on the part of Louvois, are fully proved to have had a very unfortunate effect on French science. All such general sketches have been executed by Mr. Benjamin upon a basis of thorough study.

There are few contested points in the history of electricity from Gilbert to Franklin. One of these is whether Cuneus, a gentleman of Leyden, had any hand in the discovery of the Leyden jar. In the first printed account of it by the Abbé Nollet, in the 'Mémoires de l'Académie Royale des Sciences' for 1746, it is said that Cuneus had seen some of the experiments upon which the celebrated Musschenbroek of Leyden was then engaged, to ascertain whether the effects of electricity would not be increased by enclosing the electrified body in glass, and that Cuneus undertook to repeat one of them at his home. But instead of leaving the flask in which the conductor to be electrified was placed, on the table, he held it in his hand, and thus got a strong shock. It was afterwards said that Cuneus had nothing to do with it; that that was a story got up to detract from Musschenbroek's credit. But Dr. Priestley, writing his history only twenty years later, was in a condition to collect testimony. He says: "The views which led to this discovery in Holland were, *as I have been informed*, as follows." He states that Cuneus accidentally made the experiment in repeating an experiment by Musschenbroek; but he does *not* say, as the Abbé Nollet does, that to Cuneus belongs the credit. As Cuneus never made any reclamation, the inference is that he immediately communicated his experience to Musschenbroek, and that the analysis of the phenomenon was completed by the latter. Perhaps Cuneus did not of himself find out that the shock depended on his holding the bottle in his hand. Mr. Benjamin inclines to disbelieve entirely in any share in the discovery by Cuneus.

Mr. Benjamin is quite wrong in speaking, as in one place he does, as if the use of experimentation as an instrument of discovery was at variance with the Cartesian philosophy. We will also venture to doubt his confident assertion that Sir Kenelm Digby, in his 'Two Treatises, in the one of which the Nature of Bodies, in the other the Nature of Man's Soule is looked into in the way of Immortality,' plagiarizes extensively from the 'Principia' of Descartes. The latter work appeared from the press of L. Elzevir in

Amsterdam on July 10, 1644. Descartes had set out from the Hoef in May for Paris; for the censure (we presume) would not in those days permit "author's corrections" of the proofs. He arrived in Paris at some time between September 27 and October 1, inclusive, and there first received copies of his book. Digby had been in Paris all along. There is evidence that his book (a folio of medium thickness) had been substantially written in the previous spring. The dedication is dated in August. The last imprimatur was affixed September 26. Now, there could hardly have been time for extensive plagiarisms (for every hypothesis, if plagiarized, is modified) between the date at which Digby could have seen the 'Principia' and the date of the imprimatur. Descartes remained in Paris ten or twelve days, during which, though much pressed for time, he had several prolonged interviews with Digby. He never made the least reclamation, though he hinted that Digby was a bold theorist, for he says to the Princess Elizabeth, "Pour ce qui est de l'état de l'âme après cette vie, j'en ay biens moins de connoissance que Monsieur d'Igby." Digby and Descartes never corresponded, and Descartes was a cautious man in the matter of communicating unpublished ideas, while Digby, on the other hand, was a

talker. Finally, although no man ever more widely missed the *style* of Nature than Digby did in his physical hypotheses, yet those hypotheses have a strongly marked style of their own. They have nothing of the flavor of eclecticism. Nor can we admit that any hypothesis of the 'Two Treatises' is so precisely accordant with that of the 'Principia' that it is necessary to attribute them to one author. Digby, by the way, is a better psychologist than physicist. He treats of the association of ideas, and even proposes a physical hypothesis to account for it.

We find it very difficult to let this interesting work go without saying anything more about it. An excellent present for a scientifically minded young person would be Mottelay's translation of Gilbert on the Magnet (Wiley) and Benjamin's 'Intellectual Rise' (Appleton).

62 (9 January 1896) 32-33: ADDITION AND SUBTRACTION

TO THE EDITOR OF THE NATION:

SIR: To your admirable review of 'The Psychology of Number,' by McLellan and Dewey, in the issue of November 28, this addition may be seasonable.

We read of a scholastic discussion as to how many angels could stand on the point of a needle. Suppose that some investigator had decided that the true number was exactly one hundred and eleven, and had recorded this result in a textbook. Suppose that all subsequent text-book-makers had adopted this conclusion, until finally some one thought out and published "The Psychology of the Angelico-humanistic Interrelations, founded on the Number 'One Hundred and Eleven.'" Undoubtedly, we should be interested in it as an instance of mental ingenuity, but should consider that its purely arbitrary foundation rendered it of slight practical value.

As regards one phase, at least, of the work under discussion, the above illustration would be a fair parallel--the phase which treats of "addition" and "subtraction," wherein the authors travel a purely arbitrary path. To illustrate: In the school-room, a child's attention is directed to two groups of blocks, and he is asked to tell the total number in both. Counting those in either group, he goes on counting from that point till he has the sum of both sets. He is now asked to tell the difference between the two sets of blocks. Again he counts from the number in the larger set backwards till be comes to the number in the smaller set, or from the smaller number forward; in either case, finding the same difference. If he wishes to record on paper each of these steps, he arranges the symbols representing the number of blocks in each set in convenient position to aid him in his counting, by custom (not by necessity) one under the other. He still finds sums or differences wholly by counting or by memory of previous countings.

In an evil day of the long ago, some genius determined to call it "addition" when counting totals, "subtraction" when counting differences. Not only was this purely arbitrary, but its effect was to completely obscure and keep out of the arithmetics the real addition and subtraction as we know them in our daily

experience. The child, *e. g.*, could have really added blocks to the place where either group was situated, but he could have done it only by a simultaneous subtraction from some other place. He could have subtracted any block from its place in either set, but he must have added it at once to some other place. Addition and subtraction form inseparable parts of one operation, and the child would have represented this operation on paper in very different fashion from his representation of the artificial "addition" and "subtraction."

In arithmetic, where the so-called "subtraction" is confined to counting from a smaller number to a larger, the mischief was confined to a wrong order of development, and to the suppression of the equation that follows at once the true addition and subtraction. When it came to counting from a larger number to a smaller--calling it "subtracting" a larger number from a smaller--the mischief was complete; for the scholars, at any rate, thought the attending concepts applicable to dollars and other material objects, instead of being purely imaginative. It is unfortunate that these conventional terms, with their affixed conventional meanings, should have been so long followed by the text-books; a careful examination, so far, revealing but one honorable exception, which is worth noting. In a little text-book, published at Exeter, N. H., in 1845, by Z. Jones, principal of Hampton Academy, the common use of the terms "addition" and "subtraction" is noted, but the author studiously avoids them, choosing more exact terms.

It is still more unfortunate that two author of such repute as Messrs. McLellan and Dewey should have incorporated into their 'Psychology' a fundamental arbitrary concept. Had they searched carefully, they would have found it the source of many contradictions and absurdities in our mathematics, and might have changed some of their own psychological conclusions.

WILLIAM D. MACKINTOSH.

CHAUNCY HALL SCHOOL, BOSTON, MASS., Jan. 3, 1896.

62 (9 January 1896) 42: Science and Art Drawing:

Complete Geometrical Course, consisting of Plane and Solid Geometry, Orthographic and Geometric Projection, Projection of Shadows, the Principles of Map Projection, Graphic Arithmetic, and Graphic Statics.

By J. Humphrey Spanton. Macmillan. 1895.

Attributed to Peirce by Fisch in *Second Supplement*. Peirce owned a copy of this book, probably acquired as a review copy since it was not the sort of book he was likely to purchase. Note, also, the mention of map projection and Pythagoras. This piece is unassigned in Haskell's *Index to* The Nation, vol. 1.

The idea of teaching geometry to draughtsmen while their pencils are in their hands is in itself an excellent one, and, were it only well carried out, would seduce them into real mathematical thinking before they knew it. Moreover, some of the subjects here treated bear such stamps of the great geometers who established their theories as it would require a mind of more ingenuity than Mr. Spanton's to obliterate. Descriptive geometry and graphical statics, let the teacher do his worst,

cannot but inculcate some genuine mathematics. With map projection it is different. The whole subject has never been very well treated, except by Herz, whose work is probably unknown to Mr. Spanton; and to call the few items here given "Principles" is ridiculous. The chapters on Elementary Metrical Geometry could not well be worse than they are. Thus, for the construction of a regular heptagon, three different methods are given. For one of these, the information is vouchsafed that it is not mathematically exact. That the problem itself is insoluble by rule and compass, the author, though a gold-medallist, does not seem to suspect. The Pythagorean proposition, to say nothing of such theorems as the 35th of the 3d Book of Euclid, will be sought in vain. The problems that are solved rightly are often solved clumsily. Let us say to the young draughtsman, If you want to be a master of your art, take the trouble to study geometry. You will be terribly handicapped in problems upon which bread and butter depend if you content yourself with any such smattering as this book affords.

62 (6 February 1896) 122: NOTES

Attributed to Peirce by Fisch in *Third Supplement*. Peirce had received a copy of this book from Halsted (see MS L 181, Halsted to Peirce, 17 December 1895). There is also a card in the Halsted correspondence (MS L 181) that is unaddressed and is in Peirce's hand. There Peirce asks for a prompt response to a query concerning the foundation for a statement that Bolyai was once insane. Peirce had received the first edition of Halsted's translation of Bolyai (see MS L 181, Halsted to Peirce, 15 January 1892). This item is unassigned in Haskell's *Index to* The Nation, vol. 1.

--The fourth edition of Dr. George Bruce Halsted's translation of Bolyai's 'Absolute Science of Space' (The Neomon, 2407 Guadalupe Street, Austin, Texas), is enriched with many interesting particulars about the lives of the celebrated author of the Non-Euclidean Geometry, Bolyai János, and of his father, Bolyai Furkas. If we admit that there is any natural and important distinction between men's mental constitutions corresponding to the words genius and talent--if the man of genius is anything more than a man of high talent, *plus* a bold, adventurous spirit--then the father must be ranked high up on the list of men of talent; and not the smallest proof of this was his instant appreciation of that discovery of his son's which superseded his own principal life-work. The son, on the same system of parcelling, must be called a genius, though, being a man of one idea (for he survived his one revelation by thirty-seven years without any other remarkable achievement), he cannot be rated as an exalted genius. He inherited a valuable imaginative element from his mother. Lombroso sets him down as insane; but we find nothing in Dr. Halsted's present account to support that charge, unless it be the circumstance of his fighting thirteen duels the same day with as many cavalry officers, playing on the violin between every two successive duels, and getting cashiered for the performance. Dr. Halsted surmises a psychological connection between the muscular precision of the man, as fencer and violinist, and his mathematical precision. Even in this day of hardy psychological classifications, such a guess startles us. It is stated quite in the Lombroso-Nordau style of assurance. Would the muscular strength exhibited in the thirteen duels be connected with his mathematical strength? There is a winningly enthusiastic letter from Bolyai János to his father, telling him of the great step. He says: "I have discovered such magnificent things that I am myself astonished at them. It would be damage eternal if they were lost. When you see them, my father, you will yourself acknowledge it. At present I cannot say more than that from nothing I have created a wholly new world." Dr. Halsted announces a life of Bolyai from unused Magyar documents. Our countryman as little shrinks from the Magyar tongue as from the Russian, in the pursuit of his valuable researches.

62 (13 February 1896) 147: **Molecules and the Molecular Theory of Matter**.

By A. D. Risteen. Boston: Ginn & Co. 1895. Pp. 223.

CSP, identification: MS 1405 (draft). See also: Fisch, *First Supplement*.

Allan Douglas Risteen was associated with the United States Coast and Geodetic Survey, first in New Hampshire and then in New York City, as assistant to Peirce, with whom he made pendulum observations at the Stevens Institute of Technology and in Washington, D.C. He was graduated B.S. from Worcester Polytechnic Institute in 1885 and Ph.D. from Yale in 1903. After his work with the Coast Survey, Risteen became director of technical research for the Travelers' Insurance Company of Hartford. He made numerous contributions to scientific literature in the form of papers on astronomy, safety engineering, and physics. Risteen's *Molecules and the Molecular Theory of Matter*, reviewed here by Peirce, was widely used as a college textbook.

Mr. Risteen's object is to give, in elementary form, a complete and connected account of what is known of the constitution of matter. Such a book has long been wanting, for a very good reason--namely, that there are few physicists who are not painfully aware how far they fall short of competence to produce such a treatise. In the main, Mr. Risteen has done very well. He has taken account of almost all the greater contributions, mathematical and experimental; he has so put them together as to render his pages intensely interesting, by virtue of the thread of cunning reasoning and apposite observation that surely leads to the heart of the great puzzles which he follows out; and he argues some points with real power. The work will prove extremely useful to all who wish to know what the scientific theory of molecules is in detail, and what are the grounds upon which it rests.

The great memoir of Helmholtz upon the conservation of force assumes that all material forces are between pairs of particles--in short, are attractions and repulsions. But measurements upon the elasticity of bodies have thrown grave doubts upon that assumption; and some writers upon elasticity profess to demonstrate that the forces between the parts of solids cannot be of that description. In reprinting his memoir, Helmholtz undertook to modify his expressions, so as to give room for the modern doctrine; but such modifications leave his arguments without much force, and deprive the theory itself of the greater part of its significance. It is on account of those observed facts about the elasticity of solids that

Kelvin invariably expresses himself with reserve about molecules--saying that he believes that matter "has some kind of grained structure." It is not too much to say that this question is the principal question of to-day in natural philosophy. If central forces will suffice, so that the conservation of energy is to retain its full meaning, then the Boscovitchian conception (it ought not to be called a hypothesis) is the only rational way of thinking. But if central forces will not suffice, we are driven, it would appear, to conceive of matter as continuous, and therefore as a fluid in some respects homogeneous, throughout space. Thus we come to that order of ideas about media for the action of forces, the attraction of force-lines, etc., which have marked the physics of Great Britain since the time of Faraday. Here we find a rational motive for the vortex theory of atoms. Something of this great discussion might well have been allowed to appear in the introductory chapter of a work on the constitution of matter; but Mr. Risteen finds no place between his covers for any portion of it. Though he touches upon crystals, he never speaks of any doubts as to the sufficiency of central forces. He never mentions the name of Boscovitch. He speaks of the vortex theory, but does not show in what its real peculiarities consist, nor where the suggestion really came from.

The kinetical theory of gases, which now begins to take on the highest degree of certitude and something like completeness, is very well elucidated in Mr. Risteen's second chapter; yet we are amazed that the vast researches of Amagat should be passed by without mention (except that one constant is borrowed from him).

In the molecular theory of liquids nothing is said, either *pro* or *con*, in regard to the theorem of the virial of Clausius, which, it seems to us, ought to be the cynosure to guide our speculations upon this subject. In one passage we are said to be ignorant what the quadratic mean of the molecular translational velocities in a liquid may be at a given temperature; in another place it would seem to be assumed that the velocities in liquids and solids are less than in the gases of the same constitution at the same temperature. If the theorem of the virial is true, this question is easily answered; if it is not admitted, the objections to it ought to be stated. A strong attraction between the molecules of a liquid is manifest in its surface-tension, its heat of vaporization, etc. Its definite density is an effect of equilibrium between this attraction and the translational velocities of the molecules. It would thus seem to be evident that the velocities of molecules in the liquid cannot be less than they are in its saturated vapor above it. Mr. Risteen very promisingly commences an explanation of the incompressibility of liquids, by attributing it to the centrifugal force of the molecules. No doubt he is right, as far as he goes; but a more precise elucidation is desirable.

The molecular theory of solids appears to be beyond Mr. Risteen's present powers. At all events, he has not entered into the considerations which are prerequisite to any serious attempt at an outline explanation of the properties of these bodies.

In a chapter on the size of molecules, the author calls attention to the extreme vagueness of the idea of the size of a molecule. One might as well attempt to

measure in inches the diameter of a crowd of people before a street show. It has no definite limits. We measure the length of a bar, because if we attempt to compress it we meet with a counter-pressure which, before we have sensibly reduced its length, exceeds any force we can bring to bear upon it. But it is not likely that molecules have this property to anything like the same degree. When we speak of their size we do not know what we mean; and one method of determination might perfectly well give one result, and another a widely different result, and yet both might, in their several senses, be correct. It is, therefore, a very remarkable fact that different calculations of the size of molecules based upon the most widely diverse considerations turn out to agree very well. Nobody ever supposed that in asking how large a molecule was, he was asking anything much more definite than it he had asked what the average size of an ordinary portable object is. The answer in the latter case might be, its size is somewhere from a fraction of an inch to a few yards. The size of molecules seems to be known quite as definitely. The diameter is somewhere about a ten-millionth or hundred-millionth of an inch.

A final chapter is devoted to speculations in regard to the constitution of molecules. Mr. Risteen defends very ingeniously the equation by which the number of "degrees of freedom" of a molecule is supposed to be determined. He has, on the whole, proved that he has the power to produce a treatise upon the subject adequate to the needs of students; and if the weak spots of his first essay receive the necessary attention, we may hope that a perfected edition will meet every desideratum.

62 (26 March 1896) 261-262: Mind and Motion, and Monism.

By the late George John Romanes. Longmans. 1895. Pp. 170.

CSP, identification: MSS 1404(s), 1406 (drafts). See also: Burks, *Bibliography*.

George John Romanes (1848-1894) was Canadian-born and educated at Gonville and Caius College, Cambridge. He formed a strong friendship with Charles Darwin and, due to his influence, turned his studies toward physiology. Romanes' theories caused a stir in academic circles when in 1886 he published a paper propounding the theory of physiological isolation, dealing with the possible evolution of a distinct new species from an isolated group of an original species.

When Mr. Romanes began this book entitled 'Monism' (to which a lecture on 'Mind and Motion' is prefixed) by saying that it is established to the satisfaction of every physiologist that there is an absolutely exact correspondence between every mental fact and some concomitant fact of the brain, he exaggerated. There are physiologists enough who regard the correspondence, whether absolutely exact or not, as limited to feeling and sensation corresponding to excitation of nerve-cells, and to volition corresponding to nervous discharges, while maintaining that there are in the mind general ideas which correspond only to potentialities in the brain, not to any actual facts. However, having put out of court all who do not pin their faith to the invariability and exactitude of the

correspondence between mental and material events, Mr. Romanes proceeded at once to divide believers in that proposition into Spiritualists, Materialists, and Monists, thus furnishing the last word with one signification the more. Monism originally meant the doctrine that mental phenomena and material phenomena have one substratum; and monism was said to have three forms, Idealism, or the doctrine that material phenomena are but a species of ideas; Materialism, or the doctrine that mental phenomena are merely a special variety of those facts which lie at the bottom of material phenomena; and Neutral Monism, which was described as the doctrine that material phenomena and mental phenomena are equally universal, and merely different aspects of any facts. The monism of Mr. Romanes seems to be a variety either of materialism or of this neutral monism; for he says, in the introductory essay, that mind and motion are substantially identical. Thus, of the three elements which compose the physical universe, to wit, matter (or inertia and identity), motion, and energy, he holds that one is coextensive with mind. In the old triad, he has displaced Idealism to make way for Spiritualism, which was always held, and which he himself held, to be a dualistic and, therefore, not a monistic doctrine, though as monistic he classes it. But he does not mean spiritualism in general; for of spiritualists and others who do not accept his first axiom of the absolute perfection of the correspondence between mental and cerebral events, he takes no notice whatsoever. Upon this point he is explicit (p. 42).

What Mr. Romanes wishes to prove is, that the hypothesis that all material motion has a feeling, and *vice versa*, besides accounting for sufficient facts to render it reasonable, leads to the proposition that all "causality" (could not this antiquated notion have been replaced by something more scientific?) is, on its inside, volition, and gives room for, as he at first says, but subsequently (for he never gave the work the revision necessary to make its doctrine quite consistent) that it "sanctions" and almost necessitates, the assumption of a universal mind of the world (which he calls Theism), and, finally, that it it reinstates the freedom of the will, and, with that, moral responsibility. Many readers will seem to see in the book the phenomenon of a man setting out from materialistic assumptions, but led, under the influence of a broad study of nature, toward idealistic conclusions, and going, at last, so far as to say that the ultimate reality is "either mental or something greater." Others will say, with some justice, that it is the work of an invalid, so weak that pages are occupied with reasonings and logical diagrams to show that a universal affirmative proposition cannot be converted *simpliciter*, and with another diagram altogether worthy of Dr. Fludd (except that it is a rough woodcut, instead of a beautiful copper-plate), and full of the most puerile propositions. The style, however, is as strong and clear as anything Romanes ever wrote, if not more so. That, if he had recovered from his illness, he would, by this time, have been advocating an idealistic theory of the evolution of all things, including the laws of causation, there is hardly room to doubt. Such is the theory that the great advocate of Darwinian ideas would inevitably have adopted as the fittest survivor in the struggle of theories.

62 (23 April 1896) 330-331: Algebra und Logik der Relative, der Vorlesungen über die Algebra der Logik.

Von Dr. Ernst Schroeder. Leipzig: Teubner. 1895. Vol. I., Part I. 8vo, pp. 649.

CSP, identification: MSS L 159.76, L 159.87; MS 1406a (draft). See also: Burks, *Bibliography*.

Schröder's great treatise on deductive logic, the most extensive that has ever been written, cannot well be neglected in Germany; and it is hard to imagine how any person who has been through the work can ever be again guilty of such logical absurdities as have been scattered hitherto through the very best of German textbooks. Everything, or almost everything, so far written about the logic of relatives has made use of some kind of technical algebra. The result has been to convey the idea that the logic of relations is an exceedingly specialized branch of logic. This is not true. At least, those who cultivate it maintain that it is much more general than ordinary logic. They hold, too, that our ordinary reasonings, so far as they are deductive, are not, in the main, such syllogisms as the books have taught, but are just such inferences as are particularly dealt with in this new branch of logic.

To make this plain, they point to the fact that the old syllogistic inference can be worked by machinery, but characteristic relative inferences cannot be performed by any mere mechanical rule whatever. Alike in the forms of inference which they have added to logic, and in the old syllogism, the relativists trace the following steps: first, the choice of premises, and second, the bringing together, or colligation, of the premises chosen, and the union of them in one conjunctive proposition. They show that, even in non-relative logic, there are occasional cases in which there are different ways of connecting premises: and, in the logic of relatives, the ways are simply innumerable, for it makes a difference *how often one and the same proposition is taken as a premise*. This being the case, it is plain that a machine cannot indicate *the* conclusion from given premises, since the number of such conclusions is endless. The different premises having been united into one, this one is subjected to certain inferential transformations, which in the case of ordinary syllogism can be analyzed into two steps. Following upon these, there is a substitution of a "term of second intention," or logical conception, for an ordinary conception of experience; and, finally, this logical term is removed. At every step of this there are different courses which reason may pursue; so that the conduct of the reasoning far transcends the powers of any machine. Nor can our ordinary procedure in thinking possibly be mapped out in advance by turning the crank of a machine.

We will not find fault with Dr. Schröder for devoting his own researches to the solution of problems which American thinkers had put aside as of inferior interest, on account of their special and technical character; for every inquirer should follow his own bent. Besides, it is extremely useful to place within reach of German philosophers a work which may train them to a really precise logic. We repeat that it would be needless to fear that the work will be passed over in neglect and silence. To affect to treat such a treatise with contempt would, in Germany, expose any man who might attempt it to severe blame. It cannot, therefore, but prove a useful book.

Another "Abteilung" of it still remains to appear, although nearly 2,000 large octavo pages are already before the public; and we may hope that, in that concluding part, Dr. Schröder may yet show how some of those who have laid the foundations of this method of studying logic, conceive that it ought to modify those general notions about reasoning and other mental processes which are expressed or implied in the hurried talk of the street, and leave their traces upon all our thought, and also how it ought to modify our general philosophical conceptions--conceptions based far more upon logical analysis than upon anything else.

As this is a branch of study in which American students have done more than their share of the work, our readers may like a slight hint of what the nature of the new light is supposed to be. First, what is the Logic of Relatives? It is a subject treated in all the more complete mediæval handbooks, and hinted at by Aristotle. But it was Robert Leslie Ellis, the editor of Bacon's philosophical works, who first got some idea of how it ought to appear in a modern shape. Namely, instead of analyzing a proposition into subject and predicate, it analyzes it into subject, predicate, and objects--which last it conceives as so many additional subjects. In 1858 Augustus De Morgan published a long memoir on the subject, in which, besides establishing many important truths, he clearly showed that, instead of being a special branch of logic, it is, in fact, a great generalization of the old conceptions. In 1870 appeared the first of a series of contributions by an American writer, Mr. C. S. Peirce, one of which forms the acknowledged basis of the present volume by Dr. Schröder, who, however, has remodelled the whole and made extensive additions. Other Americans have materially advanced the subject, expecially [[sic]] Prof. O. C. Mitchell of Marietta, to whose work both Dr. Schröder and Mr. Peirce attach a high value. Students all over Europe have done good work, most of them following more or less closely the methods of Peirce. Mr. A. B. Kempe, however, formerly President of the London Mathematical Society, in an important memoir in the Philosophical Transactions, has struck out an original path.

The first general notion of logic which becomes profoundly modified by the study of relatives, is that of deductive reasoning itself, which the old logic represents to be something purely abstract, intellectual, and virtually mechanical. The new school not only declare that deduction is regulated by choice and a deliberate plan, but, further, that it reaches its conclusions by observation; in fact, they hold that it differs from inductive reasoning mainly in this, that it observes objects of our own creation--imaginary or graphical--

instead of objects over which we have relatively little control. This doctrine is not unlike Mill's analysis of the "pons asinorum." It is a two-edged weapon, cutting both of the great philosophical doctrines pretty seriously.

Another common notion of a logical kind which is strangely transformed by the new views is that of generalization. The generalization of the books is, for the Relativists, merely the simplest and least important variety of a process which we will refrain from defining, but of which an example is the passage of thought of the geometer by which he comes to conceive that a straight line returns into itself.

62 (21 May 1896) 404: **The Number Concept; Its Origin and Development**.

By Levi Leonard Conant. Macmillan. 1896.

CSP, identification: Haskell, *Index to* The Nation. See also: Burks, *Bibliography*; Fisch and Haskell, *Additions to Cohen's Bibliography*; MSS L 159.83-84.

This volume is made up of tables of the numerals of a great many (perhaps 500) different languages, with a slight connective commentary, drawing attention to the signification and composition of the words. The shortcomings of the work are numerous and regrettable, though by no means fatal; its merits are few and simple, but considerable.

The title is a misnomer, and the author shows that his own number-concept is in a low stage of development. Numerals are not themselves concepts at all, nor do they signify concepts. They are simply a scale of vocables, which we use very much as we use a foot-rule. We apply them to a multitude, and mark how far on the scale that multitude will go. In explaining this, we explain what the number concept really is: it is the intelligent conception of the purpose and method of the system of numerals. It is entirely unnecessary that this should, in the form of a concept, or intellectual product, be in the minds of those who use numerals. It is sufficient that they should know by experience that counting is somehow useful, that it aids bargaining, etc., and that they should be habituated to the use of a series of words in counting. The continual use of the word "concept," instead of speaking of "words" or "terms" and their "significations," is a German way of speaking, very inferior, both in logical accuracy and in perspicuity, to our English idiom. At any rate, the real subject of this book is numerals and their modes of formation.

Very little is said of the number-concept (which is really of very late development), nor of the idea which the tribes mentioned may entertain in regard to number in general; and what little is said is not worthy of criticism. Not only does the author fail to discriminate the number-concept from the use of numerals, but he also falls into a confusion of thought which must greatly embarrass his mathematical pedogogy, namely, a confusion between *number*, in the sense of the result of counting, and *multitude*. He tells us that all tribes "show some familiarity with the number-concept." Yet he mentions Bolivian tribes which are said to have no numerals whatever. Still, he says they show "a conception" of the difference between *one* and *many*. In another place, he says that the "number-concept" of ordinary people is imperfect, in that they have little sense of the different degrees of multitudinousness of high numbers. On the contrary, this has nothing to do with the accuracy of their "number-concept," or of their power of applying numerals to the purpose for which they were invented. It is true that to the mind trained in certain branches of applied mathematics the word "trillion" carries associations of rigid statistical uniformity which the word "million" lacks. Such a mind may be said to attach different conceptions to the two words; and the distinction is useful to such a mind. But this has nothing to do with the use of numbers as numbers. The person considered will put all that out of his mind when he has any definite numbers to deal with, and will perform his arithmetical calculations just like anybody else. A system of numerals is an apparatus for counting. Those numerous tribes which have names only for *one*, *two*, and *three*, which express four by *two twos*, five by *two* and *three*, etc., evidently did not count at the time their language was formed, and probably do not count now. They, like all men, recognize pairs and triplets by their configurations, fours as pairs of pairs, etc. The so-called numerals of such tribes are, properly speaking, not numerals at all. When a tribe has a numeral system based upon *five*, *ten*, or *twenty*, the evidence is that they possess the art of counting. They are quite prepared to count indefinitely as soon as they can count at all, provided they have the power, possessed by most savages, of unconsciously coining a name as soon as they need it. The limits of their numeral words mark the limits of their need of such words.

From a philological point of view, the execution of the book is slovenly. The author copies the various transcriptions of the writings from which he has compiled the lists, without explanation, and omitting all diacritical marks. We do not know whether *c* is to be pronounced *k* or *sh* or *tsh* or *th* or *dh*, whether *g* represents the German guttural *ch* or the velar *k*, whether *x* stands for *ks*, for *h*, or for the Arabic *ghain*, whether *j* has the English, French, German or Spanish sound, etc. When we remember that the English word *fox*, pronounced by a Cherokee, and transliterated according to a recognized system, but with the diacritical marks removed, appears as *kwagisi*, we see that, for the purposes of comparison of languages, this book presents nothing but an imperfect list of references. There is little notice of Semitic numerals, none of the Egyptian, and scarcely any of the

Babylonian. There is no mention of the so-called Chaldean names for the Arabic figures found in Latin twelfth-century works. There is no classification by races; but North American and African languages, the furthest remote from one another in their spirit of any of the tongues of men, are shovelled in together. Of many minor faults we take no notice.

The merits of the work are that it exhibits all the modes of formation of numerals, that it shows the universality of the bases 5, 10, 20, and the non-existence of any true binary scale or any use of 6 or 11 as a base, that it affords evidence that many tribes do not count, and consequently have no proper numerical system, and that there are the greatest differences in the arithmetical capacity of races equally barbarous.

63 (23 July 1896) 71-72: KÜLPE'S OUTLINES OF PSYCHOLOGY

Outlines of Psychology, Based upon the Results of Experimental Investigation.

By Oswald Külpe. Translated from the German by Edward Bradford Tichener.

London: Swan Sonnenschein & Co.; New York: Macmillan & Co.

Attributed to Peirce in *Annual Literary Index* for 1896. See also: Fisch, *First Supplement*. This review is unassigned in Haskell's *Index to* The Nation, vol. 1.

This work is dedicated "To my revered teacher Wilhelm Wundt," and it pursues, in general, the method of Wundt. The translation is exact, but too literal. The sentences are not sufficiently broken up; and inaccuracies of expression, customary enough in German, are reproduced, which will be serious obscurities to the English reader. Thus, the effect of the translation upon the mind of the English reader is not so close to the effect of the original upon the German reader as it would be if literalness had been somewhat sacrificed to English habits of thought. Psychology is now, of course, one of the inductive sciences; and the present work is not a popular presentation, but a higher treatise on the science. It is not an encyclopædic handbook. It is, as its title describes it, an outline account, yet it is by no means an elementary book; and it has 450-odd pages, of 42 lines to the page and 10 words to the line. It has the usual qualities, good and bad, of a German treatise. It is scientific, earnestly bent upon the ascertainment of the truth, in the general current of scientific research, painstaking. Let us examine a section or two, which shall be fair samples of the whole.

The subchapter on the quality of visual sensations begins with a section (§ 17) which we are told at its opening is to treat of the relations existing between sensation and the various forms of light stimulation. But when we come to read it, we find that it also treats of the relations between the different sensations, and of other matters. Meantime the two most striking relations of the kind proposed to be treated--namely, first, that two lights which, when unmixed, match to the eye, also have precisely the same effect in mixtures, and, second, that a three-dimensional continuum is adequate to represent the relations of lights, in so far as they appear differently to the eye--are neither of them stated. The first assertion in the section, after delineating the subject of it so falsely, is that "the velocity of the individual waves is very different in different cases." This vague German way of speaking of "different cases" ought in a translation to give way to the more definite style to which the English reader is accustomed, and be rendered, "for different parts of the spectrum." But, of course, the statement is not true. No doubt, the author was thinking of the *frequency of oscillations*, which is not a velocity at all. He next goes on to say that the sensation of brightness varies from the deepest black to the most brilliant white. This is a capital error. The color of brightness is not white, but intense yellow. Whoever is capable of making an observation of what he sees (as a psychologist, at any rate, ought to be able to do), and who compares the color of a piece of white paper where direct sunlight falls upon it with the color of the rest of the paper illuminated by irregular reflection from white walls, will find the bright part to be yellow; and exact experiment will show that the color of brightness is a yellow, differing little from, though somewhat more chromatic than, the D line. To mistake white for the color of brightness is to involve the subject in a terrible snarl.

Light is absurdly divided into *mixed* and *homogeneous*. Mixed light is defined as that "in which no particular wave-length has a noticeable predominance," so that it might be of any color; but we soon find the author means white light. Then comes the following: "We reach two conclusions, then: (1) that brightness qualities do not possess intensity as a separate, variable attribute; and (2) that an analysis

of the qualities contained in mixed light requires their spatial or temporal isolation." Observe what this means. It is, first, that if we take a certain colored surface (for whether it be white or not, makes no difference) and merely vary its illumination, we shall vary it in only one respect. Perhaps the author means that the sensation will have the same hue with all intensities; but that is not true. And, second it is gravely stated that to analyze the "qualities" it is necessary to separate their constituents. This may mean that we cannot analyze them by attending to one and neglecting the rest; but (not to speak of the inappropriateness of the remark in a section on sensation pure and simple), since the sensation itself is incomplex, the author might as well remark that nobody can analyze water by attending to the hydrogen and overlooking the oxygen. In short, the conclusion introduced so formally is as good as meaningless.

Külpe then goes on to homogeneous light. "Homogeneous light, in the strictest sense of the term, has never been seen, so that we cannot say how it would be lensed." This is not true. The parts of any good spectrum of considerable dispersion, say from half-a-dozen flint-glass 60º prisms, are sensibly homogeneous. But perhaps what our author is thinking of may be that on Young's hypothesis, according to which there are three fundamental sensations--red, green, and violet--the pure green is certainly not seen *by a normal eye*, and probably not the red either; so that we cannot *exactly* imagine how they would look, Külpe then goes on to say, "We thus have the curious result that what we call a color is really a connection of two simple qualities, color-tone and brightness." Now, in the first place, there are three, and not two elements only. They may be stated as *hue*, *chroma* (or saturation-intensity), and *illumination*; or they may be stated as *red*, *green*, and *violet*; or in any one of innumerable other ways. Külpe has given no proof that any one of these methods of analysis is more or less true than any other, certainly not that the particular method he imperfectly describes is the only true one. He goes on to talk of sensations as if they were so many distinct entities, as contradistinguished from coordinates of a continuum, without the slightest pretence to any proof of such a conception. Then he makes this formal statement: "A mixture of two mixed lights produces on the physical side a mean intensity, and on the psychological a brightness sensation lying midway between the two primary sensations." This is simply absurd. Do two lights shining on a piece of paper illuminate it less than the brighter light alone would do? He refers to the result of aggregating the lights and then halving the energy. But even that is very false "on the psychological side." There is no use of following further this sadly confused, vague, and somewhat inaccurate description. As Külpe proceeds, it becomes worse and worse, and the section on "Theories of Visual Sensation," besides overlooking entirely the theory of Mrs. Franklin, discusses the other three weakly, and without understanding what real arguments can be brought forward.

Let us, by way of another sampling, see how the limen is defined. "The just noticeable stimulus is technically termed the *stimulus limen*, and the just noticeable stimulus-difference the *difference limen*." Now the stimulus limen is not a question of *noticing* at all. It is the limit between what is *sensed* and what is *not sensed*. As for the "difference limen," if there is any such thing, it is not what *can* be just

noticed, but what the subject makes just sufficient effort to notice, and nothing more.

Let us next assay the chapter on Association. Here we find matter of real, though mainly negative, value. The first remark is, that the common assertion of the English school that ideas differ from sensations only in being fainter "has never been demonstrated." Certainly, if it be meant that remembering the sight of a Jacqueminot rose is indistinguishable from seeing a tea rose, it has not only not been demonstrated, but we are in possession of all the facts needed to explode it for ever. It may be doubted whether any English writer has ever distinctly stated that there are two sorts of intensities to sensations--the objective intensity of the sensation, and the subjective intensity of the percept, or height of consciousness, insistency of presence, and overriding of other ideas. Külpe describes some experiments instituted by himself to ascertain whether ordinary persons could, in their normal waking state, mistake fancies for things seen or the reverse. He finds that when, in a perfectly dark room, a light very close to the stimulus limen is thrown upon a screen, most persons, though not all, occasionally, but rarely, think that it is only fancy and that they do not really see it; and that the same persons more often fancy they see a light when there is none. Some persons never have such fancies. This accords with ordinary experience so entirely that we might be tempted to declare the experimentation futile. Yet whoever will actually make experiments in the general line of Külpe's will find them instructive. If they are so made that they can be subjected to mathematical analysis, it will be found that there is a subjective intensity (whatever that consists in) which, in ordinary persons, for sensations well above the limen, is vastly greater than for any ideas, but which depends besides upon the objective character of the idea, upon the effort of attention, upon the strength of the association, etc. Persons there are whose ideas so closely resemble sensations that they can distinguish some of them as unreal only by their being more influenced by efforts of attention. Thus, some artists speak of copying upon the canvas the imaginary pictures which they see by the side of their easels, and will request a person who stands where the image is to stand aside. Composers with a genius for instrumentation do something still more surprising when they combine the sounds of different instruments in their mind's ear and experiment upon the effects so produced. Other persons, as Külpe has found, have no memorial or imaginative ideas which seem to them at all comparable with sensations. It is remarkable that those persons have not necessarily bad memories for sensations. The writer of this notice, until past middle life, could not seem to call up in his mind anything in the least like a color; but he has since acquired the power in a slight degree. Yet he always had an unusually accurate memory for colors. Külpe divides recognition into the *direct* and the *indirect*. The former is that in which the feeling "I am acquainted with that," or "That is what I am after," appears directly as soon as the right object is presented to sense. Indirect recognition is recognition by means of a comparison with an image carried in the memory. Külpe remarks that the existence of direct recognition is proved by certain diseases of the brain. Thus, a patient has been known to describe a fork accurately, and yet when a fork was put into her hands she declared she was familiar with it, but did not know what it was. Now her not recognizing it as a fork shows that a satisfactory image was not called up by the definition of the fork; and yet the feeling of being acquainted with it was there just the same.

It appears, then, that the merit of this work is unequal. Sections which are exceedingly instructive will be found sandwiched with others upon which not sufficient study seems to have been bestowed.

63 (3 September 1896) 181-182: Elements of the Theory of Functions of a Complex Variable, with especial reference to the Methods of Riemann.

By Dr. H. Durège. Authorized translation from the fourth German edition by

George Egbert Fisher and Isaac J. Schwatt. Philadelphia. Published by the

Translators. 1896.

CSP, identification: MS L 159.84; MS 1407 (draft). See also: Burks, *Bibliography*.

Three years ago we had not a treatise in our language upon that subject to which the attention of most of the strongest mathematicians has been chiefly and increasingly devoted for now near a generation. Two admirable and extended manuals (neither of them, however, exhaustive) have since appeared; and in this publication the strictest essentials of the theory are presented in an easily comprehensible form, free from unnecessary details and subtleties. The translators may be sure that their work will find welcome from teachers. Two classes of students are likely to use the book. Even to a man who does not intend to pursue mathematics, and to whom any training in working algebraical operations or those of the calculus would be time thrown away, the theory of functions may prove interesting. It will give him a tourist's glimpse, at least of a new world of thought. It will show him that, scattered about in the community, are men of our race, in ordinary costume, some of them transacting affairs in down-town offices, just as any man might do, who are yet filled with ideas far more foreign to the rest of us than are those of the Chinese in Mott Street. Surely, it were consonant with a generous education to gain some glimpses of the mathematician's world, even if there were no lessons to be learned from it but that of a phenomenon in anthropology. But when we consider that the very finest and most prolific deductive logic can scarcely be learned in any other quarter, and that the life-thought of these who wield that logic cannot fail to afford some useful hint towards a solution of the problem of what sentiments we ought rationally to entertain towards matters and things in general, it becomes all the more plainly worth the while, even of the {ageömetrëtos}, to look into the easy and pretty elements of the theory of functions. The first class of students, then, who are likely to want this book consists of those who do not mean to go deeply into mathematics.

The other class consists of those who purpose subsequently to attack the large treatises, perhaps even the memoirs, but who desire a preliminary introduction to the subject. For these last, supposing they already have some ordinary acquaintance

with the calculus, it is hard to see how a book could be better suited than the treatise of Durège. It presents the leading ideas so that the student can see how they are applied; and when he has read its pages, he will feel quite confident that he understands at least the general theory of functions, if he be not even ready to handle it himself. At the same time, students who have learned their calculus out of a modern treatise have no need of a separate preliminary work upon the theory of functions. They are already in possession of its governing ideas, and might as well pass at once to more elaborate treatises. For students of all kinds the best way to teach the calculus is to make it a part of the doctrine of functions.

Decidedly, for mere tourists into the world of mathematics, a minuter guidebook than the present work would be preferable, were there one that was not too difficult. One of the chief uses of the study for such a student lies in the accurate yet productive logic it inculcates. Of late years a great deal of research has been devoted to the exact analysis of the conceptions of the theory of functions, such as infinity, continuity, differentiability, etc. The science of logic has been advanced, in no small measure, by those studies. They ought not to be neglected by any close thinker, nor does their exposition necessarily involve difficult technicalities; but of all these recent investigations Durège might, for all his book shows, have heard nothing. His aim, not to say his boast, seems to be that he shall present the theory just as it appeared to the thought of Riemann, sixty years ago. He discourses at great length of the topological characters of Riemann's surfaces; but he never points out, perhaps is scarcely aware, that the connectivity of space, as it is imagined to be in the theory of functions (and as Listing also conceives it), is essentially different from that which is attributed to it in modern geometry. For example, according to the theory of functions, upon a plane surface four colors suffice to paint a map, while, according to projective geometry, six colors are requisite. For the student of ideas, the development side by side in mathematics of two disparate notions of space, and that, too, without giving rise to any dispute, is surely of sufficient interest to be noticed. Again, the logic of the interchange of roots in making a circuit round a branch-point is passed over with all the naïveté of Cauchy, who first remarked the phenomenon. Cauchy was a miraculous algebrist and a great logician, but it is notorious that the feet of his agile logic are rather apt to trip. In this matter he skips over a cañon so easily and so lightly that one can hardly make out how he does it, even if one remarks that any cañon is skipped.

The publication will be well received, as it deserves to be. Yet the translation appears to be of most rudimentary literalness. We say "appears," because we have not made comparison with the original, and also because in sundry places we have been puzzled to guess what the German words could be. Thus, on p. 46, in an italicized sentence, "other" is used where "second" or "additional" would be much clearer; one cannot think the original reads "andere." "This" is frequently used where the English idiom calls for "that." In perusing the pages we are continually coming across such sentences as the following: "By this also the name *modulus of periodicity* is justified, since we can say, analogously to the language of the theory of numbers, that z acquires equal values for such values of *w*

as are congruent to one another to a modulus of periodicity." Such language is not English. In many passages it is not even good German, unless logically confused expression can be said to be supported by such a weight of usage in Germany as to make good sense kick the beam.

1897

64 (25 March 1897) 227: NOTES

CSP, identification: MS L 159.63. Halsted in *Science* [new series 5 (16 April 1897) 597-604] criticizes Peirce for an alleged error in this note. See also: Burks, *Bibliography*; Fisch and Haskell, *Additions to Cohen's Bibliography*.

--Very few mathematicians of the strength and originality of the late James Joseph Sylvester, who died in London on March 15, have ever lived. There have been great analysts whose secret was a symbolical method--it was, so to speak, their little game, by which they made problems of a certain class easy; others have accomplished great things by turning problems into geometrical shape; others have carefully avoided problems that were not adapted to their peculiar powers; but Sylvester seemed ready to attack any problem, provided only it was difficult--even problems in geometry, for which he was wanting in the peculiar knack that some men have; he never employed symbolical methods, but seemed to create a method specially adapted to each problem he took up. Perhaps he was not, on the whole, a mathematician of the greatest kind; but for naked logical strength but two or three have ever equalled him. He was of Jewish extraction, and was born September 3, 1814, in London. In 1837 he graduated from Johns, Cambridge, being second senior wrangler. After an inappropriate appointment to the professorship of natural philosophy in the University of London, he accepted a professorship in the University of Virginia, where he stayed less than a year, however. Other experiments, in England, were equally uncongenial or unfruitful. In 1876, at the instance of Peirce, he was called to the Johns Hopkins University. He accepted with much diffidence, for he always said he had not much mathematical reading. Nevertheless, his occupancy of the chair proved a glorious success, and a school of enthusiastic and very able young mathematicians grew up under his guidance. His students were always introduced to the matter that was glowing upon the anvil of his own workshop, and so learned how to make researches. In this way he conferred upon this country an inestimable benefit. He established here the *American Journal of Mathematics*, which continues to occupy a more than respectable position among journals of discovery. In December, 1883, he was elected Savilian professor of geometry in the University of Oxford, and thereupon returned to his native land, where, at length occupying a position such as ought to have been imposed upon him forty-five years earlier, he immediately began to stimulate the development of mathematics as he had done here.

65 (4 November 1897) 362-363: **Studies in Psychical Research**.

By Frank Podmore, M.A. G. P. Putnam's Sons. 1897. 8vo, pp. 458.

CSP, identification: Haskell, *Index to* The Nation. See also: Burks, *Bibliography*; Fisch and Haskell, *Additions to Cohen's Bibliography*.

Frank Podmore (1855-1910) was graduated B.A. from Pembroke College, Oxford, in 1877. He was active in the study of psychic phenomena, serving from 1882 until 1909 as a member of the council of the Society for Psychical Research. Podmore helped found, and originated the title of, the Fabian Society.

This pleasant volume may be recommended to those who desire to inform themselves concerning the progress of psychical research since the publication of 'Phantasms of the Living.' It must be acknowledged that in the meantime the reasons for believing in something which may vaguely be called telepathy have been considerably strengthened. This has, in part, been due to the influence of facts already in print before that publication, but which had not yet been sufficiently digested even by those who knew them best. Among the corroborations the first to be mentioned is the enlarged census of hallucinations. It is true that this did not strictly conform in its scheme to the logical requisita--so much so as to bring its authors in some quarters under suspicion of dreading to meet the facts face to face; and, in consequence of its defects, the new census leads to no decisive conclusion. Yet it was certainly an improvement upon the first one, and did contribute something to the evidence for telepathy. But that which has strengthened the case much more has been the multitude of experiments, mostly unreported, which have been undertaken of late years. Almost everybody, roughly speaking, who has been interested in the subject, has made such experiments for himself. These experiments, too, have often been conducted with a measure of good sense, not in mechanical routine (which is as unfavorable to any other mental phenomena than those of fatigue as can be), but with some appreciation of the style in which telepathy, granting it to be a real action, appears as a matter of fact to operate. The results of the best of these experiments have usually afforded what appear to be indications, though extremely slight ones, of some tendency to something like telepathy. Now it was precisely such very slight phenomena, occurring with a certain frequency, that were wanting a dozen years ago to bridge the chasm between the ordinary course of nature and the apparently supernatural. In the third place, the decade has not been altogether barren of striking phenomena suggesting telepathy, which, though less probative than the more ordinary results just mentioned, yet, as being open to experimental tests, are infinitely more so than all the ghost stories that ever were told. The striking phenomena can amount to nothing until they are welded into continuity with ordinary experience. Finally, the very simple mechanical philosophy which was in high vogue still a dozen years since, though it had already been weakened by physical investigations, has now suffered serious dilapidations. It is now doubtful whether the conservation of energy can be regarded as the original and ultimately fundamental law that lies hidden beneath the corner-stone of the universe. The hypothesis which it behooves science provisionally to adopt is not in every case the most probable theory.

One source of pleasure in reading Mr. Podmore's present book is that, from beginning to end, there is scarcely a decided flaw in his inductive reasoning. He judges of a conclusion, not by his inclination to think one way or the other, nor by any rule of thumb, but by the necessities of the case. The general plan of the book

is to proceed from beliefs which the author refutes or only partially accepts, first to those which seem to him undeniable, and finally to those which try his faith. The first chapter sketches the history of the movement in psychical research. The second, upon "Spiritualism as a Popular Movement," shows the extravagant hardihood of the credulity of the spiritualists of the last generation. The next chapter criticises the experiments of Thury and Gasparin, of Dr. Hare, of Lord Lindsay, of Crookes, of Stainton Moses, and of Zöllner. The next chapter shows how the more severe investigations of the Society for Psychical Research refuted apparent phenomena of like description, sometimes identical. Chapter v. explodes Poltergeists. Chapter vi. shows how the Society exposed Madame Blavatsky's fraud. It concludes with these words:

"Of the later history of the Theosophical movement, and of the revelations made by Mr. Edward Garrett in 'Isis Very Much Unveiled,' it is not necessary to speak here. And it would be rash to prophesy even now--notwithstanding all the damning evidence of fraud, notwithstanding the loss of the unique personality of the foundress--that the movement is near dissolution. To most men who have given themselves over to a false belief, there comes a time when the ears are deaf and the eyes are closed and the heart is hardened, so that they will not believe even the testimony of the false prophet against himself. For are there not, as we have seen, black magicians and other powers of darkness who may transform themselves into the likeness of angels of light? With such men and against such a contention, argument is no longer even possible. *Discipiantur*."

With chapter vii. the constructive part of the book begins. Experimental Thought Transference is rightly taken up first, since it must form the logical basis of the whole doctrine of telepathy. The results of the experiments are very slender, and any one series is quite inconclusive. The circumstance that two minds of the same level living constantly together will, under given circumstances, be apt to notice nearly the same sequence of ideas, is left out of account. But then, on the other hand, were such phenomena more than just discernible in the long run, they would stand in flat contradiction to the ordinary course of experience. Their slightness is thus negative evidence of the veraciousness of the effects.

Telepathic hallucinations are next taken up. What the judicious reader will think of this chapter will naturally and logically depend upon the impression made upon him by the direct experiments. If these, upon the whole, can be considered as establishing telepathy as a real phenomenon, however slight it may ordinarily be, this conviction will be drawn upon to explain the highly intensified phenomena narrated in this chapter. In regard to hallucinations, it is to be remembered that while most men never have them at all, many others have so lively a visualizing power that they may be said to live amidst hallucinations all their lives. Others, such as many painters and chessplayers, have ordinarily no hallucinations about ordinary things, yet habitually behold as real the imaginary objects concerning which their brains are chiefly exercised. It is probably mostly among men of this intermediate class that visions of dear friends occur at times when their hearts are excited, whether through ordinary or telepathic channels.

Ghosts are next considered, and strong reasons are shown for believing them to be merely hallucinations. Haunted houses is the next topic. Nothing is said about the great number of houses which are inhabited or frequented by tramps and squatters. In the first case mentioned in this chapter, a Miss Laurence was going up stairs:

"When she reached the second-floor landing, she saw a cotton skirt, of a lilac shade and indefinite pattern, disappearing round the bend of the stairs leading to the top floor. Supposing it to be the housemaid, she called to her; and the housemaid appeared from a door close to her on the second floor. The only other servant was the cook, who was down stairs. Miss Laurence told the housemaid of her experience, and the housemaid replied, 'Oh, that's nothing, miss; I often see a skirt go round that corner.'"

Now is it natural the housemaid should think that "nothing," unless she knew it was a natural appearance? It was a house in Hyde Park Place; and intruders into city houses generally pass from one to another by the roofs. It often happens.

The author next considers Premonitions and Previsions. Here we meet with a number of stories marked by all the general characters of those relating to telepathic hallucinations. But the difference is, that these are not backed up, as those were, by experiments. Nobody has experimented upon premonitions and previsions, simply because the ordinary course of life has too thoroughly prejudiced everybody against any such thing. Yet it is hard to say how prophecy runs more counter to experience than does telepathy. It is, however, interesting to see that Mr. Podmore draws back before prevision, and wishes to consider this class of events as chance coincidences.

The next chapter, upon Secondary Consciousness, which has no obvious connection with psychical research, seems to be inserted merely as an introduction to the final chapter, which relates to Impersonation, Obsession, and Clairvoyance. It is the clairvoyance which particularly belongs to the questions here considered. As these phenomena, real or fraudulent, stand at present, they are too isolated to be brought under the dominion of science until further research shall discover other phenomena bridging the gap.

65 (25 November 1897) 424: **The Principles of Chemistry**.

By D. Mendeléeff. Translated from the Russian (Sixth Edition), by George Kamensky. Edited by T. A. Lawson. In two volumes, 8vo, pp. 621, 518. Longmans, Green & Co. 1897.

CSP, identification: Haskell, *Index to* The Nation. See also: Burks, *Bibliography*; Fisch and Haskell, *Additions to Cohen's Bibliography*.

The fifth Russian edition of this work was translated by Kamensky, and published under the editorial supervision of Mr. A. J. Greenaway in 1891. The present is almost equivalent to a new translation. It is somewhat surprising that it should be called for so soon, for there are many serious objections to its use as a class-book or text-book of any kind, while it is not full enough for a hand-book or work of reference. But the secret of the wide favor which it enjoys among students of chemistry is that the facts are so strung upon a thread of argumentation and of theory as to render the retention of them in the memory less operose.

We must not, however, be understood as saying that this is the chief claim of this treatise upon our attention. Nothing could be further from the truth, for, as an achievement of reasoning, it must for ever stand as one of the great monuments in the history of the progress of science. It was in writing this book that Mendeléeff discovered the periodic law of the chemical elements. Now there is no kind of physical law so difficult to demonstrate as a periodic law, nor any phenomena so deceptive as those which appear to manifest a periodic character. With very few exceptions, of a trifling kind, all those periodic laws which had been made out were either simply harmonic, or easily analyzed into harmonic constituents, or were supported, as in the case of the law of tides, by deductive considerations. In these cases they had been studied by means of residual phenomena--a method which supposes that the phenomena, at least in their means, are not much deranged by causes in which no regularities can be traced. But the chemical periodicity is extraordinarily complex; it is known to us only by pure induction, and, for reasons which are altogether unknown to us, only very roughly determines the phenomena. Thus, according to the latest calculations of atomic weights by Clarke, there are two cases in which values are transposed in their order of succession. In addition to that, the lacunæ in the list of chemical elements known to us, and the mistaken ideas which prevailed in regard to the chemical characters of some elements, rendered the attempt to discern the true order of nature in this respect excessively difficult. But Mendeléeff's predictions based upon his theory were such as to show a perfect mastery of it and extraordinary confidence in the truth of his *aperçus*. The next three elements to be discovered were gallium, scandium, and germanium. They not only fell into the places indicated by Mendeléeff, but they possessed precisely the properties which he had described them as having.

Considering all the difficulties and the striking success of this purely inductive inference, it may be ranked as second to the research of Kepler into the motions of Mars. But this is not the sole feature that makes the extraordinary work in which it occurs important in the history of chemistry. Mendeléeff's views upon many branches of chemical theory are highly original and suggestive, and are urged home with great force. To be rated rightly, the book should be read consecutively from beginning to end. Unhappily, it is impossible to praise the work of the editor. Errors of every description abound, and the English is here and there such that the meaning is doubtful.

65 (9 December 1897) 458-459: NOTES

CSP, identification: MS L 159.152; MSS 1408, 1408(s) (drafts). See also: Burks, *Bibliography*.

--A work that for nearly a thousand years--say from A. D. 850 to 1750--enjoyed a popularity almost unparalleled, appears (who knows but for the last time?) in an English dress. It is 'The Consolation of Philosophy of Boëthius,' translated into English prose and verse by H. R. James (London: Elliot Stock). It is on the whole a satisfactory piece of work, though the versification is hardly easy; and makes a pretty book, which anybody will like to put on his shelves. Mr. James mentions that before his there have been "nearly a dozen" English and Anglo-Saxon versions. This statement is probably based on an inspection of Watts and Lowndes, but we can enumerate more: First, King Alfred's; second, Chaucer's; third, that of John Waltionem, or Walton, 1525; fourth, Lydgate's, 1554; fifth, that of "George Colvile, alias Coldewel," 1556; sixth, that of I. T., 1609; seventh, that of S. E. M., 1654; eighth, that of H. Coningsby, 1664; ninth, that of "A Lover of

Truth and Virtue," 1674; tenth, Lord Preston's, 1695 (revised 1712); eleventh, Warburton's partial translation; twelfth, W. Causton's, 1730 (improved by Bellamy, 1768); thirteenth, that of the Rev. Philip Ridpath, 1785; fourteenth, R. Duncan's, 1789; fifteenth, that of J. S. Cardale, 1829 (from Alfred's paraphrase). In English no one version has gone through many editions. In French, that of Pére René de Ceriziers, published in 1636, appeared in its twelfth edition in 1647, and was the leading one for very many years after that. The translation of Léon Colesse was also often reprinted. That of the dramatist Judicis de Mirandol received a prize from the Academy in 1861. In Italy the translation of the poet Varchi was printed every few years from 1551 to 1798. In Spain that of Villegas, 1665, is highly extolled. Of versions earlier than Chaucer's, two, into High German and French, are of great linguistic importance. There was one into Hebrew, and a second into French by the author of the 'Roman de la Rose.' Leibniz abridged the work for his private edification. But even Leibniz evidently found the first two of the five books the best, the later ones being too much occupied with metaphysico-logical subtleties. By a noticeable coincidence, along with Mr. James's edition we receive from David Nutt, London, a sumptuous yet modestly tasteful reprint of our No. 5 above, viz., Colvile's 'The Boke of Boecius, called the Comforte of Philosophye, or Wysdome,' but without "the Latin added to the mergentis." The style is simpler than most of the Elizabethan of a generation later, and is pleasing. The metres are in prose; but the translator's marginal glosses afford some compensation. This elegant volume is the fifth of the "Tudor Library," and is edited by Ernest Belfort Bax. Two hundred and fifty copies of it have been printed at the Chiswick Press. Mr. Bax, in an introduction, seems to think that the Christian books attributed to Boëthius may have been written by his son. They are, however, almost unquestionably earlier than the celebrated Boëthius, and are probably by that Boëthius whose wife was Elpis.

65 (30 December 1897) 515: PROPOSED SYLVESTER MEMORIAL

TO THE EDITOR OF THE NATION

SIR: May I be permitted to appeal through your columns to all friends and admirers of the late Professor J. J. Sylvester to assist in founding a suitable memorial in honor of his name and for the encouragement of mathematical science? A movement

was inaugurated on this side of the Atlantic soon after his death, and it was resolved by the promoters that a fund should be raised for the purpose of establishing a Sylvester Medal, to be awarded at certain intervals for mathematical research to any worker irrespective of nationality. For the purpose of carrying out the scheme, a strongly representative international committee has been formed, and I should like to take advantage of this opportunity of expressing the great satisfaction which it has given to the promoters to be enabled to include in this committee so many great and distinguished names from the American universities. In every case our invitation to join the committee has been most cordially responded to, and the consent has in many instances been accompanied by expressions of the greatest sympathy and encouragement. The list, as it stands, practically includes the leading mathematicians of the whole world.

It has been estimated that a capital sum of $5,000 will be sufficient for the proposed endowment, and of this about one-half has already been subscribed here. In appealing to the American public to enable us to complete the desired sum, I am in the first place prompted by the consideration that Sylvester's association with the Johns Hopkins University, and the leading part which he took in advancing mathematical science in America, render his claim to estimation on the part of the citizens of your country quite a special one. It is but a modest endowment that we are asking for, and I am sure that all those who were personally acquainted with him, and who realize the great influence which he exerted in raising the intellectual level of every institution with which he was associated, will be glad of this opportunity of cooperating in the movement.

It is proposed that the fund, when complete, shall be transferred to the Council of the Royal Society of London, that body having undertaken to accept the trust and to award the medal triennially to mathematicians of all countries.

I can hardly venture to trespass upon your courtesy to the extent of asking you to print the complete list of our committee, but for your own information I beg to send a copy herewith. It will be sufficient to state that it comprises the names of President Gilman of Johns Hopkins University, of Professor Simon Newcomb of Washington, of Professor Willard Gibbs of Yale, of Professor Peirce of Harvard, and many other well-known American men of science. Subscriptions may be sent to and will be acknowledged by Dr. Cyrus Adler, the Smithsonian Institution, Washington, or by Dr. George Bruce Halsted, President of the Academy of Science, No. 2707 Guadalupe Street, Austin, Texas.--I am, sir,

Yours obediently,

RAPHAEL MELDOLA,

Professor in the Finsbury Technical College, London, England, Hon. Organizing

Secretary to the Sylvester Memorial.

DECEMBER, 1897.

65 (30 December 1897) 518: NOTES

CSP, identification: MS L 159.160. See also: Fisch, *Second Supplement*.

Prof. John Perry's 'Calculus for Engineers' (Edward Arnold) may be mentioned on account of its singular plan. It has three chapters. The first is entitled simply "*xn*," and treats of such things as velocity, acceleration, energy, thermodynamics, moments of bending, etc. Chapter ii., entitled "*ex* and sin *x*," treats of compound interest, telegraph leakage, Newton's law of cooling, slipping of belts, harmonic functions, imaginaries, forced vibrations, various electrical questions, symbols of operation, struts with lateral loads, etc. Chapter iii., entitled "Academic Exercises," gives Taylor's and Maclaurin's theorems on pp. 317-319, and on p. 322 takes up the solution of differential equations. These, with numerous practical examples (matter for several large volumes), carry us to p. 338, leaving thirty pages for elliptic functions, spherical harmonics, conduction of heat, the gamma function, etc. Such a lightning express through the calculus may very likely serve to give young men a sense of the importance of the subject and a desire to study it more systematically.

65 (30 December 1897) 524-525: **The Conception of God**.

By Josiah Royce, Joseph Le Conte, G. H. Howison, and Sidney Edward Mezes.

The Macmillan Co. 1897. 8vo, pp. xxxviii, 354.

CSP, identification: MS 1512. See also: Fisch, *Second Supplement*.

Josiah Royce (1855-1916) was, after William James, the best known Harvard philosopher. He took his first degree from the University of California in 1875, and then spent a year at Leipzig and Goettingen, where he was influenced by Lotze and Schopenhauer. He returned to the U.S. and was graduated Ph.D. from John Hopkins University in 1878. In 1882, Royce was appointed to the faculty of Harvard, mainly through the influence of James.

Joseph Le Conte (1823-1901) was graduated A.B. from the University of Georgia in 1841. He entered the College of Physicians and Surgeons in New York, from which he was graduated in 1845. He practiced medicine for seven years before entering the Lawrence Scientific School at Harvard to study under Louis Agassiz; he was graduated B.S. in 1851. During the Civil War, Le Conte served as a chemist in the Confederate Government Medical Laboratory at Columbia, South Carolina. After the war, he became a professor of geology and natural history at the University of California. Le Conte's writings are mainly on topics of science, philosophy, and religion.

George Holmes Howison (1834-1917) aided W. T. Harris in founding the *Journal of Speculative Philosophy*. He was graduated A.B. from Marietta College in 1852 and in 1864 became assistant professor of mathematics at Washington University. In 1871, he was appointed professor of logic and the philosophy of science at MIT. Howison lectured on ethics at Harvard (1879-1880), on speculative philosophy at the University of Michigan (1883-1884), and on moral philosophy at the University of California (1885-1909).

Sidney Edward Mezes (1863-1931), a native Californian, was graduated B.S. from the University of California in 1884. He thereupon left the West and entered Harvard, from which he was graduated A.B. in 1890, A.M. in 1891, and Ph.D. in 1893. In 1895, Mezes became adjunct professor of philosophy at the University of Texas, where, in 1902, he was made Dean of the College of Arts and Sciences, and, in 1908, he became president of the University. After many years of successful administration, Mezes left Texas to become president of the College of the City of New York.

On some day in 1895 Professor Royce delivered an address before the Philosophical Union at the University of California, which occupies fifty pages of this

volume. This address was devoted to a restatement and simplification of the argument for the existence of a God given in Professor Royce's earlier book 'The Religious Aspect of Philosophy.' The argument substantially is, that the mere existence of experience shows that something exists, no matter how incoherent that experience may be. That experience, with whatever its existence involves, has a whole; and therefore there is an all-embracing being, which is all-knowing. Then, following out this line of reflection, the other attributes of Deity are regarded as deducible from omniscience. Of course, the other disputants, Professors Le Conte, Howison, and Mezes, were familiar with this argument in the earlier form in which its author had broached it. Following Professor Royce, Professor Mezes presented certain objections, which, as printed here, occupy a dozen pages. He substantially admits Prof. Royce's metaphysics, but is unable to see that the existence of a good God is thereby proved. Prof. Le Conte followed, and, in remarks of about the same length, urged, in very simple eloquence, the existence of a soul of the world, as harmonizing with our knowledge of nature and with the theory of evolution. The debate was closed by an attack upon the argument by Professor Howison, of equal length with the address that set it forth, from the standpoint of a Berkeleyan idealism. Prof. Howison, however, does not explicitly state his own theory.

It appears that there were subsequent private discussions among the disputants, which had the excellent result of inducing Prof. Royce to write a supplementary essay that fills considerably more than half the volume, which is thus two-thirds of his writing. An introduction of thirty-eight pages, by Professor Howison, resumes the course of the disputation, and informs us that it resulted, in accordance with the time-honored custom of debate. Whether philosophical or political, in all parties retaining their original opinions. The Supplementary Essay treats chiefly of the principle of individuation, and this, even more than the other parts of the book, is of decided value as a line of reasoning concerning logical and logico-metaphysical matters.

For few men will the book have any practical religious importance. In the eyes of the majority of modern logicians, religious metaphysics for the most part falls into two logical sins, which are far worse than fallacies. In the first place, it violates that logical rule which may be said to supersede all others with an imperial sway; namely, owing to the power of genuine scientific reasoning to correct itself--that is, to correct its own previous conclusions by the admission of additional evidence, to correct its very premises, as it constantly does in the most exact sciences, and even to correct its own fallacies, of which there are many historical examples--and owing to the fact that this scientific procedure is nothing but the self-development of man's original impluse [[sic]] of curiosity or interest, it may be said that the only one thing absolutely indispensable to the discovery of truth is the perfect sincerity and earnestness of the endeavor to get one's errors corrected and be set right. But the religious philosophers are not striving to get set right, but to defend a foregone conclusion, and, as history shows, by their mutual conflicts they attain only the imposing persuasions for which they strive, and not the truth for which they do not strive. It has been as often remarked that the religious metaphysicians usually commit another logical fault, that of totally misunderstanding the nature of necessary reasoning. Metaphysics has always been an ape of geometry. But nowadays geometers no longer regard the postulates of geometry as axioms, but merely as hypotheses. All that necessary reasoning can do is to keep an initial hypothesis consistent with itself; it cannot prove any matter of fact. But the religious metaphysicians seek thus either absolutely to hoist themselves by their boot-straps, or at least to very much increase the height of their jump. Starting with no premise except such as every man knows, they seek to make this take the place of the special religious experience upon which Christianity professes to be founded. Since Hegelianism was exploded, the world will not believe that philosophy (that is, so much of science as can be inferred from the common experience of all men) can do the work of the special sciences, each of which is founded on some department of experience, to be undergone only upon the fulfilment of laborious conditions.

The question to which Royce's Supplementary Essay is mainly devoted has an intimate connection with this frequently urged objection to metaphysics. The world of possibilities, in which necessary reasoning holds a solitary sway, is a world of generals. You can no more suppose an individual horse than you can wish for an individual horse. You can suppose a pseudo-individual--for example, a vague individual--just as you can wish for one horse and no more. Yet even this you can do only by the aid of a real experience. The world of existences to which truth relates, and in which necessary reasoning is out of place, is a world of individuals. The question for metaphysics is, therefore, how deep into the nature of things does the distinction between the general and the individual go. Prof. Royce comments upon what Aquinas and Duns Scotus have to say upon this subject, which was the leading topic of logico-metaphysical discussion from the middle of the thirteenth to the middle of the fourteenth century, say during the period of the decorated Gothic architecture. Duns Scotus, one of the ablest logicians that ever lived, was the principal doctor of that period to hold virtually the proposition that metaphysics has nothing decisive to say concerning religious questions, which must be left to religious experience or special information, or concerning special science, which must be left to the appropriate department of experience. "Non potest probari Deum esse vivum." See this and some dozens of similar propositions in his 'Tractatus de Creditis.' But if pure reasoning is thus impotent as to existence, the concrete world cannot be a mere solidification of the world of ideas. In harmony, therefore, with that view of demonstrative reasoning, he held that the principle of individuation was a certain "positing mode of being" (*positiva entitas*). This was not very explicit, but it was important as showing that the individuality was something utterly different from anything else. Throughout the writings of Scotus, not merely in the famous discussion concerning the third distinction of the second book of the 'Sentences,' but wherever the question of individuality is approached (as in the 'Questiones Subtilissimæ') we see its perculiarity insisted upon; yet Prof. Royce has read Scotus through such spectacles that he adduces him as an authority in favor of his own views. But Royce's speculations burningly approach the truth, and become most interesting when he connects individuation with will. This is an important step for one who has always been an absolute idealist in the Hegelian sense; for the moment that the

phenomenon of will, in its strictly individual character, is given its due place in philosophy, though a sort of absolute psychism may survive, a seed of death would seem to be implanted in the Hegelian system. Here is the keynote to this highly important contribution to logico-metaphysical thought:

"We have seen that the completion of the unity of Absolute Consciousness demands the presence of a factor not separate from thought and experience, yet not definable in terms either of bare thought or of the data of immediate experience, in so far as they are merely felt or are present as the merely sensuous fulfilment of thought. Theis [[sic]] new factor we have defined as will. We have seen that it does not form merely one of the contents of experience to which thought refers, but determines the world which fulfils thought to be this world rather than any of the other to be this world rather than any of the other of the abstractly possible, but not genuinely possible worlds."

The last words indicate that Prof. Royce is still unwilling to admit an element of blind force in the universe. Nor does he make much of the intrinsically dualistic character of will.

1898

66 (3 February 1898) 96: Some Unrecognized Laws of Nature:

An Inquiry into the Causes of Physical Phenomena, with Special Reference to Gravitation.

By Ignatius Singer and Lewis H. Berens. D. Appleton & Co. 1897. 8vo, pp.

511.

CSP, identification: Haskell, *Index to* The Nation. See also: Burks, *Bibliography*; Fisch and Haskell. *Additions to Cohen's Bibliography*.

The purpose of this work is to revolutionize physics by showing that the cause of forces is a tendency to equalization, an action of which viscosity is a familiar example. Viscosity equalizes the velocity of the different layers of a stream. Now this, as is well known, is not a *conservative* force--that is, neglecting effects of heat, it appears to violate the law of energy; so that to make an action like viscosity the fundamental cause of all forces is to set up a theory in opposition to the doctrine of the conservation of energy. The principle of equalization is represented as one of four--persistence, unequal resistance, reciprocity, and equalization--from which the whole play of nature may be deduced. This formula was first remarked by the authors as governing the growth and development of animals and plants, and is now extended to pure physics.

All this might have been maintained, one would suppose, without calling in question the majority of the great discoveries, or propositions accepted as discoveries, which during the last three centuries have been so laboriously, and, as it would seem, so successfully, elaborated by physicists. But, as the discussion is conducted in this volume, substantially the entire body of established dynamics and physics is proposed to be swept away as worthless. Comprehending these circumstances, no person of good sense will undertake to read this book, unless as a study in psychology, without being thoroughly versed, at least, in every branch of mathematical physics; and those persons who really are competent to discuss questions of this kind will soon discover that neither Mr. Singer nor Mr. Berens is among their number.

What, for instance, is to be thought of authors who calmly remark that "not sufficient notice has been taken by physicists" of the law of action and reaction? That law, as everybody knows, is insured of thorough-going application by the aid of the conception of Mass. But these authors are capable of saying, "Whether a pendulum set swinging *in vacuo* and having no friction would actually keep on swinging for ever, . . . the matter of doubt in our mind is whether the downward pull of gravitation would not have to be regarded as another retarding element, besides friction and the resistance of the air." This betrays utter want of grasp of the principle of the pendulum, connected with a failure to distinguish between mass and weight. And, accordingly, we are informed that *mass*, in its last analysis, is nothing but *gravity*. Now this is not so. There might be so such force as gravitation, proportional to mass, and yet the action of bodies under magnetical and

electrical forces, and under impact, would still proclaim the reality of mass so long as the law of action and reaction continued to operate.

We meet with equally gross misunderstandings when we pass to thermotics. For instance, we are told (pp. 24, 93) that the doctrine of the absolute zero rests upon nothing except "on the theory that at that temperature a body would be practically annihilated"; that is, the authors suppose the air or hydrogen thermometer is its only basis. One would find it hard to invent a remark which should more manifestly betray total ignorance of the theory of heat. Yet the following beats it: "The late Professor Tyndall held that heat was 'a mode of motion.' It is not quite clear whether the phrase is to be understood," etc. A man who does not know what the kinetical theory of heat is, and supposes Tyndall the author of it because of his popular book, and who nevertheless presumes to speak of the greatest physical speculator of our time except Helmholtz thus: "Thomson's error consisted in confounding the common or industrial meaning of the term 'work' with its philosophical meaning," cannot expect to make a favorable impression upon the scientific world. The book is simply chock-full of blunders quite as bad as the above few specimens.

The work is not entierly [[sic]] one of ratiocination. Various experimental results are announced which are supposed to support the theory. These are, for the most part, old mare's-nests which have already been sufficiently investigated. Some of them might be worth reexamination with all our modern appliances of precision, but none of the experiments herein described appears to have been conducted with any extraordinary refinements. A few of them are rather curious, though probably not important.

The first words of the preface are these: "At last, after years of patient plodding in dim regions where the footprints are few and the pitfalls many, the time has arrived when we are enabled to place before the world of science the firstfruits of our explorations." No doubt a great part of two men's lives has gone into this enterprise, and it is tragic to see that the result is good only to fill a page in some supplement to the 'Budget of Paradoxes.'

66 (31 March 1898) 250-251: Social and Ethical Interpretations in Mental Development.

By James Mark Baldwin. Macmillan. 1897. 8vo, pp. 574.

CSP, identification: MS 1513 (draft). See also: Fisch, *First Supplement*.

Prof. Baldwin here puts forth a sequel to his remarkable work on 'Mental Development in the Child and the Race,' which our readers will remember contained a most valuable body of observations upon two children. The aim of the present volume is "to inquire to what extent the principles of the development of the individual mind apply also to the evolution of society." But no insignificant part of the former volume was devoted to this same subject; so that the contents of the present work were largely anticipated in their outlines in the former publication. About five-sixths of this new book is occupied with the development of the individual consciousness, and is substantially a restatement of the author's previous results, without any gain in clearness.

The general position of the author, that the individual mind is produced by intercourse with other persons, while on the other hand society is the composite of the individuals, so that the two factors are inseparably conjugate, is certainly far from being novel, and no doubt Hegelians will see in it a new instance of the permeation of their master's doctrine. In point of fact, there are in these pages many indications of the great interest that Prof. Royce has felt in the labors of Prof. Baldwin. But what is entirely fresh is the discussion of that proposition upon the basis of rich stores of scientific observations.

In the previous volume the author showed how his observations had led him to admit three distinct stages in a child's knowledge of personality, those of persons as "projects, subject, and ejects." The first of these is the most difficult to understand, and it is itself subdivided into three imperfectly distinguished stages. The baby first distinguishes persons from inanimate things, according to Prof. Baldwin, by their moving about; and by the character of these movements it distinguishes one person from another. This is the state of consciousness during the first half-year of its life. But gradually it becomes impressed by the irregularities of some of the movables. The pendulum goes tick-tack with perfect uniformity, while the father sometimes notices the child and sometimes does not. Thus, persons become known as movables that are eccentric. In this irregularity Prof. Baldwin thinks that the child recognizes Agency; and after the second half-year and up to the age of two years, it is learning to recognize a special uniformity, or characteristic, in the peculiarities of each movable eccentric. This is a recognition of Personal Character. So far, persons are known merely as "projects." But now the child is beginning to act, and in acting it recognizes its own person as similar to the agents that are already familiar to it. It thus attains the second stage of knowledge of personality, that which connects its own feelings with the idea of agency previously acquired, bringing it to a sense of its own subjectivity, and a knowledge of Self, as subject. Finally, it hypothetizes for each of the other agents a corresponding subjectivity, and thus converts them into ejects.

Such is Prof. Baldwin's theory of the development of self-consciousness. As the title implies, the present book is largely an interpretation of phenomena connected with personal and social growth in the light of that theory. Great weight must certainly be attached not only to the describable observations of Prof. Baldwin, but also to those subtler intuitions which can only express themselves as his convictions that such and such are the thoughts and feelings of the child. But the above theory contains more than such observations, as we will venture to show. In the first place, it does not appear from the observations that, during the first half-year, the baby pays any attention at all to things that do not move. If it does not, then motion cannot serve to separate persons from things, but only to individualize the differently moving objects. In the second place, Prof. Baldwin may be quite correct in his insight into the infant's mind, so far as to perceive that the irregularity of persons is perplexing its mind, and also that some idea allied to that of Agency is present to it. Yet whether or not it is the former idea which suggests the latter, is not a question of observation but of inference.

There are several different ideas, mostly of an intellectual character, which might be denoted by the word Agency, but none of them have any logical connection with irregularity, which is mostly associated with the absence of any definite Agency. Now, although it is certainly conceivable that one idea should suggest another with which it has no logical connection, yet the hypothesis that any particular such illogical suggestion has taken place must remain quite gratuitous, unless a mass of facts can be adduced to support such an irrational connection. There is a great gulf between the idea of an eccentric, surprising thing and that of agency in any sense. We cannot help suspecting that, notwithstanding the close observation of Prof. Baldwin, the child has made innumerable *efforts* before the age of two, which the author assigns as the commencement of the subject-knowledge. If these efforts have escaped his keen eye, it is because they were so futile. Not only does the sense of effort necessarily involve a sense of resistance so as to objectify itself immediately as an I and a not-I, however rudimentary these conceptions may be; a sense of failure, which is sure to accompany the first efforts, must magnify the effort and the resistance, and thus stimulate the subjective tendency. It is very doubtful whether there is any earlier idea of agency than that which must thus come from futile effort. If not, Prof. Baldwin's "projects" are merely ideas of queer, eccentric, startling movables--the only distinct objects of the baby's world--and are in no proper sense ideas of personality. If we remove from the author's philosophy of society all that is said about "projects," it may lose a good deal of its freshness, but it will become more widely acceptable.

Prof. Baldwin has a great deal to say of the influence of the child's own actions, particularly in his games, in shaping for him clear conceptions; and he rightly regards this truth as highly important. He adds that these very actions are for the most part imitations of the conduct of his elders, and thus the child's understanding becomes formed after the pattern of the grown-up people about him. All this he terms social heredity. Whether or not this begs a question will be a point sure to be discussed. The individual, says Prof. Baldwin, is the product of society, while on the other hand to all which he "inherits" from his family he imparts his own personal signature. Invention invariably accompanies imitation, although in very variable proportions.

Prof. Baldwin thinks that the "project" is recognized as the master of the "subject," and the "subject" in its turn of the "eject," and that the disposition of children to domineer over weaker children is a case under that rule. There are, he declares, two sorts of social influences, that which produces social organization and that which appears in particularizing and synthetizing actions of individuals. All individual variations are particularizations of earlier generalizations. The author is thus working his way toward the conception of a public self, and the further he proceeds the more he seems to be influenced by Hegel or Hegelians.

The matter of social organization consists, he says, of imaginations, knowledges, informations--a statement which, in its desire to minimize the individuality of things, betrays already the incipient sway of Hegelian tendencies. He uses the term "self-thought-situation" for the social situation implicated in the thought of self, where a dialectic process productive of the thought of self is plainly recognized.

After that we are not surprised to be told that "every socially available thought implies a public self-thought-situation which is strictly analogous in its rise and progress to the self-thought-situation of the individual member of society." The author differs from Hegel only concerning a matter of detail, namely, in recognizing imitation as the bridge from the private thought to the public thought, which enables the self-thought-situation to become public. He considers that all that has been written by the School of Moral Sentiments concerning sympathy as imagining one's self to be in another's situation, is so much in favor of his own doctrine of the importance of the imitative process in the development of public consciousness.

The application of this to ethics, as developed in the chapter devoted to Rules of Conduct, is sufficiently smooth sailing. To those who think that in Morals, at any rate, conservatism is the safest course, and who are sceptical about the desirability of carrying any system of philosophy into practical applications until there can be a little more agreement among philosophers as to what is proved and what is not, the present volume, however interesting and important, will be deemed inferior to its predecessor in almost every respect. That it richly deserves the gold medal of the Danish Academy with which it has been crowned, there can be no doubt.

66 (21 April 1898) 311: Memory and its Cultivation.

By F. W. Edridge-Green, M.D., F.R.C.S. (International Scientific Series, No.

78.) D. Appleton & Co. 1897. 8vo, pp. 311.

CSP, identification: Haskell, *Index to* The Nation. See also: Burks, *Bibliography*; Fisch and Haskell, *Additions to Cohen's Bibliography*.

This author recognizes thirty-seven faculties of the mind, differing in but few particulars from the thirty-seven accepted by the phrenologists, and located like theirs in the cortex of the brain. This is utterly at variance with all the results of the last thirty years' study of the functions of the brain. Still worse, although Dr. Edridge-Green acknowledges that memory is the most important of all the powers, he places it in the corpus striatum and optic thalamus--that is, in organs between the cortex and the spinal cord. Two chapters are devoted to arguing these positions, yet the only reference to the experimental researches of our time is one brief and vague mention of Ferier's work. No reasons are put forward which are not either old or insignificant.

The opening sentence of the book is, "What is memory?" This is pertinent; but the true answer is not given. The phenomena of memory are nothing but those of the phenomena of association by contiguity, in which the suggested idea brings with it so much of its environment as to be referred to the past. Hence, whatever cerebral explanation is given for association in general must be applied to the chief constituent of memory. Dr. Edridge-Green (p. 145) appears to locate association by contiguity in the optic thalamus. Considering that association by contiguity is nothing

but mental habit, and that habit-taking is one of the fundamental attributes of protoplasm in general, the theory of this work could not well be narrower or more arbitrary.

Although the theoretical part of the book is so unimportant, it might be hoped that some wise counsels would be given about the cultivation of memory. All we find, however, is a series of twenty-two mnemonic rules. Though there are so many, they omit some well-known principles, such as that of the summation of stimuli. Among the rules given are a few which will probably be of some value. Others, though well enough, are trite. Still others are both trite and pernicious, because they recommend the burdening of the memory with utterly trivial and useless associations. Thus, we are advised to remember that the first Roman invasion of Britain took place 55 B. C. by associating it with the vocables "Juliud Cæsud," and remembering that the letter *d* means 5. Is it not much easier to remember that it was seven years after the defeat of Catiline and seven years before Pharsalia? The good old way of learning a few important and familiar dates *per se*, and the rest by their intervals between those, can hardly be improved.

66 (28 April 1898) 330-331: **Astronomy**.

By Agnes M. Clerke, A. Fowler, and J. Ellard Gore. D. Appleton & Co. 1898.

Attributed to Peirce by Fisch in *Third Supplement*. The review answers the description at 7.182 in Arthur W. Burks, ed., *Collected Papers of Charles Sanders Peirce* (Cambridge: Harvard University Press, 1966). This item is unassigned in Haskell's *Index to* The Nation, vol. 1.

Agnes Mary Clerke (1842-1907) was a historian of astronomy. She resided in Italy from 1867 until 1877, writing on astronomical and literary subjects, mainly for the *Edinburgh Review*. Clerke published several works on the history of astronomy and on astrophysics. In 1903 whe [[sic]] was elected an honorary member of the Royal Astronomical Society. She was an accomplished musician, as well as possessing other talents.

Alfred Fowler (1868-1940) was a British astrophysicist. He studied mechanics at the Normal School of Science, South Kensington, and from 1888-1901 was demonstrator in astronomical physics there. In 1915, he was made assistant professor of astrophysics, and he was Yarrow Research Professor of the Royal Society from 1923 until 1934. In 1919, Fowler served as the first secretary of the International Astronomical Union.

John Ellard Gore (1845-1910) was an astronomer and member of the Royal Irish Academy. Gore's best known works are *Catalogue of Known Variable Stars* (1884), *The Worlds of Space* (1894), and *The Stellar Heavens* (1903).

So many able men have entered the contest to produce the best popular astronomy that the standard of excellence for such works is now very high. At the same time, they have, on the whole, as one after another has appeared since Herschel's 'Outlines,' been growing less and less intellectual. The present volume is not as sensational as those of Flammarion: it contains nothing calculated to terrify the reader, nor any scheme for communicating with inhabitants of Mars. Those particular varieties of silliness are not this year in fashion. We read, p. 404, "According to the Book of Enoch the constellations were already known and named in the time of that patriarch." Since everybody knows that the

Book of Enoch is not even admitted into the Vulgate, a stronger impression of the antiquity

of the constellations, which undoubtedly go back to the very beginnings of astronomy, could have been conveyed by mentioning that Alexander is said on good authority to have sent home from Babylon astronomical records going back to 2300 B. C.; and the "Phænomena" of the Macedonian poet Aratus, which, according to an ancient witness, depicts a globe, really describes the heavens as they were 2300 B. C. But this way of stating the minimum age of the constellations would not answer the purpose, because it supposes the reader to have sufficient logical power to follow an argument intelligently. There are no tables nor lists pretending to completeness (except that a list of the signs of the zodiac has been admitted, probably because the traditional methods of young ladies' academies make a good deal of signs of the zodiac); nor is there any attempt at summing up, or an account in any respect thorough of the present state of any branch of research. At the same time, the book reflects to-day's current opinions among English astronomers, and, touching most of the questions of descriptive astronomy now uppermost (and not too difficult), has its value, and will be used to advantage in schools of a certain character. There is no transcendental accuracy about the work in any particular.

66 (16 June 1898) 467-468: **Dynamic Idealism: An**

Elementary Course in the Metaphysics of Psychology.

By Alfred H. Lloyd. Chicago: A. C. McClurg & Co. 1898. 16mo, pp. 248.

Practical Idealism.

By William De Witt Hyde. Macmillan Co. 1897. 8vo, pp. 335.

Attributed to Peirce by Fisch in *First Supplement* (internal evidence). Peirce owned a copy of *Dynamic Idealism* which he had annotated. This review is unassigned in Haskell's *Index to* The Nation, vol. 1.

Alfred Henry Lloyd (1864-1927) was a Harvard man through and through: A.B., 1886; A.M., 1888; Ph.D., 1893. For several years, Lloyd was professor of philosophy at the University of Michigan. In reply to William James' *The Will to Believe*, Lloyd penned *The Will to Doubt* (1907). An honorary LL.D. was conferred upon him in 1924 by the University of California.

It is pretty confident to preach such brand-new theory as Mr. Lloyd's, unpassed upon by any jury, as an "elementary course." Far be it from us to pronounce this or that general attitude in philosophy unsound, for nothing is established in that science as yet. The author of this little book at least makes his own position perfectly clear, and develops it with no feeble thought and with an unusual power of compact expression. Whoever succeeds in doing that, in the present unsettled state of opinion, renders a service to philosophy. Though the book shows many marks of digested study of other philosophers, the author does not attain to mastery of his idea. It masters him; it is his element; he fails to comprehend that other minds do not share in it. This fundamental idea is nothing new or rare. Outside the garden where philosophers converse, it is the common opinion. Namely, it is, that Doing is higher than either Being or Knowing, and necessarily includes them both. So axiomatic does this seem to Mr. Lloyd that he is capable of such assertions as this: "Science is never only for science's sake. Men have often appeared to think that science as a body of knowledge was its own end, but obviously to think so long is quite impossible." Now, as a matter of historical fact, real scientific men, in every age when science has been animated by a vital spark, have one and all pursued science for its own sake. Of a piece with that, is Mr. Lloyd's claiming the support of Aristotle, whose {energeia psychës} is a development of Being, and not a mere Doing, and who might probably have said: "Doing is never for doing's sake. Obviously to think that it is so for long is quite impossible. Doing for doing's sake is what we call pastime; it cannot form the staple of life." Do not let us be understood as arguing the question; we only point out how immersed the author is in his own mental element.

This leading idea, taken in itself, is nothing but the usual Philistine apotheosis of brute force. It naturally allies itself with dualism, since an action is essentially an accident of two individual things. And accordingly we find that all philosophers who have adopted it have been given to abrupt distinctions between pairs of opposites--such as right and wrong--to the neglect of any gradation from one extreme to another. Yet the elaboration of the idea has by no means always been so homogeneous as to include a belief in the duality of body and soul. On the contrary, no philosophy ever appeared more satisfactory to the class of minds who are attracted to this idea or was more thoroughly believed in by matter-of-fact respectability, than Stoicism, which moulded this principle into a system of the most wooden materialism. Stoicism, we can hardly doubt, has been secretly entertained by millions throughout the Christian era--at any rate, down to the introduction of ether and chloroform. Their conduct is not otherwise explicable.

But to-day we live in an age whose prevalent spirit is intensely idealistic, even verging upon the mystical; and in a forest it is impossible to look far over the general level of the treetops. And so, in this book, we find the deification of force clothing itself in a "Dynamic Idealism." It is a strange phrase, a wondrous seething snow. It accurately names an emulsion of philosophical opinions that one would not have believed could ever be worked up into so homogeneous, substantial, and inviting a mayonnaise as Mr. Lloyd has managed to compound. The philosophy in its entelechy is as far as possible from deserving the disparaging epithets we apply to its first principle. Not only will it not be affectioned by the philosophically unregenerate, but it may be doubted whether even the elite will be able to accommodate themselves to it.

New systems of idealism nowadays get patented in such swift succession that novelty's self has long ago worn out its novelty. But this little book, it must be confessed, has something of the interest of a novel; for the reader's curiosity becomes whetted to learn by what surprise Mr. Lloyd will bring about a marriage between Dynamism and Idealism. In the first place, in order to detach Action from its inherent bruteness and impart to it an intellectual character, he defines it as relation--not relation such as our minds confer at will, but relation in the very real fact itself. Undoubtedly an action is a pairing *in re* of two things. It must also be admitted that nothing has an intellectual character except relations. Some readers may suggest that those relations which are intellectual are not mere pairings,

but rather mediations--that is to say, gatherings of threes, or, in Aristotle's language, syllogisms. Mr. Lloyd, however, does not notice this objection, and our purpose is only to sketch the contents of the book, not to criticise it. But, having thus described action as relation *in re*, or real pairedness, and having identified this pairedness with the mind, Mr. Lloyd seems to be as far from monism as ever; for are not the things paired one thing, and is not the pairing of them an accident over and above their matter? To avoid this result, Mr. Lloyd, as a second step, denies the separate existence of the correlates paired. According to him, nothing really exists but pure pairedness (his word is *relationship*) without any pairs of objects to be paired. That this doctrine must be classed as idealism is beyond dispute. Its upshot resembles Hegelianism. Here, then, is dynamic idealism. For all details we must recommend the reading of the volume, only copying the brief summary which the author prints over against his title-page: "Relationship among things is the criterion neither of a life nor of a mind that exists apart from the substance of the universe. It is, however, the criterion of substance itself, and as the central truth about things it bears this witness: *The universe itself lives; the universe itself thinks*."

Calculated for the meridian of Chautauqua, Mr. Hyde's 'Practical Idealism' is a manual of wholesome sentiments forcibly put. "Its practical aim precludes the discussion of ultimate metaphysical problems." That is to say, it is not scientific. At the same time, it was requisite to strengthen the heart of the semi-student by making him feel that he is studying philosophy. The author says that "philosophy is . . . tempted to forsake her mission as . . . guide to noble living, for the . . . technical craft," etc. In short, readers are taught to believe that Aristotle's great conquest for speculative science, in separating it sharply from questions of conscience and the like, was a great mistake and ought forthwith to be surrendered. Precisely that defines the efforts of the philosophical reactionists of to-day. As far as science is concerned, every shot they fire will fall harmless to the ground. But what their effect may prove to be upon the life and morals of their adherents, will depend upon the wholesomeness of calling that scientific which is not scientific. Were "practical idealism" plainly to confess itself to be no more than good wholesome feeling, sanctioned by the experiences of millennia, its practical aspects might be far more satisfactory than any scientific, and therefore merely provisional, hypothesis could be.

To mingle the two--philosophy and practical wisdom--is to invite vagueness and confusion, such as we here see, where the conflicting logical principles of Mill, Jevons, Sigwart, Bradlee, are jumbled together, where calling pure sensation a "continuum" is said to mean precisely the same thing as calling it a "confusion," and where the reasoning of Socrates in the 'Gorgias' is held up to admiration. Some of his criticisms of psychologists and logicians will give more aid and comfort to those who wish to separate speculation and conduct than the author seems to be aware. We believe that, in the long run, it will be found dangerous to teach Chautauquans that they are to "guide their conduct" by what may recommend itself to them as philosophy.

67 (14 July 1898) 31: NOTES

CSP, identification: Haskell, *Index to* The Nation. See also: Burks, *Bibliography*; Fisch and Haskell, *Additions to Cohen's Bibliography*; MS L 159.91.

It is a little strange that, after a generation of celebrity, Reye's 'Geometrie der Lage' should now be translated into English for the first time. Part I. comes to us from the Macmillan Co., Prof. Holgate of Evanston being the highly competent translator. The original has long been used in some of our American universities to great advantage. In certain respects it is a more brilliant book even than the treatise of Cremona, and it covers a somewhat wider field. But its merits are too well known to need any comment from us. Later researches into continuity go to show that Topology and not Graphic forms the real foundation and generalization of geometry; and the moment is almost at hand at which Reye's book must be superseded by one which shall lay the foundations of its logic deeper still. Meantime, this well-executed translation, with a useful preface, will serve a good purpose. We shall speak more particualrly [[sic]] of the version in noticing the following part.

67 (14 July 1898) 38-39: **The New Psychology**.

By E. W. Scripture, Director of the Yale Psychological Laboratory. [Contemporary Science Series.] Scribners. 8vo, pp. 500; 124 illustrations.

Attributed to Peirce by Fisch in *Second Supplement* (internal evidence). Peirce owned a copy of this book. This piece is unassigned in Haskell's *Index to* The Nation, vol. 1.

Edward Wheeler Scripture was a pioneer in the science of psychology. He was a graduate of the City College of New York, and received his Ph.D. in 1891 from the University of Leipzig. He was appointed director of the Yale Psychological Laboratory in 1897. In 1895, Scripture developed a color-sense tester for detecting color blindness and weakness. Among his other discoveries are those of the sensation of acceleration and of a method of measuring the intensity of hallucinations and imaginations.

The present volume worthily fills its place in the highly modernistic series edited by Mr. Havelock Ellis. It consists in the main of a general account of all that is done in the psychological laboratories. We cannot say that it renders a satisfactory measure of justice to American work. The uninformed reader will gather that, except at Yale, the experimental psychology of this country does not rank very high, which is far from the truth. Considering how young the experimental science of the mind is, and that it is not of full-blooded experimental parentage, it is not surprising that, to a chemist or physicist, the psychological laboratories should not appear as marked by great experimental ability. It is probably a more significant fact that the results, after all, seem to be confined to a narrow department of the mind, although precisely what the significance of this fact may be it is too early to judge with confidence.

There is nothing at all in this book about cerebral physiology, except some words of exaggerated praise, accompanying the opinion that that study throws little

light on pyschology. The psychophysicists, however, can hardly afford to dispense with the aid of the cerebral physiologists, for it is a question whether both together have accomplished as much as Berkeley, Hartley, James Mill, Young, and the rest of the old associationalists.

Apart from its account of psychology, this book does, in a sense, mark a stage in the growing importance of that science. Fechner's 'Psychophysik,' from which the science dates, was published in 1860 (a date Dr. Scripture ought not to have omitted); but the present general interest throughout the scientific world in the subject arose about 1888, when Dr. Stanley Hall's *Journal of Psychology* was established, and when Prof. James's expositions were beginning to attract attention. Yet, young as is the movement, in this volume psychology already begins to jostle astronomy from her throne, as that one of the sciences from which the logic of science is to be learned. Thirty years ago, when Herschel's 'Outlines' was still in use in colleges, when Bertrand and others were still explaining to the general public how Copernicus, Tycho Brahe, Keppler, and Newton had laid the foundations of science, the ordinary student expected that it would be in the course of his study of astronomy that he would become acquainted with the methods of scientific discovery and with the precautions needed in applying them. But the first four chapters of this book--one-sixth of the whole in space and about half its whole significance--are devoted to the general consideration of Observation, Statistics, Measurement, and Experimenting. This is in part due to Dr.

Scripture's field of view being limited by a German horizon, and to a consequent confusion between psychology and logic, a science properly and in itself no more connected with psychics than with physics. Nevertheless, as well as the horoscope of twentieth-century science can be read, there are still much greater heights of importance to which psychology is destined to rise before its culmination; and its educational conjunction with the logic of science may be expected to become closer and closer.

The chapters on method are well enough executed to be exceedingly instructive to beginners in science, though the author does not always attain the Ameriican [[sic]] standard of clearness. For instance, in defining what it means in the doctrine of chances to say that a die turns up a particular face "about one-sixth of the time," Mr. Scripture not only talks about a "scale of certainty" (meaning a scale of uncertainty), which has nothing to do with the frequency with which the face turns up, but he also introduces the probability-function, thus substituting a theorem for the definition on which that theorem rests; and while he thus admits alien ideas into his definition, he altogether ignores several essential elements, and, in doing so, leaves the door open for serious fallacies.

Again, Dr. Scripture is excessively strict, not to say stern, in requiring that statistics and bodies of measurements shall conform to the characters of random collections. He demands, for example, that the number of extreme values shall not exceed that determined by the theory. This would, from a theoretical point of view, be perfectly just, provided the probable effects of given amounts of departure from randomness were calculated and allowed for. Practically, however, Dr. Scripture's rule would have the effect of stripping science in every branch of almost all observations except such as had prudently not been often repeated or not been made numerically exact. It would leave astronomy without a leg to stand upon. We are not surprised to find this hyperrhadamanthine theory associated with practices equally removed from good sense in the direction of laxity. Thus, as a model of the proper treatment of observations, a series of eight observations are given, their mean taken, and their "mean error" calculated (not with strict accuracy, by the way, and several of the formulæ exhibit this fault), although of the eight obervations [[sic]] one departs from the mean in one direction by four times the amount by which the worst of the others departs, along with all those seven, in the opposite direction. Dr. Scripture suggests that the "median," or middling value, would be better than the mean, which is as much as to say that, rather than rank the discordant observation as of equal value with the others, it would be preferable to use but one of the eight as a measure and all the rest as mere qualitative indicators. In reply to this, a remark in the previous chapter is pertinent: "Yet [if we substitute counting for measurement], in a case where a possibility of measurement exists, we are really throwing away an accurate method for a poorer one." Here native Yankee gumption peeps out. Had Dr. Scripture studied the works of first-rate mathematicians among his own countrymen, as well as those of fourth-rate writers in inferior German translations, he would have learned of a better way of treating series of observations into which abnormal observations are mixed than that of merely counting them. There are other instances of extremely lax practice in treating measurements; but the above example will suffice.

The portion of the book devoted to psychological experiments proper is divided into three parts entitled Time, Energy, Space. This adopts an idea of Ostwald, the chemist, that Energy ought to be substituted for Mass, as a fundamental quantity in dynamics. Nothing more feeble has been put forth by a man of ability since Sir Isaac Newton's commentaries on the book of Daniel. That where there are three connected variables, independent functions of them may be substituted for the variables themselves, is a matter of course; so that nobody will dispute that, if we choose to do so, we can, for example, instead of saying that momentum is the product of mass into velocity, say that it is the quotient of the kinetic energy divided by the velocity. In that case, one might say, the less the velocity, the greater the momentum; meaning that if a moving body burst so that a portion of it has the same energy as the whole had had, then the greater its velocity, the smaller would be its momentum. But because moving bodies do not usually burst and in that sense alter their masses, but do continually change their kinetic energies, it follows that Ostwald's system would be inconvenient in experience; and since the human mind is formed upon the ordinary course of experience, that system is equally unnatural for the human mind. It has, in fact, nothing at all to recommend it, unless it be its enveloping the whole subject in a fog of metaphysics.

A fifth part of the book gives the history of the new psychology. We notice one interesting remark, that Fechner's law was in part anticipated in Daniel Bernouilli's theory of "moral expectation." We are glad to acknowledge that that

idle theory did contain the germ of a great truth, which Dr. Scripture has so acutely pointed out. We will not say more of the historical part, because we wish to avoid appearing utterly to condemn a work of a good deal of merit.

67 (25 August 1898) 154-155: THE PSYCHOLOGY OF SUGGESTION

The Psychology of Suggestion.

By Boris Sidis, M.A., Ph.D., Associate in Psychology at the Pathological Institute of the New York State Hospitals. With an Introduction by Prof. William James. D. Appleton & Co. 1898. 8vo, pp. 386.

CSP, identification: Haskell, *Index to* The Nation. See also: Burks, *Bibliography*; Fisch and Haskell, *Additions to Cohen's Bibliography*.

Boris Sidis (1867-1923) was a Russian-born psychologist and psychopathologist, who emigrated to the United States at age 20 and later enrolled at Harvard. After years of intensive work, Sidis was graduated Ph.D. there in 1897. He developed a deep friendship with William James, who wrote an introduction to the work reviewed here by Peirce.

An interesting book is this in more ways than one, beginning with the title-page, for it marks a stadium in the progress, both of psychology and of medicine, that the need of such an official as an "Associate in Psychology" should have been felt and filled in the Pathological Institute of the New York State Hospitals. It speaks well for that institution. It is interesting, too, to meet with so signal a vindication of the appointment as is afforded by the successful treatment of the extraordinary and interesting case of the Rev. Thomas Carson Hanna. This gentleman, of superior endowments and accomplishments, when he came to himself after a carriage accident, was like one new born. He had clean lost all knowledge, all passion, all voluntary activity. He was bereft of every vestige of familiarity with everything, had no suspicion that the sounds of speech had any meaning, never thought of the persons about him as persons, had no consciousness of self as such, and did not look upon the external world as real. When, after a few weeks, he had begun to use his hands, he had become ambidextrous. His logical acumen returned very early, while he was still in passional innocence, was asking the meaning of the simplest words, and wondering at the most every-day matters. The extraordinary rapidity with which he acquired and applied new knowledge, his keen sense of music and symmetry, and the significant fact that he learned English in a few weeks, and pronounced it well and correctly, confirmed Dr. Sidis in his first impression that the old personality had not been crushed to death, but had only been dissevered from conscious life, still in great measure swaying the newly-formed personality from the subconscious depths of being.

In order to "tap the subconscious self," the patient was questioned about his dreams. It turned out that, besides ordinary dreams, he had "clear picture dreams," which were in reality fragments of his former life, although he did not recognize them, but thought them very strange. Thus, in one of them, he saw a house with these letters on it: NEW BOSTON JUNC. He had lately learned to read the word NEW; but the other letters were entirely unintelligible to him. Latent

memory being thus proved, the problem was to bring it up into connection with consciousness. This was gradually accomplished by means of a method, due to Dr. Sidis, which he calls *hypnoidization*, and which is described. At length he was brought into a condition of double consciousness, complete amnesia separating the two states. Finally, by means of a method for an account of which we are referred to a subsequent publication, the two states were run into one. "The patient is now perfectly well and has resumed his vocation." There have lately been some further reports which confirm this statement.

The main purpose of the book professes to be to show that every man has a double personality--the one person dominant and self-conscious, the other subordinate and subconscious. This is not a theory towards which psychologists will antecedently incline; nor will they accept the evidences here adduced as at all sufficient. That the subconscious part of the mind makes up a unitary self will not readily be admitted.

The work is divided into three parts entitled Suggestibility, The Self, Society. The middle part occupies more than half the volume and is the centre of the author's thought. To this the chapters on suggestibility are introductory. "Suggestibility" is the Nancy word for that docility, or, if you please, that incitability, which is so exaggerated in the hypnotic trance, but of which everybody, especially an agreeable and sympathetic person, has a large share. This faculty, or state of mind, was first assigned as the main secret of the ordinary phenomena of hypnotism as long ago as 1845 by the American itinerant lecturer Grimes. But he was not an academic person, and was naturally ignored. He proposed the word "credenciveness" as the scientific name for the universal incitability of which we are speaking, briefly defining it as "that conforming social propensity whose natural stimulus is an assertion," but not neglecting to describe all its principal effects. "Credenciveness," he says, "is the key to most of the wonderful experiments of Buchanan and Sunderland, of Braid, Hall, and Elliotson." We may add that, by reducing Consciousness to the rank of a special faculty, Grimes paved the way to the modern doctrine of the subconscious mind. Modern psychology is suffering grievously from the lack of a precise and consistent terminology. The experience of other sciences shows that the only possible basis for a universally accepted scientific terminology lies in a strict adherence to the rule that the word proposed as the scientific designation of a concept by the discoverer who first introduces that concept into science, shall be adopted unless there are very solid objections to it. The word "credenciveness" is not particularly apt, because it does not obviously imply a tendency to action, although it was so understood by Grimes. On the other hand, the word "suggestibility"--aside from its awkwardness in seeming to substitute "facility to be suggested" in place of "facility to receive suggestions," and aside from its implying no tendency to action, but only the calling of an idea into notice--is seriously objectionable for the reason that "suggestion" was already an accepted term of psychology, and a quite indispensable one, in a totally different sense, before it was applied to hypnotic incitation. Namely, Hartley and the English associationalists, men whose own distinguished courtesy and freedom from insolence towards all philosophers--not to speak of their scientific merits--

must command the same treatment from men who really respect themselves, desired to appropriate the word "suggestion" to the calling up to the surface of consciousness of one idea by another idea associated with it. No term could be more apt; nor is it pleasing to see the terminology established by these masters hustled aside by their inferiors.

The first part of Dr. Sidis's book, then, is concerned with the laws of credenciveness, or incitability. He argues that its general law is, "Suggestibility varies as the amount of disaggregation, and inversely as the unification of consciousness." We are glad to find he uses the term "disaggregation" and not "dissociation"; for the implication of the latter term, that the phenomenon consists solely in the abrogation of habits of association of ideas, is incorrect. Both words were used by M. Pierre Janet, to whom the recognition of the importance of the matter is due. In ordinary parlance, we call it distraction of mind. For example, an artist who eats his luncheon while he paints, and in his absorption puts his pigment into his mouth, and without remarking its bad taste, yet spits it out unconsciously, exhibits a disaggregation of consciousness. It consists in the cutting of communications between two parts of the mind which are occupied with different matters. Drowsiness and slumber are conditions of extensive disaggregation.

The mathematical form in the phraseology of the statement we have quoted, which is repeated in all Dr. Sidis's formulations, is here and elsewhere quite meaningless, and is calculated greatly to repel all students of the mathematical sciences. If that be eliminated, there is nothing at all new in the statement, as Dr. Sidis's earnest tone of argumentation would lead one to suppose he thought there was. Everybody knows that if, while a man is writing out a check, he is carrying on a lively correspondence about a Rothschild, he may sign that name to the check instead of his own. What seems more original, in Dr. Sidis's account of the matter, is that he represents the credenciveness of waking persons and of hypnotic subjects as if they followed diametrically opposite laws, which he prints in parallel columns, thus:

THE LAW OF ABNORMAL SUGGESTIBILITY.	THE LAW OF NORMAL SUGGESTIBILITY.
Abnormal suggestibility varies as	Normal suggestibility varies as
direct suggestion, and inversely as	indirect suggestion, and inversely as
indirect suggestion.	direct suggestion.

But this seems to be merely an effect of exaggerated expression. Dr. Sidis would, we may hope, not himself maintain that the phenomena were really of fundamentally contrary characters; for this would subvert the whole doctrine of his book. It is merely that, the hypnotic subject being in a state of extreme mental disaggregation, we can give him sharp commands without fear that they will evoke the rebellion of another part of his mind which they never reach, while because of his disaggregation sharp commands are required. With a waking person, on the contrary, not sharp command but an underhand mode of incitement is requisite in order to avoid offending his egotistical susceptibilities. It would be quite unjust, and would show little power of weighing evidence, to say that the experiments in this part of the book are insufficient to establish a proposition so

thoroughly borne out by all our ordinary experience and instinctive knowledge of human nature. We do not doubt, though, that a good many psychologists will make just that criticism.

When, in commencing part ii., we find Dr. Sidis maintaining that what went before, the substance of which we have indicated, affords "strong proof" of there being "two selves within the frame of the individual," that is, that the subconscious parts of the mind are unified as if by a controlling consciousness, we are amazed at the width of his leap. But what are we to say when we find such experiments as the following put forth as "facts which directly and explicitly [note the inexactitude of this word] prove the same truth"? Upon the nose of an hysterical patient who complains of blindness of one eye, is placed a pair of spectacles of which the two glasses do not both transmit the light of any one part of the spectrum. (The author says of "complementary colors," but that is neither a necessary nor a sufficient condition.) The patient is then asked to read an inscription of which every other letter is covered by the one kind of glass and the others by glass of the other kind; so that, to each eye, half the letters must be invisible. The patient, however, promptly reads the whole. This proves that the patient has a preconceived idea that she is blind of one eye, which idea, acting upon her credenciveness, leads her to say (no doubt, to herself, as well as to others) that she does not see with that eye. But in what way does this begin to show that all the subconscious parts of the mind are organized into a single self?

When we find that all the facts adduced are equally impertinent, we begin to think that, just as Dr. Sidis overstated his own position in part i., so here in part ii. he does not really mean to say that there are just two selves in every man, but only that the conscious and subconscious parts of the mind are related to one another *somewhat* as the two selves of a patient with double personality. But if this is the case, he has no scientifically definite and novel proposition to enunciate; for everybody has perceived that there was *some* degree of analogy between the two classes of phenomena. It may be somewhat closer than has been supposed; but no contribution to science will have been made until we are informed definitely in what respect the analogy is close. Although it is not a positive contribution to science, however, the array of facts in this part of the book is striking and suggestive (if we may be allowed to use this word without being understood to mean "incitive to action").

In part iii. the author gives a slight account of some of those mental epidemics of which several French writers, beginning with Moreau, have made admirable studies. That the mob self is a subconscious self is obvious. It is quite true, too, as Dr. Sidis says, that America is peculiarly subject to epidemic mental seizures. In fact, it may be said that democracy, as contrasted with autocracy--and especially government by public opinion and popular sentiment as expressed in newspapers--is government by the irrational element of man. To discover how this can be cured, as a practical, realized result, without the ends of government being narrowed to the good of an individual or class, is our great problem. Prof. James seems to think this third part of the book is the best. We will defer to his judgment; but certainly a great subject here remains almost virgin ground for a writer of power.

67 (22 September 1898) 228-229: The Wonderful Century: its Successes and its Failures.

By Alfred Russel Wallace. Dodd, Mead & Co. 1898. 8vo, pp. 400, with 12 folding diagrams and author's portrait.

CSP, identification: Haskell, *Index to* The Nation. See also: Burks, *Bibliography*; Fisch and Haskell, *Additions to Cohen's Bibliography*; MS L 159.96.

Alfred Russel Wallace (1823-1913) was a British naturalist who is credited with having independently discovered the principles of natural selection as the key to the method of evolution while on a expedition to Moluccas in 1858. Wallace was also well known for his work on geographical distribution.

Mr. Wallace, in his preface, hesitates to declare categorically the purpose of this volume. He says, "It may perhaps be termed an appreciation of the century-of what it has done and what it has left undone." So considered, it is not a strong performance--is strangely incompetent for a strong man like Wallace. Does it not sound a bit like a school-boy's composition to hear an age criticised substantially in this fashion: "It has achieved some splendid successes, but it has fallen into some lamentable errors"? Of any human production whatever we know in advance that it will have its merits and its faults. What we ask of the major critic is to make it plain to us what the psychological qualities are, and what the experience and discipline have been out of which merits and defects have alike sprung. To do this for the nineteenth century, with the manifold agencies that have gone to make it what it is, is, no doubt, a problem of the most intricate. And yet there is one word that goes so far towards formulating the age, and is, at the same time, so obvious, that one cannot easily pardon its omission from the slightest description of the century. That word is Accuracy. To the spirit of accuracy (derived ultimately from the seventeenth-century mathematics, whose ideas the eighteenth had pumped into every cranny of thought) may be historically traced the larger part of the characteristic traits of the nineteenth century, even in cases where these seem to be of quite the contrary complexion. Of this Mr. Wallace tells us nothing. He never so much as mentions even precision in machinery as a vital factor in the evolution of some of out grandest ideas, such as the conservation of energy. The course of events was this: precision in the machine-shops made the application of the steam engine to ocean vessels practicable; the necessity of accurate economy of coal on those vessels stimulated, as their engines aided, the study of the theory of heat; the mechanical theory of heat easily suggested the conservation of energy.

No account of the achievements of the nineteenth century can be considered satisfactory which, like this, is confined to the physical and natural sciences, and the arts connected with them, and says nothing at all of projected geometry nor the theory of functions in mathematics, nothing of the logic of relatives, nothing of psychological measurements, nothing of the ascertainment of laws in the growth of languages, nothing of Egyptology nor of the decipherment of the cuneiform inscriptions, nor of the excavations about the Aegean and their results, nothing of the rewriting of every branch of history, nothing of Ricardo and later economists. Nor ought such a description to be confined to science: in poetry, romance, music, painting, our century may claim to have gone deeper than the

last. It is not altogether wanting even in improvements in the organization of society. Laws have been reformed, slaves emancipated, education extended, women treated seriously; sobriety, decency, and self-restraint generally, respected and demanded. To talk of appreciating the nineteenth century without the slightest thought of any of these things is so extraordinarily superficial that we are justified in suspecting that Mr. Wallace has not made public his real purpose in writing this book. The only part of it that is really vigorous is an argument of surprising force against vaccination. What the author really proves, however, is not so much the small efficacy of vaccination as the relative importance of other municipal and personal precautions. It is incontestable that a man may die of smallpox though he have been vaccinated ever so thoroughly; while he cannot have the disease at all if he is not exposed to its contagion. A recognition of this principle might save a good many lives, should we have a severe epidemic of smallpox next winter. Mr. Wallace's own conclusions go very much further; but in so far they are not legitimated by the scientific logic of statistics. It is curious, however, that he does show that the great falling off in mortality from smallpox at the beginning of the century was not mainly due to vaccination.

The whole argument occupies about a third of the volume. If this is germane to the professed subject of the work, it is difficult to say what would not be so. Had the author published this chapter as a separate essay, he had good reason to think it would not be read. If, however, his design was to seduce the reader into this chapter by prefixing 150 pages of light, entertaining discourse upon the glories of the century, further covering his purpose by appending some tame chapters on imprisonment, militarism, poverty, and the plunder of the earth, and if he threw in a couple of chapters in defence of phrenology, hypnotism, and psychical research (under which name he really squints at Spiritualism), partly because of his interest in the subjects, and partly to relieve the exceptional character of his chapter on vaccination, then the book has by no means been unskilfully put together.

One word about phrenology. Mr. Wallace claims for this doctrine the substantial support of modern cerebral physiology. This is audacious. No scientific psychologist will for an instant admit that the function of any part of the cortex of the brain can be accurately defined in terms at all resembling the marvellousness, veneration, etc., of Gall. Phrenology has been quite stagnant for half a century, a collapse in our day not at all likely to occur to an experimental doctrine not finally defunct. If it is not dead, let its students publish photographs and measurements of the heads of say a hundred of the men whose characters have become most publicly known and who have lived since the bumps were located, and there will be a mass of irresistible facts that will do more for phrenology than any amount of mere disputation. A phrenologist, Mr. Wallace tells us, said of him, "He is fond of argument, and not easily convinced." A disciple of Lavater might take the face of the frontispiece for that of an ecclesiastic rather than a scientific man, for whom it seems too argumentative. But for that trait, he would have been an excellent statistician. The same phrenologist said, "If wit were larger, he would be a good mathematician." Thereupon, Wallace, in his eagerness to advocate phrenology,

remarks, "Most great mathematicians are either witty or poetical; Rankine, Clifford, De Morgan, Clerk-Maxwell, and Sylvester being well-known examples." A man who justly prides himself on ability as a statistician should not have been guilty of that induction. A fair list of great British mathematicians among Wallace's contemporaries would be Hamilton, Sylvester, Cayley, Boole, Smith, Kelvin, and Stokes. None of them were remarkable wits, although two amused themselves with poetry. Of wit Wallace admits his lack. For poetry he seems to have some penchant, since he regales the reader with upwards of fifty elegant extracts, of the taste of which the following may serve as a sample:

"O Lavoisier, master great,

We mourn your awful fate,

But never tire of singing to your praise.

You laid foundations true,

And we must trace to you

The chemistry of our enlightened days."

There is a tolerable index, though it omits more than two hundred names of persons mentioned.

67 (29 September 1898) 242: NOTES

CSP, identification: Haskell, *Index to* The Nation. See also: Burks, *Bibliography*; Fisch and Haskell, *Additions to Cohen's Bibliography*.

Sir William Henry Preece (1834-1913) was an early pioneer of wireless telegraphy. He applied the fruits of his work in this field to the improvement of railway signaling.

'Wireless Telegraphy,' by Richard Kerr, F. G. S. (Charles Scribner's Sons), is a very popular little book indeed; there is not a dry word in it. Mr. W. H. Preece, an adept in the art, supplies a chernimmatic preface which was richly deserved. He goes as far as he well could--not to the point of justice.

Whoever reads the book should read between the lines of this preface. Mr. Preece, by the way, is at as much pains to do justice to Americans as Mr. Kerr seems to be to avoid mention of them. One blunder is worth notice simply because it is a frequent popular misapprehension. Mr. Kerr (p. 18) says: "It was conjectured by . . . Clerk Maxwell and others that light . . . and electricity . . . only differed in degree.... Their velocity ... was the same." What Clerk Maxwell repeatedly said, as in his posthumous book ('Elementary Electricity,' p. 98), was: "It is quite possible that the velocity of electricity in a telegraph wire may be less, say, than the hundredth of an inch in an hour." The ratio between the dynamical and statical methods of measuring electrical phenomena is a quantity of the dimensions of a velocity, and that velocity was shown by Maxwell and later physicists to be the velocity of light. Hence, we have reason to believe that the luminiferous ether is really electricity. Electricity is not a vibration, like light; it is a thing, attracting and repelling as other things, though very differently from other things. It is a fluid, though a peculiar fluid. To hesitate to call it so is simply to boggle over

names. Whether or not electricity is a chemical substance, or whether it be something *sui generis*, is a question probably needing a good many years definitely to answer.

67 (13 October 1898) 281-282: BALDWIN'S STORY OF THE MIND

The Story of the Mind.

By James Mark Baldwin. [The Library of Useful Stories.] D. Appleton & Co. 1898. 16 mo, pp. 236.

CSP, identification: Haskell, *Index to* The Nation. See also: Burks, *Bibliography*; *List of Articles*; MS 1513 (draft).

Here is a little book, easy to hold, pleasant to read, warranted to get read, without skippings, to its last word. Yet, after all, it contains some seventy thousand words--enough to outline any science, or, for that matter, all human knowledge, usefully. If the publishers will only remember that, while a narrow subject needs to be treated in some detail and at large, under pain of superficiality, a broad survey of a broad subject may have a high value, provided it be executed by a broad and deep student, they will do something for our enlightenment by this happily conceived series.

Probably among all our eminent psychologists nobody was better circumstanced than the author, by the interest and practical importance of the branch in which he has specially distinguished himself of late years, to make this outline of psychology. The two chapters about children are most charming. The characterization of the "motor child" is remarkable. Most of the chapters are skilfully and artistically constructed. The effort to maintain the extreme simplicity which is the idea of the series, and apparently to address an audience not at all in the habit of thinking, has had a questionable effect upon the chapter which, after all, was the most important, that upon general psychology. There is a certain limit beyond which it is impossible to simplify this difficult subject; the effort to do so can only result in loss of real perspicuity, especially to that public of psychologists by nature who are most likely to read the book, and who have been thinking about the mind, off and on, all their lives. We read that, "the electrician, say, cannot observe . . . the electric sparks without really using his introspection upon what is before him." If the meaning is that attentive observation of an outward object, without thought of self, involves all the introspection there is, would not the same idea have been more clearly expressed by denying that there is any such thing as introspection? But, in any case, the reader will sorely feel the lack of any account of what self-observation really consists in. We read: the "association of ideas, thinking, reasoning, etc., ... used to be considered as separate 'faculties' of the soul, and as showing the mind doing different things. But that view is now completely given up.... Mind does only one thing.... That one thing is combining." Perhaps this very strong language might be necessary in a book addressed to German readers, but the misapprehensions of many an English reader are quite

on the side of the *tabula rasa*; and the whole English tradition is that by faculties are meant mere logical classes. The classical British psychologists, whom it would be base for us to decry, as Germans do, far from considering the association of ideas as a special faculty (although they certainly did not deny that the mind "does different things," just as gravitation may produce orbital or hyperbolic motion or impact), did maintain that the one law of association, which Prof. Baldwin would call the law of "combining ideas," exclusively governs all the actions of the mind. Were this not otherwise plainly their doctrine, a moment's consideration of the theory of vibrations would render it so, in Hartley's case. When a school of students makes a scientific discovery, though it were not, like this, of the first order, and when, having demonstrated its truth as well as their historical position permits, they invent a special term to serve as the scientific name of what they have discovered, is it good morals for subsequent writers of the same language to assent to a narrowing of the meaning of that term, the purpose of which, in the first instance, was to deprive that school of the honor justly due them and transfer it to foreign plagiarists?

But, without insisting upon this point, it seems to us that many an American and English reader will do one of two things: either he will understand that Prof. Baldwin plants himself flat-footed upon the proposition that to feel a sensation of green, to make an effort, to acquire or break away from a habit, to experience fatigue, not only one and all consist in combining ideas, but are not "doing different things," or else he will be utterly puzzled at the outset as to what is meant, and will read no further. True, he can procure Prof. Baldwin's 'Handbook' in two volumes octavo, and thus ascertain just what he does think. But we cannot withhold the opinion that a further elucidation of general psychology would improve this little book, even if it had to be relegated to an appendix. In a certain recent work by a medical man we came across an emphatic reiteration of Claude Bernard's dictum that "disease is not an entity." That sounded like the proposition that the mind has no faculties, both being cases of routine clinging to a phrase of nominalistic metaphysics after its original meaning has completely evaporated. That the mind has no faculties was at first the shibboleth of Herbartian metaphysics, which has completely gone by. Though it is gross exaggeration--as if the human race were devoid of special instincts and as if there were no inborn differences of bent--still, it served in Germany to shake people out of a certain confusion of thought.

The only other chapter in which we discover any defect of lucidity is the last, concerning genius. If the word "genius" bears any unambiguous meaning, it surely signifies a very extraordinary native departure from the ordinary proportions of the faculties in a man, such as goes far toward fitting him for very extraordinary achievements. In this sense, there is no other department of science in which there is half the opportunity for genius that there is in mathematics. Hence, it is not a bad plan, when a generalization about scientific genius is made, to test it by comparison with the great mathematicians. Now, if we understand Prof. Baldwin, which seems hardly possible, in proposing as the criterion for distinguishing between the genius and the crank, that the true genius "and society must agree in regard to the fitness" of his ideas, and "for the most part his judgment is at once also the social judgment," he means that if the society of the day regard a man's ideas as unsound, he is no genius. But the history of mathematics simply swarms with instances in which work utterly neglected and despised in its day was found by a later generation to be fundamentally important, so that mathematicians proceeded to build upon it. The world had to grow up to it.

There was one Girard Desargues, who in 1639 printed a volume entitled 'Brouillon Project d'une atteinte aux évènements des rencontres d'un cône avec un plan.' Although Descartes praised Desargues in a private letter, as did the young Pascal in a work never yet printed, and Fermat somewhere (very likely on the margin of a book he was reading), yet he was so generally regarded as a crank by his contemporaries that early in this century he would hardly have been remembered at all, except for some sparse contemporary allusions to his "faiblesse pitoyable," etc. In fact, Montucla's great history of mathematics, enlarged by Lalande and extended by Delambre--five bulky quartos, which certainly did not intend to overlook any French name of the least account--does not recognize the existence of Desargues. As far as we are aware, no printed copy of the original book has ever yet turned up. But it happened one day in 1845 that M. Michel Chasles, the great geometer, started so early for the Monday meeting of the Institute that he lingered on the quay and began turning over the books exposed for sale on the parapet. He came across an old MS. copy of the book mentioned, which he purchased for a trifle. He took it home, and, having done so, he violated all book-buyers' manners by sitting down and reading it. He found it to be what, had it been written the day before, would have to be considered a very able treatise on that projective geometry which was rightly reckoned as the great glory of the nineteenth century in pure mathematics (though of course with important lacunæ), and he further found that Desargues had made an immense stride in advance of modern geometers in recognizing the fundamental importance of a relation which he called involution, and which, under the same name, is still regarded as a cornerstone of geometry! There are certain *minutiæ* of the history into which we cannot enter. The above account is as correct as its brevity permits.

Space does not permit us to set forth other like cases, not even a poor half-dozen selected from those of our own century--say the cases of Gaulois, Listing, Lobatchewski, Plücker, Green, and Hesse. Of these great names we find but two in the body of Phillips's Index; and one of these has no reference, so that it was doubtless inserted by the searching editor of the second edition. The man in vogue cannot escape the influence of the psychological law which causes him to desire to deny such facts; but they have their own sullen way of mutely but stubbornly continuing to exist. Happening to open the works of Beaumarchais at that piece in which his genius first found its strength, one would be surprised to read as its title, 'Le Barbier de Séville, comédie en quatre actes et en prose, représentée et tonbée sur le théâtre de la Comédie-Française, le 23 février, 1775.' A public may need a little education even to appreciate a farce. On the other hand, nothing is more amusing that to compare those whom the society of their day looked upon as immense with some contemporaries who passed unknown,

beginning with Alexander of Ales and Albertus Magnus against Petrus

Peregrinus and Roger Bacon. Prof. Baldwin speaks of the "supreme sanity" of Newton--a decidedly unfortunate instance from various points of view. But here we only note that since Newton considered his commentary on Daniel to be his greatest work, it follows that, according to the criterion seemingly proposed, he would have to be reckoned as no genius.

67 (20 October 1898) 300-301: **Logic, Deductive and Inductive**.

By Carveth Read. London: Grant Richards. 1898. 8 vo, pp. 323.

CSP, identification: Haskell, *Index to* The Nation. See also: Burks, *Bibliography*; *List of Articles*; MS L 159.94; MS 1411a (draft).

It was so many years since we had had the pleasure of reviewing a logical work by Mr. Carveth Read that we hoped, in opening this volume, to find that the long silence had ripened a rich fruit; and in point of fact experience has made of the author a wary defender of his doctrine. It is refreshing to meet with a logician of to-day who does not think he does a fine thing in putting logic upon a philosophical basis. The special sciences only occasionally have any need of considering the theory of reasoning, but philosophy can be successfully erected on no other foundation. Now if philosophy be founded upon logic, and logic in its turn upon philosophy, neither has any foundation at all. Besides, putting logic upon a philosophical basis always involves confusing the logical question of whether certain premises can be true, and can have presented themselves as they have done without the invariable (or almost invariable) truth of a certain conclusion, with the psychological question of whether the passage from premises to conclusion is gratifying to the logical sense. Mr. Read does not fall into this common confusion. The questions he discusses are genuine logical questions and are considered in their proper logical aspect.

The first sentence of his book reads, "Logic is the science that explains what conditions must be fulfilled in order that a proposition may be proved, if it admits of proof." This is a little narrow. There is no reason why the logician should be restricted to looking back from a foregone conclusion to possible premises, and never be permitted to look forward from premises in his possession to their necessary result. Besides, all logicians, including Mr. Read himself, make their science embrace the doctrines of definition and division, which cannot by any means be included under his definition. But Mr. Read at once proceeds to narrow this definition still further by excluding from the consideration of the logician all mathematical reasonings. He seems to think that these are coextensive with reasonings about quantity; as to which any modern mathematician could have set him right. Mathematical reasonings differ from other deductive reasonings only in their greater intricacy. The reason Mr. Read gives for this exclusion is that mathematics takes care of its own reasonings. It is very true that in mathematical reasoning there is no occasion to appeal to the theory of reasoning; but that is no evidence that the student of the theory of reasoning will not find any

advantage in studying mathematical reasonings. Both good sense and experience show that if, of two closely connected branches of science, the one has no need of appealing to the other, then the latter will be very apt to gain greatly by basing its principles largely upon the former. The intricacy of mathematical reasonings acts as a sort of microscope in bringing into plain view features of all deductive reasoning which without such aid could not be discerned. But the truth is, that Mr. Read is not altogether free from that common vice of the ordinary text-bookwriter of regarding as the most important aspect of his subject the fact that he has to teach it. When a man knows so little of mathematics as to lay down as one of the propositions that are past all denial that "all spaces are commensurable" (p. 142), although Euclid proves that the diagonal of a square is incommensurable with its side, it is plainly not convenient for him to say much about mathematical reasonings.

In induction Mr. Read stands upon the unmodified position of Mill. It is interesting to see how an experienced logician will defend this doctrine in 1898. The task before him is simply to answer two plain objections. Whewell's 'History of the Inductive Sciences' appeared in 1837. Its purpose was to show that success in inductive researches depends upon the student's coming to his subject provided in advance with appropriate ideas--a view to which the history of science since 1837 (particularly Darwinian ideas and those of physiological psychology) has brought much additional support. But John Mill saw in this doctrine an attack upon the associationalism in which his mind lived and moved and had its being. In truth, it was in conflict, not with the original associationalism of Gay, but with the *tabula-rasa* doctrine which, to a disciple of James Mill, seemed the lynch-pin of associationalism. And Whewell's pure metal was pretty thickly overlaid with slag, too. Whewell's doctrine was that appropriate ideas rendered inductive researches *successful*, not that they made induction a *valid* logical operation. But Mill did not very sharply distinguish between these two things. He wrote his 'System of Logic,' which appeared in 1842, largely to refute Whewell's philosophy by showing that it is not the appropriateness of our preconceived ideas, but the uniformity of nature *per se*, which gives induction its strength. It is necessary to bear in mind these circumstances in order to understand the true meaning of Mill's "uniformity of nature."

The first objection that Mr. Read ought to have noticed was that when Mill pronounced nature to be uniform, he meant in the general run of its characters; looking upon "characters" as all logicians since the Port-Royalists had looked upon them, as if they were so many self-subsistent things, of which the logician was equally bound to take account whether they appear to us important or insignificant, manifest or recondite, related to our powers of sense and thought or not. For if he only meant that nature is uniform in regard to such characters as we should be apt to attend to, his doctrine would simply relapse into that of Whewell, that our ideas are naturally appropriate to making inductive discoveries. If, however, the almost absurd idea of giving all characters equal weight is adhered to, it is susceptible of mathematical demonstration that any one universe has necessarily the same degree of uniformity as any other, since any collection of objects whatever has some character common and peculiar to it.

The second objection which Mr. Read had to answer was, that studies in the theory of probabilities made subsequently to Mill's writing have shown that, in any case, no peculiarity of this universe can be the sole support of the validity of induction, since in any universe whatever in which inductions could be made, induction would in the long run lead toward the truth. Mr. Read, however, overlooks both of these points, and bases his defence of Mill's doctrine mainly upon the proposition that "The Uniformity of Nature cannot be defined" (p. 141). This is indeed extremely prudent, but it places the modern Millian in the unrationalistic attitude of upholding a sort of Athanasian creed which he devoutly believes without being at all able to explain what it is that he believes, since he has learned that the moment he attempts to do so he falls into one difficulty or into another. Mr. Read would defend what is often called "Mill's account of causation," although it does not differ essentially from that of Kant, by the aid of the conservation of energy. Now there are countless facts which it seems hopeless ever to explain without supposing that Kantian causation is at least one of the factors of the universe. But phenomena governed by conservative forces are precisely those which are so utterly refractory to every attempt to bring them under any such formula, that they constitute a most serious argument against it.

67 (24 November 1898) 390: NOTES

CSP, identification: Haskell, *Index to* The Nation. See also: Burks, *Bibliography*; Fisch and Haskell, *Additions to Cohen's Bibliography*.

P. A. Lambert's 'Differential and Integral Calculus for Technical Schools and Colleges' (Macmillan) deserves mention for the remarkable power of condensation which it shows. Power of condensation in such a subject is a power of thought that breeds a similar power in the student. Substantially all that is practically needed of the calculus, in any ordinary application of it, is brought within the compass of a small volume, with an ample sufficiency of examples, many of which are practically instructive.

67 (24 November 1898) 397: The Story of Marco Polo.

By Noah Brooks. The Century Co. 1898.

CSP, identification: Haskell, *Index to* The Nation. See also: Burks, *Bibliography*; Fisch and Haskell, *Additions to Cohen's Bibliography*; MSS L 159.98-99, L 159.103.

Like the subject of the book here reviewed, Noah Brooks (1830-1903) was, himself, an adventurer and author. While a young man, he tried his hand several times at business, but he soon found journalism more to his liking and determined that exploration of America's ever widening western horizons would better advance this interest than would shopkeeping. So, in 1855, he joined a wagon train to California and from that journey derived many of the experiences of which he later wrote. Brooks was a personal friend of Lincoln, and two of his best works are Lincoln biographies.

This is substantially an abridgment, to perhaps 100,000 words, of Yule's 'Marco Polo.' It is, by a necessary consequence, highly entertaining to grown persons and to the young. Nevertheless, we cannot altogether approve of the manner in which it has been executed. Col. Yule's notes, although written in a popular way, are addressed to persons interested in geography and not ignorant of it. But the present volume, which does not contain a single map, nor any recommendation to consult one, is to go into the hands of persons to whom the location of the countries is not clearly known, and who have hardly heard of the most celebrated cities mentioned. Now the book of Marco Polo, if it is so read as to lend an interest to a study of historical geography, will afford a delight that nothing exhausts but the work's coming to an end; but if it is read without any definite ideas about the relative situation of the countries, the whole narrative produces a confused effect like the hum of many voices; the marvels overheat the air for one another, until the book is laid down as tiresome.

More information ought to have been given about the countries mentioned, and especially about the cities, as well as about many other matters. For example, the reader is throughout wearied with remarks about spelling, like this: "He also calls him Cublay at times, but most scholars give the name as Kublai." Now, since Marco tells us that he had in his early years in China to learn four different kinds of writing, why would it not have been well to anticipate the mention of this, and explain near the beginning of the volume that none of those modes of writing had any connection with our alphabet, that they were none of them alphabetical at all, but partly composed of ideographs and partly of syllabic signs, like all the most ancient writing? The obvious fact might then be pointed out that "Cublay" was one of Rusticiano's ways of representing the name he heard Polo pronounce; while "Kublai" is the spelling now generally used to represent what appears to be the same sound. When Rusticiano and Polo between them distort the name of a place, or use some other name, Mr. Brooks almost invariably remarks that it is "the modern" so and so, no matter how old the name may really be. Thus, we are told that "the Bastra of Marco Polo is the modern Basra"--the town which in our childhood's 'Arabian Nights' figured as

Bassorah, which we accented on the penult. But there never was a t in the name of this town; Bastra is simply a forgetfulness of Polo, or a mishearing or euphonic change of Rusticiano--probably the last.

In these and other places, Mr. Brooks seems perfectly indifferent to imparting correct information. Of course, the limitation of the text of Col. Yule--who never sufficiently reflected that the narration of journeys had long been diligently studied as an art by Polo, as he himself gives us to understand, and that he could tell his story in different spirits to suit the taste of different scribes--remains as a fault of the present volume.

67 (1 December 1898) 417-418: Radiation:

An Elementary Treatise on Electromagnetic Radiation and on Röntgen and Cathode Rays.

By H. H. Francis Hyndman. London: Swan Sonneschein & Co.; New York:

Macmillan Co. 1898. 8vo, pp. 307.

CSP, identification: Haskell, *Index to* The Nation. See also: Burks, *Bibliography*; Fisch and Haskell, *Additions to Cohen's Bibliography*.

Somewhat more than half this book is devoted to the properties of electromagnetic radiation--that is, of light, of the discharges of Leyden jars and oscillators, and of various other kinds. This part of the book answers its purpose; but we shall say no more of it, because the chief interest of the work lies in its collecting all that is essential in our knowledge of the newly discovered kinds of radiation.

The discussion necessarily begins, just as the history of discovery does, with the so-called *negative* rays of Crookes. These rays can exist only under pretty high exhaustions. The negative rays can pass through glass; but if they are focussed on a part of the tube where thin aluminium, and not glass, is the material of the wall, a faint glow will be perceived outside. In fact, there are there, not negative rays, but another kind of rays, called cathode rays. These rays are susceptible of reflection and refraction; their interference is a disputed question; their polarization has never been effected. They do not affect the eye. Their velocity of propagation is less than that of light, suggesting that they reside in a substance similar to, but distinct from, the luminiferous ether. When the current, instead of being steady, is rapidly intermittent, then, without any piece of aluminium, but with some focussing, X or Röntgen rays are emitted. These rays can be reflected, especially from zinc; but no interference or polarization of them has ever been discovered. Differently produced X-rays differ greatly in their penetrative power.

Becquerel rays are rays having peculiar properties, which are emitted by salts of uranium and of other metals. The metals copper and platinum are readily transparent to them; silver and tin are less so; zinc and lead are opaque. Becquerel rays are readily polarized; which shows that they are only ultra-violet-light rays. And the contrast to the X-rays in this respect ought to throw a light on the nature of the latter. "Le Bon's rays" are rays by which photographs can be taken from which the light is shut off by a sheet of iron. "Discharge rays" are rays emitted by an electric spark. They are, no doubt, merely ultraviolet rays. The light of fire-flies, glowworms, etc., is known to be peculiar, but there is no reason to suppose it other than ordinary light peculiarly sifted.

We cannot say this book is skilfully written. A certain number of facts are overlooked, and it seems almost a matter of chance as one reads whether anything will get mentioned or not. It has evidently been composed by taking all the memoirs on each branch in chronological order, and making an abstract of each; but unless every single statement of every memoir is included, this procedure will result in omissions. If all statements are included, the abstract must itself be digested with pains and skill to make a thoroughly good book. However, the present work is not very faulty in its omissions.

67 (22 December 1898) 469-470: DARWIN'S TIDES

The Tides and Kindred Phenomena in the Solar System.

By George Howard Darwin. Boston: Houghton, Mifflin & Co. 1898. 8vo,

pp. 378.

CSP, identification: Haskell, *Index to* The Nation. See also: Burks, *Bibliography*; *List of Articles*; MSS L 159.99, L 159.101; MS 1411b (s) (draft).

George Howard Darwin (1845-1912), the son of Charles Darwin, was a mathematician and astronomer. He received his formal education at Trinity College, Cambridge, taking a B.A. degree in 1868. He served as Plumian professor of astronomy and experimental philosophy at Cambridge from 1883 until his death. Darwin was an authority on tidal theory, geodesy, and dynamical meteorology.

There are probably not a great many people who feel any burning desire to know just how the tides behave, and just why they behave as they do, and just how predictions of them are made; so that this book may not be much sought after at the mere mention of its title. Readers of it must pass around from mouth to mouth how interesting it is, and how much the author had made of a seemingly most refractory subject. He has shown us that it is possible to write a popular book upon a branch of mathematics without childishness, without sensationalism, and to give it a real value for him who is versed in the science as well as for him to whom it is all new.

The variety of topics that are found pertinent to the general theme is astonishing. No less than twenty chapters treat of nineteen different questions, each of great interest quite apart from the rest, but at the same time the whole nineteen have such a unity that we cannot say that any one of them could have been omitted without serious detriment to the general sketch of this branch of science.

All these topics are so interesting that we hardly know which ones to choose in order to give our readers an idea of the contents of the volume. Perhaps chapter ii., partly (no doubt) from its giving the first marked impression of the work, is as striking as any. It relates to "seiches" in lakes. The word "seiche" is vernacular, we believe, only on the shores of Lake Leman, where alone this phenomenon has attracted the general attention of the inhabitants. It is a rise and fall of the water on the shore of the lake of a few inches, sometimes with a period of about an hour, sometimes in a less regular way. This phenomenon has been investigated, not by a mathematician or physicist, but by a naturalist of the school of De Saussure and De Candolle (not to leave the shore of the lake for exemplars), Dr. Forel of Lausanne, a town well placed for observing the seiches. It is interesting to see the methods of observation of this naturalist in physics; they are very ingenious, and are such as a physicist would hardly have lit upon. The observations render the nature of the seiches quite evident. The commonest are just like the slopping of a tea-cup carried in the hand--that is, the whole mass of water of the lake rocks about a nodal line in the middle. In other seiches there are two nodes or even more; but these are infrequent. It is important, in view of phenomena presently to be mentioned, to note the periodic times of these oscillations. The uninodal seiches are observed to have a period of 73 minutes, the binodal of 35 minutes. Ordinary visible waves on the lake have a period of 4 to 5 seconds. Intermediate between these there are waves having a height of from 1-25 to 1-10 of an inch, and of periods of from 45 seconds to 4 minutes. Of course, they can be observed only by a delicate recording instrument. Some of these waves, which Dr. Forel designates as "vibrations," are due to the wind, but the most interesting ones are caused by steamboats. When a steamboat running at full speed is still twenty-five minutes from the pier, as soon as she rounds a point that gives a clear way for the wave the vibrations are plainly marked upon the instrument. After the boat leaves, they are much more violent, and it is more than two hours after her departure before they disappear. These waves are entirely unlike anything that can be seen with the naked eye, as their periods show.

As we have space only for one other example of the interest of the matter of this volume, we will pitch almost at haphazard upon chapter xvi., on tidal friction. Since the tide-wave moves with the moon and sun, from east to west, while the earth rotates daily from west to east, it is plain that the friction of the tide against the earth tends to retard the earth's rotation. Now, since it is the action of the moon that mainly makes the tidal wave, and, therefore, thus retards the earth, and since action and reaction are always equal and opposite, it follows that the moon in thus drawing the earth towards the east must itself be drawn towards the west. But that is the direction in which the moon is moving in its revolution round the earth; and thus the motion of the moon in its orbit is accelerated, as the comparison of ancient and modern observations shows it to be. This acceleration of the moon's velocity causes it to fly off a little further from the attracting centre of the earth, and enlarges its orbit, and the complete revolution in this enlarged orbit takes more time than that in the unenlarged orbit; but since the day is also considerably longer, there are fewer days in the month than there were at first. All this is familiar to everybody who has recently read a good treatise on astronomy. But at this point Mr. Darwin starts a most interesting question. Namely, he asks when this process of gradual recession of the moon from the earth could have first begun. Clearly it must have been going on whenever the moon was making tides upon the earth. It must, therefore, be traced back to the time when the moon was so close to the earth as to form one mass with it, when it would, of course, make no tides. But however the moon came to be thrown off from the earth, it would be likely to preserve its linear velocity tangential to the earth, while, the circumference in which it moved being larger than the earth, the month from the very start must have been longer than the day, which is sufficient to insure the recession of the moon. M. Poincaré, a mathematician of great genius from Nancy, has shown that one form that a mass of liquid rotating about an axis in equilibrium might have, would be a pear-shape, the axis of the pear being at right angles to the axis of rotation. Now there can be little doubt that the

protuberance at the stem of the pear would tend to become a bulb and finally to be thrown off from the rest. What would ensue? The tides caused by the earth's attraction in the satellite so formed would tear it to fragments. The fragments near the earth would be just small enough to hold together, those further away would be larger, while the part of the satellite farthest from the earth would form a satellite of such a size as to be distant from the earth about two and a half of its diameters, which would be the moon. The small pieces nearest the earth would probably return to the earth. Those somewhat further away may very likely be there yet; for any appearance of them could be distinguished from the zodiacal light only by more careful observations than any that have yet been made. This theory of the genesis of the moon seems to us, we must confess, not so "wild a speculation" as the author is willing to grant that it is.

The only chapters of the book with which we cannot feel ourselves quite satisfied are those which deal with mathematical arguments. We are here sometimes at a loss to understand what it is that the author is aiming at. He seems to be *explaining* the reasoning of the mathematician. But mathematicians, especially when they are dealing with the most difficult applications of mathematics, have not been inventing abstruse and difficult ways of reasoning; on the contrary, they have been trying with all their might to find the simplest and easiest ways; and they are men of great genius and training in finding out simple methods of reasoning. By far the shortest way to understand the reasoning of the mathematician about the tides is to begin by buying a book on the calculus; and when that is mastered, to go through with the rest of the course required for a thorough understanding of hydrodynamics. Any pretended "explanation" of the reasoning shorter than this either is fallacious, or covers only a small and insufficient piece of the reasoning for even a vague conclusion, or it is open to both criticisms.

The author, in his preface, has this remark:

"A mathematical argument is, after all, only organized common sense, and it is well that men of science should not always expound their work to the few behind a veil of technical language, but should from time to time explain to a larger public the reasoning which lies behind their mathematical notation."

There is more than one fallacy here. In the first place, the term common sense, the middle term of the first half-expressed syllogism (whose strict conclusion Mr. Darwin would not have found it convenient to state), is ambiguous. Common sense, in the proper acceptation of the term, resides mainly in the subconscious department of the mind. It informs us how things go in this world of ours, and in this world exclusively. It has nothing whatever to say about the pure hypotheses of the mathematician, and therefore has no bearing whatever upon mathematical reasoning. But it appears that in the sentence quoted something quite different is meant, which is called "common sense" only because these words impart that popular tone and the *bonhomie* which the author is endeavoring to assume. What seems to be meant is, that each step of the mathematical argument is perfectly evident to the vulgar equally with the learned, to the mathematically dull as much as to the mathematically bright, *provided he clearly apprehends the premises*. But this proviso

contains the whole difficulty. Every mathematical premise contains so many elements, so intricately related, that most minds are unable to apprehend the proposition distinctly. The abibility [[sic]] to do so is precisely what makes the mathematical mind. It is impossible to dissect a mathematical reasoning so that the mathematically very dull can apprehend it, because, when the dissection reaches a certain point, the logical relations become different; and the result is a fallacy.

It is not true, then, in any acceptation of the terms, that a mathematical argument is only organized common sense. As for what is said about a "veil of technical language," it involves an egregious begging of the question. The technical language of science is composed of words intended to aid the search after truth by facilitating the work of the mind in dealing with complicated conceptions. If, instead of fulfilling that purpose, it is that Talleyrand dialect which veils the thought, it ought to be altogether spurned, and will be so treated by every real devotee of science. But the "language" which Mr. Darwin has in mind is not speech--it is the language of algebra and the calculus. To the disciple of Lagrange and Laplace, the analytical formula is simply the most perfect possible description of the hypothetical phenomena. It is something into which geometrical representations ought to be translated, being itself as near pure thought as it is in the nature of thought to be. When it comes to such a question as the phase of a forced oscillation, especially of an oscillation in two dimensions (and such is the problem of the tides), the frankest way is to leave the mathematical argument untouched in that utmost simplicity to which generations of the most skilful reasoners have been able to bring it. By all means illustrate any steps of it that you can, by parallels drawn from familiar experience; but do not attempt to "explain" that which, on the contrary, must explain your explanation. Is there one bicyclist in five hundred who thoroughly understands why his wheel behaves as it does? Is there one in fifty who does not imagine that it stands up because of its rectilinear velocity? How utterly visionary, then, it is to attempt to popularize the mathematics of any less familiar and still more intricate subject.

1899

68 (2 February 1899) 95-96: Matter, Energy, Force, and

Work.

By Silas W. Holman. Macmillan. 1898. 8vo, pp. 257.

CSP, identification: Haskell, *Index to* The Nation. See also: Burks, *Bibliography*; Fisch and Haskell, *Additions to Cohen's Bibliography*; MSS L 159.106-108, L 159.110.

Very competent physicists may have a difficulty in forming perfectly clear conceptions of the fundamental ideas of physics; and the more difficulty they have, the more likely they are to want to write books on the subject, and the more deleterious those books will be. These are the minds which neglect the maxim of logic that the meaning of a word lies in the use that is to be made of it, so that every term of general physics ought to stand for a definite general phenomenon; and whoever clearly apprehends to what phenomenon a physical term refers, has nothing futher [[sic]] to learn about that term except its grammatical construction. For instance, the word *mass* serves to express the law of action and reaction. If *mass* is defined as "the quantity of matter," then what is meant is the quantity of matter as measured by action and reaction. Mass, therefore, has to be distinguished from *weight*, if by weight we refer to the pull toward the earth against the elasticity of a spring-balance. But to introduce distinctions of terminology which refer to no differences in the phenomena, is an idle pedantry that only confuses at once the language and the ideas of students, and puts them out of *rapport* with the body of scientific men. Prof. Holman's word "weightal" is as superfluous as it is unbeautiful.

The only way to keep scientific terminology free from confusion is to recognize the right of him who introduces a given conception into science to confer upon it its scientific designation and symbol, which should never be rejected nor changed except for really substantial reasons, such as the previous use in another signification of the word chosen. No man of sense will upon any light occasion violate all usage in this matter, any more than in any other. For instance, the word *gravitation* is appropriated by all writers to that fixed attraction between distant bodies which varies only as their mass, while *gravity* is used for the acceleration of bodies toward the earth under the influence of gravitation combined with centrifugal force. It is, therefore, injudicious for Mr. Holman to attempt to reverse this practice by calling that "gravity" which is known as gravitation, and that "weight" which is known as gravity. He seems to be particularly enamoured of the word "kinergety" for kinetical energy; but it is not likely to be adopted.

As an example of the want of clearness of the book, we may take the following, which is printed in italics: "The sufficient evidence that all resistance is due to the action of energy lies in the fact that through resistance change in state of motion of bodies occurs." If, however, by "due to the action of energy" is meant, as should be meant, due to the production by kinetical energy of changed positions with changed positional energy, and the production by the distribution of positional energy of accelerations working changes of kinetical energy, then it is plain that, unless the conservation of energy be assumed at once, a resistance

need not be "due to the action of energy." Great fallacies may lie hid behind the word "due."

Le Sage's theory of gravitation is discussed, without being criticised from a logical point of view. But if this theory is proposed in the hope that impact and a wonderful elasticity of incompressible bodies may supersede positional energy, then it would seem to be a blow aimed at the ideas of the differential calculus and of logic itself; for it would be an endeavor to form a conception of nature as discontinuous, and consequently as radically unintelligible as possible. If, however, positional energy and action at a distance are not to be attacked, why not admit that gravitation is such an action, until some facts are ascertained to the contrary?

The vortex-atom theory is regarded with great favor by Prof. Holman. Yet, though mathematically only too profound, it is logically not much better than the theory of Le Sage, being an attempt to get rid of action at a distance in another way. As for Prof. J. J. Thomson's verifications of its results by chemistry, they are too trifling to have much weight, not to speak of the difficulties they involve. There is nothing but *a priori* metaphysics against action at a distance, which is indissolubly bound up with the principle of energy.

Prof. Holman allows himself to treat with silent contempt Newton's theory that space is an absolute entity, although it is a scientific doctrine based upon the fact that bodies tend to preserve their absolute aspects of rotation. He falls into German metaphysics in accepting as self-evident Leibniz's hypothesis that space and motion are entirely relative, a notion unsupported by facts. Some of the German upholders of this doctrine say that bodies do not preserve their planes of rotation absolutely, but only relatively to an otherwise unknown body, which they name "Body Alpha." This "Body Alpha" is for all intents and purposes identical with Newton's Absolute Space. Dr. Ernst Mach wishes to substitute for Body Alpha the *tout ensemble* of the bodies in the universe. The idea that a distant star by its motion should instantly affect the rotation of a top, not by a physical force, but by a principle of dynamics, is contrary to all experience, and subversive of the validity of space as a representation of the relations of things. These people maintain that it is just as true to say that the earth stands still while the heavens move round it, as the reverse; so that we may say without falsehood that, by moving round the earth, the stars produce the phenomena of centrifugal force on the earth. This is action at a distance, with a vengeance. It is to be remarked that the preservation of the plane of rotation depends upon the law that a body unacted on by any force moves in a right line; and if the aspect of the plane is not absolutely preserved, then the body moves in a straight line, not absolutely, but only relatively to Body Alpha or to whatever substitute for that fetish may be imagined. All this because the Leibnizians obstinately adhere to a metaphysical notion that does not fit the observed facts.

Geometers are unanimously agreed that if is impossible to prove that the sum of the angles of a triangle equals two right angles except by a premise as little axiomatic as Euclid's celebrated postulate concerning parallels. But if it be axiomatic that all motion is relative, there is no difficulty about the triangle. For in

that case two bodies may have any velocity in any direction and yet remain at rest relatively to one another. Now, this is impossible if the sum of the angles of a triangle is greater or less than two right angles.

68 (16 March 1899) 210: **Leibniz: The Monadology and Other Philosophical Writings**.

Translated, with introduction and notes, by Robert Latta. Oxford: Clarendon Press; New York: Henry Frowde. 1898. 8vo, pp. 437.

CSP, identification: Haskell, *Index to* The Nation. See also: Burks, *Bibliography*; Fisch and Haskell, *Additions to Cohen's Bibliography*; MS L 159.102; MS 1413 (draft).

That a thorough and critical acquaintance wih [[sic]] the philosophy of Leibniz is an indispensable preliminary to a successful study of Kant and to a just appreciation of German philosophy, does not, perhaps, render this publication particularly opportune; for the endeavors that have been made to resuscitate Hegel in English-speaking countries seem for the present to have failed. But, at any rate, it may be considered as opportune on account of the number of ideas, now universally essteemed as highly fruitful, which trace their origin to the Hanoverian philosopher. The Columbus of the subconscious mind, the discoverer of mechanical energy, the joint inventor of the differential calculus, and, more than all these, the great promulgator of the law of continuity (understood by himself to include historical continuity, and, as he was dimly aware, supposing an evolution of all things and all laws from a primal chaos), is a figure to excite the curiosity of thinking men of the present day.

One might, at first blush, wonder that so big a book--though the light Oxford paper makes it easy to hold--should be devoted to a writing which in Erdmann's edition does not fill eight pages. There is, however, ample justification for that. Leibniz is not yet a convenient author to study. Before the completion of Gerhardt's edition of the Philosophical Works in 1890, the state of things was calculated to daunt a pusillanimous student. Even now the philosophical writings are contained in seven volumes; the mathematical works (which furnish the only key to Leibniz's thought) fill seven more by the same editor, and the mathematical correspondence has yet to be printed. The historical and political writings, which ought not to be neglected, are in ten other volumes edited by Klopp; and in order to possess all of Leibniz that has seen the light, the student must procure fourteen additional volumes (seven edited by Foucher de Careil, six by Dutens, and one by Mollat), besides those which are now in course of publication. It was, therefore, a praiseworthy act, as well as a bright idea, to take that unnamed paper known as the *Monadologie*, which compresses the metaphysical system of Leibniz into the smallest possible compass, and make it the subject of a full exposition by means of parallel extracts from other writings of the same author. For Leibniz, as a writer of papers and not of books, often repeats his thought in more and more developed forms. A true scientific man, he never held to any opinion as final and irrevocable, and he never ceased to learn and to grow.

Dr. Latta's work has been performed with erudition and good judgment. The plan once settled, the only ground of complaint with the manner in which it has been carried out is that the original French and Latin ought everywhere to have been given in place of English versions. As for the plan itself, the chief fault of it is, that, in consequence of the whole commentary being made to refer to an exceedingly compressed statement of Leibniz's metaphysical system, it hardly presents a broad view of his whole thought; and, in particular, it is a pity that the logic of so eminent and original a logician--life and soul, as it is, of his whole philosophy--should not have been more completely illustrated.

The first hundred and fifty pages of the volume are occupied with a general exposition of the philosophy, correct and full, but not deeply critical. An "estimate" thereof fills the next sixty. There will be found historical information concerning the relation of Leibniz to the scholastics and to Descartes, and concerning the relation to him of Kant, Fichte, Schopenhauer, and Hegel. Nothing is said of later German thinkers who, down to Ernst Mach, inclusive, have been influenced in no inconsiderable degree by this philosopher--at any rate, indirectly, whether directly, too, or not. What of criticism this "estimate" contains regards the subject from a Hegelian point of view; for Dr. Latta seems to be one of those Britons who clean forget the thought which is their patrimony, in their admiration for the "profundity" of philosophers who attack the most difficult problems without having had the courage to go throught [[sic]] the tedious investigations which should have come first. But it will hardly be thought by anybody that Dr. Latta's criticism of Leibniz is particularly strong or helpful. This is unfortunate, because, while the reasoning of Leibniz was nearly, if not quite, of the highest order, being far more accurate than that of Kant or almost any metaphysician that can be named, and abounding in luminous, simplifying, and fecund methods, yet he seems to have had a sort of blind spot on his logical retina that rendered him capable of accepting tremendous inconsistencies and absurdities. Witness his works upon the "Great Art" of Raymund Lully--a sort of Keely motor for churning knowledge out of ignorance--in which the man who swayed German thought so long maintains that all truths, theoretical and practical, can be mathematically demonstrated from two premises, one that he expresses thus: "Quod est (tale) id est, seu non (tale) vel contra," while the other is, "Aliquid existit."

He is a declared nominalist, and his theory of monads breathes nominalistic individualism. But he strangely fails to see how contrary to all this is his law of continuity; and it is still more curious that he found himself, at last, forced to revive the substantial forms of the mediæval realists. It will occur to almost every mind that for each Leibnizian monad all the rest are superfluous and nonexistent--a manifest absurdity; and that so great a reasoner should not have seen the inconsistency of supposing God to be one of those monads, is quite astonishing.

In his fourth letter to Clarke, he offers, as an argument in favor of his logical "principle of the identity of indiscernibles," the fact that a nobleman of his acquaintance, on hearing it enunciated, long searched in vain to find two leaves of trees exactly alike--not seeing that this was a much better argument against the principle than for it. For the proposition is that things precisely alike *could not conceivably*

be two. Now, the very fact that one may spend a long time in trying to find such a case proves that it is quite conceivable. Here lies one of the capital errors upon which the Leibnizian metaphysics comes to wreck; namely, that he does not see that existence is no general predicate or intellectual conception, but is an affair of brute fact. Was it not Carlyle who said that the very hyssop that grows on the wall is there only because the whole universe has not been strong enough to hinder it? This falsifies, too, the other principle which Leibniz in the same letter lays down as fundamental, the law of sufficient reason. There is no proving existence, as he himself once remarked; for though a thing be in itself possible, it may not, in his phrase, be "compossible" with other things which have forestalled it in the struggle for existence. Leibniz fancies he answers this objection by saying that God has created the best of all possible worlds; but that this proves itself upon discussion to be a quite meaningless proposition has long been apparent. Nor is this the only such objection to the law of sufficient reason, for nobody has answered the old question what reason there can be why red and blue light should not excite each the sensation that the other does excite. But though the doctrine that *everything* has a sufficient reason is thus untenable, yet it still may be true that reasons ("raisons"), that is, final causes, should be really operative in the universe. Only, this cannot consistently be maintained by a philosopher who insists upon denying the reality of all generals; unless, indeed, he resorts to the device of supposing a Deity in whose mind those reasons and purposes should reside--his nominalism probably passing to the conceptualistic variety. But what, after all, is such a theological nominalism but the attribution to the system of generals, not only of reality, but also of life?

Such weaknesses of the logic of Leibniz are quite overlooked by Dr. Latta; although it is not easy to comprehend how they can fail to suggest themselves to any mind trained in British strictness of reasoning.

Next in the volume comes the Monadology, which copious and pertinent notes, chiefly expository by parallel passages, expand to some sixty pages. The remaining hundred and fifty pages are given to sundry papers of Leibniz allied to the Monadology, together with the introduction to the 'Nouveaux Essais.' But we believe we need say nothing of these, since, if our views are at all correct, it must already sufficiently appear what praise and what blame are to be adjudged to this work.

68 (20 April 1899) 296: NOTES

CSP, identification: Haskell, *Index to* The Nation. See also: Burks, *Bibliography*; Fisch and Haskell, *Additions to Cohen's Bibliography*; MS L 159.113.

Ludwig Günther of Stettin publishes a German translation of Kepler's

'Somnium,' a study of how astronomy would appear upon the moon (Leipzig: Teubner). The name of Günther (probably identical in origin with Gonthier, the family name of the Schwartzburgs) is already represented in science half-a-dozen times over, without counting the English Edmund Gunter. We are glad to make a new

acquaintance worthy of the family. Prof. Sigismund Günther of Munich is a learned and delightful writer on the history of the mathematical sciences. He has sometimes been accused of too great originality, but Herr Ludwig altogether surpasses him in a title-page which is "ein genaues Faksimile" of the original edition of the 'Somnium,' except that it is somewhat reduced, and except that it is translated from Latin into German! If the volume had no other interest, it would be worth getting for a remarkable portrait of Kepler, looking much more mathematical and less witty than Nordling's, which, however, Kepler himself said was not a good likeness. That is beetle-browed; this has a low, retreating forehead. The nose, cheeks, and hair leave no doubt that this really is Kepler, and it is impossible to resist the conviction that it is a likeness. It is dated 1610, the year after the publication of the 'De motibus stellae Martis,' when Kepler was in Prague.

68 (27 April 1899) 316-317: **A History of Physics in its Elementary Branches**.

By Florian Cajori. Macmillan Co. 1899.

CSP, identification: Haskell, *Index to* The Nation. See also: Burks, *Bibliography*; Fisch and Haskell, *Additions to Cohen's Bibliography*; MS L 159.105; MS 1414 (draft).

Prof. Cajori's 'History of Mathematics' has proved a useful book, notwithstanding the fault that was found with it for not being a kind of book it was never designed to be. Should a similar mistake be committed about the present work, however, it will be in a measure the author's own fault; for in a preface he quotes from the chemical leader William Ostwald some sentences which, in German verbose eloquence, express the idea that scientific teaching is not sufficiently historical, and thereupon hopes that this book may do something towards remedying that defect. This, being the main substance of the preface, seems to declare the purpose of the book. But what Ostwald wished to recommend, if anything more than the perusal of classical memoirs, was probably text-books on the plan of Mach's 'Die Mechanik in ihrer Entwickelung historisch-kritisch dargestellt' (a good translation is published by the Open Court Co.), which imparts a very clear notion of the fundamentals of the science in a quite admirable account of the historical evolution of its conceptions; adulterated, unfortunately, it is true, with some baseless metaphysics. Since 99 per cent. of those who study chemistry, as of those who study mechanics, pursue it simply with a view to making industrial applications of their skill, we have little doubt that, for their purposes, Ostwald is quite right in confessing that the unhistorical methods are "very successful," contrary to the contention that there is economy in the historical way of teaching. But, however that may be, in what manner Prof. Cajori can imagine that an anecdotal and "crisp" (we will not say newspapery) narrative of physical discovery can subserve the historical plan of inculcating profound ideas of physics, is not very clear.

All that can be expected in a volume which compresses the whole development of elaterics, thermotics, acoustics, optics, electricity, etc., into a hundred thousand words, is a sketch of the most exterior facts for those who come to it utterly ignorant, together with entertaining reminiscences and perhaps some stray, forgotten circumstances for those who have been over the ground before; and so much this volume certainly gives us. It is impossible to blame the author for not introducing us into the inner current of physical thought, except when he himself, by direct pretension to discuss the vexed question of the reason for the failure of ancient physics, renders it impossible not to notice this side of the work. All our studies of scientific methods during the last half century have gone to confirm Whewell's sagacious induction of 1837, that scientific discoveries cannot be made until appropriate ideas have first grown up. For example, the fact that Aristotle could assert that heavy bodies fall faster than light ones shows that his ideas were not in that state of preparation for the subject which would insure its occurring to him that, whether two bodies of equal weight falling side by side were welded together or not, could make no difference in their rate of falling, unless a strain upon the welding would necessarily be brought about; and so long as such ways of thinking would not be sure to occur to him, he was plainly incapable of devising any suitable experiments relating to the phenomenon, as well as of reasoning from them rightly had they been brought before his eyes. Very few have been the exact general propositions drawn from history, perhaps none before Whewell's date, so eminently instructive as this; for it shows us that science is not unmixed receptivity, but essentially involves a conceptual element that has to go through a period of growth and a process of ripening. There is a certain psychological naivete, therefore, in Prof. Cajori's bringing forward in 1899 the objection (borrowed from the most anti-historical of all modern schools of philosophy) that Whewell does not explain why such quick-witted folks as the Greeks should have failed to catch appropriate concepts; as if concepts were things that bright minds could always pluck at will. Certainly, Whewell's law does not pretend to explain everything about its subject-matter. That is a character it shares with the theory of evolution through variations at birth, and indeed with all genuine scientific inductions. But it does render the sort of cavil noticed a mere *ignoratio elenchi*. It is true enough, as Prof. Cajori says, that the ancient Greeks were not good physicists because they did not care seriously for physics and had no turn for it. But the reason why they did not care for it and had no turn for it was that they had not yet grown up to it, nor developed the ideas appropriate to that study. In later times, they turned out extremely successful with such branches as by the growth of appropriate ideas they were prepared to study.

Prof. Cajori distributes his space justly both among the different branches of physics and among the different periods of history. The Greeks get 1-24 of his 300 pages, the Romans 1-150, the Arabs 1-75, the Middle Ages 1-37, the Renaissance 1-12, the seventeenth century 1-6, the eighteenth 1-8, and our own 5-9. The natural consequence is that the book gets better and better the further one reads. The very best chapter is the very last, on the evolution of physical laboratories. On the other hand, a person who could not off-hand furnish a more satisfactory account of Greek physics than is here to be found, could hardly be reckoned as ordinarily well informed on the subject. No doubt, Prof. Cajori could have done much better. But he has given such rough characterization as the space to which he

restricted this period would permit, as long as it was assumed that the reader was pretty thoroughly unacquainted with the Greeks beforehand.

Whatever all the similar modern compendiums get right this book gets right, and where they are apt to slip, this book is pretty sure to come to grief along with them. Thus, Mach having raised some purely gratuitous objections to the statical reasoning of Archimedes, prompted thereto by his metaphysics, we find Cajori only willing to admit that Archimedes "endeavored to establish" the principle of the lever. Good logic and good sense go with Lagrange in the opinion that the demonstration is perfect, epochal, and superbly ingenious in the highest sense. At any rate, if Prof. Cajori would only attend to the meaning of the word "establish" in English and not of *festgestellt*, or *bestäligt*, or *begründen*, in German, we think he must admit that, whether the proof was indisputable or not, the principle was, as a matter of historical fact, *established* by Archimedes. In like manner, he meekly falls into the train of those German commentators who have blunderingly accused Galileo of fallacious reasoning in his refutation of the hypothesis that the velocity of a falling body is proportional to the space described from the state of rest. The most that ought to be admitted is that, in reproducing at eighty years of age his reasonings of sixty years before, he does not set them forth with quite sufficient fulness; but that the reasoning itself, once it is fully stated, is perfectly sound, is quite beyond dispute. He assumes, of course, that the time of the fall is not infinite, and on that basis asserts that, were the law as supposed, the time of falling the first four yards would be no longer than the time of falling the first two. His suppressed reasoning was no doubt something like this: Under the supposition, the time of falling the second half of the first four yards would equal the time of falling the second half of the first two yards, the time of falling the second quarter of the first four yards would equal the time of falling the second quarter of the first two yards; so with the second eights, the second sixteenths, and so forth indefinitely. Hence, there is no fallacy in concluding that, if the total times are not infinite, they must be equal. The truth of this conclusion is an elementary corollary from an unquestioned formula (that the time is the space integral from zero of a constant divided by the space described from the state of rest); but this does not prevent congenital blunderers from flatly denying it. Prof. Cajori, by the way, tells us in a footnote where to find a German version of Galileo's 'Discorsi'; but an elegant and well-known translation into mere English is passed over in silence. Of nobody was it ever truer than of Galileo that the style is the man; and perhaps Prof. Cajori deems the German language and habits of composition fitter stuff for rendering the keen sixteenth-century Italian than

English can be filed down to be.

It is for sundry reasons a good deal easier to write a satisfactory history of physics than a history of mathematics; and probably this will prove the most successful of all Prof. Cajori's histories. The chief difficulty of such an undertaking arises from the separateness of the several branches of physics, and the consequent danger of producing, not a history of physics in general, but a fagot of historiettes of its different branches under one binding. Towards the untying of this knot the present essay affords little clue. However it may be sweetened, a book like this is mainly a record of definite dry facts; and the principal question is, Is it accurate? Without

undertaking to search out little flaws, we have found it to be in that respect all that could be expected.

68 (4 May 1899) 338: **My Inner Life**.

By John Beattie Crozier. Longmans. 1898. 8vo, pp. 562.

CSP, identification: Haskell, *Index to* The Nation. See also: Burks, *Bibliography*; Fisch and Haskell, *Additions to Cohen's Bibliography*; MSS L 159.113, L 159.115; MS 1415 draft).

What paralysis of speech prevented Mr. Crozier from affixing to his book the most attractive of all labels, that of Autobiography--when that is just what it is, neither more nor less--instead of a title both unappetizing and inaccurate, we cannot tell. He withholds nothing of his outward life about which the reader could feel any curiosity, but only his love affairs, his struggles for moral improvement, his temptations. Much of the volume is non-autobiographical, consisting of reflections upon Carlyle, Emerson, Lord Randolph Churchill, Herbert Spencer, Macaulay, Kant, Washington Irving, Hegel, and many other prosaists. These comments are not sensationally novel; and Mr. Crozier's appraisals of literature are more sure than his appraisals of philosophy. When he speaks of metaphysicians, he is apt to be sketchy, not to say superficial. Still, what he says is in the main judicious and ably expressed. His pen is flexible and adapts itself to more than one style, which is always lively, fresh, musical, and as lucid as his thought allows. It is capable of rising to genuine eloquence. His genus is that of philosophical prose poets; but he lacks the earnestness required to rival Emerson, Carlyle, Ruskin, or Henry James the elder, each of whom was in the clutch of a great idea and struck with its superhuman force. He has only his own power of thought, which may be rated as superior, but not as great nor even profound. Both at once and at different periods of his life we find him laying stress upon an assortment of ideas that have no intimate bond of union, and are not all very thoroughly worked out into the light. The Upper Canadian estimations of his youth, his half-course at Toronto, his phrenological beginning, continue to show their tint through all the reading that has overlaid them. Perhaps that reading has been too large and weighty for its foundations.

None of the book is dull; some of it is richly amusing; every part of it is instructive either for its reflections or as a "human document"; while the reader is swept forward as in a novel upon his sympathy with the hero. The book has some faint perfume of 'David Copperfield,' without being, by many leagues, as good or as bad. More than in any writing where thorough acquaintance with the matter can be attained only with great labor, in autobiography the prime need is intense interest on the author's part in his subject; and that interest the autobiographer is pretty sure to be possessed of, or he would not have undertaken an exhibition from which another would shrink. The native delicacy that literature generally demands must in him have been largely obliterated before he could bring himself to make public in their minutiæ, as he will have to do if he aims at greatness in this line, the varying states of his spiritual and intellectual being and bowels. Our author, however, we are glad to find, does not aspire to pose "in the altogether" nor aim at an anatomical demonstration, whatever his ungraceful title might suggest.

Every book is supposed to do the reader some service. There are, of course, celebrated autobiographies that really do little more than entertain us; but the pretence always is to illustrate the conduct of life either by the author's extraordinary successes or, much more usefully, by mistakes which the result makes manifest enough and which the reader is virtually invited to study. Perhaps one of those of this autobiographer has been that he has led too isolated a life. He would seem never to have entirely corrected the faulty appreciations of a semi-education by constant intercourse on many sides with the world's splendid men, so as to study their methods. He has never been drawn into one of those useful and respectable associations which, when they were bearing their best fruit, received (in 1837) the nickname of mutual-admiration societies. Yet he has not burned to lay his comfort and consideration upon the altar of any idea. A writer who is equally indisposed to the one and the other of these courses would certainly seem to have one of the qualifications of an autobiographer.

Our author, in that first happy stage of development when man swims about freely and can look out for a snug hole in which to ensconce himself for life, chose to make himself a local medical practitioner in a growing quarter of London, which answered the purpose of giving him time for that great work on Development, with too typical a title, which he has always been writing. But modern methods of business, so fatal to the small and isolated, invaded his profession, and have hurried him into making friends with the public by the present publication, being already known to them by his 'Civilization and Progress.' A friend he will find in every reader.

68 (18 May 1899) 376: GALILEO'S REASONING

CSP, identification: MSS L 159.116-117. See also: MS 1414 (draft); Fisch, *First Supplement*.

TO THE EDITOR OF THE NATION:

SIR: Galileo endeavored to show by *a priori* reasoning that the velocity of a falling body cannot be proportional to the distance fallen. In my 'History of Physics' I adopt the view that Galileo's argument on this point is illogical. Your reviewer of my history tries to show (p. 317) that Galileo's reasoning is sound, but I cannot accept his position. Galileo says:

"If the velocity with which a body overcomes four yards is double the velocity with which it passed over the first two yards, then the times necessary for these processes must be equal; but four yards can be overcome in the same time as two yards only if there is an instantaneous motion."

To test these assertions, we express Galileo's assumption by the formula:

$$ds/dt = v = as,$$

where a is some *finite* constant (not zero).

If it is possible for s to attain a finite value, then we see from this formula that the velocity is *finite*. Therefore, having a finite velocity, the body cannot pass from two yards to four yards *instantaneously*. Hence, Galileo's conclusion does not follow. The reason why the reviewer finds the time for this passage to be zero is because (contrary to Galileo's hypothesis) *he assumes this time to be zero*.

As a matter of fact, the distance s can never attain a finite value. The correct conclusion to be drawn from Galileo's assumption is that the body *can never begin to move*. Since Galileo concludes that instantaneous motion is the result when really there can be no motion at all, his reasoning is fallacious.

FLORIAN CAJORI.

[Galileo's reasoning (which, by the way, is not, properly speaking, *a priori*) was intended to refute the hypothesis that the velocities of a falling body at different times are proportional to the spaces described from a state of rest. This it did by showing that that hypothesis, conjoined with the indisputable facts that neither the time occupied in falling a finite distance nor the velocity acquired is infinite, constitutes an absurdity--that is to say, leads logically to contradictory results. We gave a conjectural restoration of the complete argument of the youthful Galileo, which in his extreme old age he but imperfectly indicated (*Opere*, 1842-56, vol. xiii., p. 161), and remarked that, so understood, it involved no logical flaw.

It is this assertion that Prof. Cajori disputes. When a disputant says an opponent's argument involves a fault of logic, it is his duty to point out clearly just wherein that fault consists. Prof. Cajori does not do this when he says that the reason we find the time to be zero is because we assume the time to be zero, for we made no such initial assumption, but only proved it must be so according to the hypothesis, if the whole time of fall is not infinite.

In that proof Prof. Cajori, it seems, can find no flaw. But he offers two arguments to show that such flaw there must be. The first of these consists in showing that the hypothesis leads to a conclusion contrary to that which Galileo deduces from it. This, however, would prove Galileo's reasoning wrong only on the assumption that the hypothesis is not one of those from which contradictory conclusions can be correctly deduced; that is, it shows that Galileo's *reasoning* is wrong only in case Galileo's *conclusion* that the hypothesis is absurd is wrong. Thus, Prof. Cajori's first argument is a *petitio principii*.

His other argument is, that Galileo's reasoning must be fallacious because quite a different absurdity can be deduced from the hypothesis. This would be good reasoning only if an absurd hypothesis could lead to but a single absurd consequence. Now, this is never the case.

Every mathematician knows that the solution of the differential equation

$$ds/dt = as$$

is $s = Ce^{at}$. In order that s and t should both be zero together, C must be infinitesimal. Then, for a finite value of s, either a or t must be infinite. That is, either the acquired velocity or the time of fall must be infinite. Galileo's argument first adduces the fact that the time is finite, and on that assumption concludes that the hypothesis would involve an infinite acquired velocity, which is absurd. Prof. Cajori says this argument is illogical, because the true logical procedure is first to adduce the fact that the acquired velocity is finite, and on that assumption to show that the time of fall is infinite, which is absurd. The truth is, that these two arguments entirely agree and support one another, and must stand or fall together; so that Prof. Cajori's second argument only goes to show that Galileo's reasoning is correct, while his first argument in no degree impugns it.

We must not be understood as acknowledging the logical accuracy of Prof. Cajori's remarks in points which, for brevity's sake, we leave unnoticed.--ED. NATION.]

68 (25 May 1899) 403: **The Gambling World**.

By Rouge et Noir. Dodd, Mead & Co. 1898. 8vo, pp. 373.

The History of Gambling in England.

By John Ashton. London: Duckworth & Co., Chicago: Herbert S. Stone & Co. 1899. 8vo, pp. 286.

CSP, identification: Haskell, *Index to* The Nation. See also: Burks, *Bibliography*; Fisch and Haskell, *Additions to Cohen's Bibliography*; MSS L 159.113, L 159.115; MS 1416 (draft).

Good for their kind are the several chapters of 'The Gambling World,' and that on the London Stock Exchange may even bring some persons to their senses. But, at the best, books that, without being works of art, have no higher aim than to amuse, almost regardless of the exact truth, are dreary things, especially when they address themselves to a class of readers who are not fond of reading. "Rouge et Noir" may imagine that he aims to instruct; and indeed he declares that gamblers are morally certain to lose in the long run. But when an author stuffs a volume with stories that verge on the incredible, without vouchsafing any authority whatever--not even his own name--the reader is driven to judge of his seriousness by such of his statements as he is himself in a situation to test. Now, exclusive of facts that might be culled from any good encyclopædia and of facts that have been thoroughly ventilated in the newspapers, we have found other assertions here so frequently erroneous as to indicate a very jaunty attitude towards accuracy. The number 1,592,814,947,068,800 is given as "the number of combinations possible by the distribution of fifty-two cards." That it is not the number of arrangements of a full pack any person acquainted with probabilities will instantly see, from the short row of figures and because that number ends with twelve zeros. In order to find out whether it is the true answer to any problem, we have separated it into its factors, and can testify that it is the scrupulously exact number of distributions of a piquet pack among the two hands and the two parts of the *talon*; but it has nothing to do with fifty-two cards.

The innate simplicity of the gamester appears in the statement that Government lotteries and great gambling casinos are honestly conducted. Will any man of sound judgment who knows how affairs connected with Government go on in Spain and Italy, hold their lotteries to be materially more trustworthy than if Croker or Quay or Platt managed them? Or will any expert in legerdemain say that it is impossible by a combination of interests to secure the drawings of predetermined numbers? As for keepers of roulettes, they are not intelligent enough to be honest; for they have themselves risen from the ranks of gamesters, and no gamesters are sound reasoners. A private gentleman who ordered a roulette from a house in New York whose business it is to make such things, found that, without special directions to the contrary, it would be furnished, as a matter of course, with a contrivance for correcting the luck. A man who was, and had for many years been, employed in a well-known gambling-house near Madison Square, confessed that in all his experience he had never known a player to carry away $200 of winnings. The limit which is everywhere put upon the martingale shows the stupidity of the management. A comparatively low limit upon initial bets may be set down to extreme conservative caution, if this seems compatible with the gambler's nature. But a limit upon the continuation of martingales is simply ridiculous, since, the bank is perfectly secured against any heavy loss, and the higher the martingale is pressed the greater the proportionate winning of the bank. At an ordinary roulette-table with a double zero, against a player whose fortune is 1,023 times his initial bet, and who doubles his bet whenever he loses, the bank wins 411 francs for every 613 francs risked: while if the player's fortune is 1,048,575 times his initial bet, the bank will in the long run make a profit of 672,676 francs on every 375,900 francs risked. The banker ought, therefore, to encourage martingales as much as possible; for at ordinary betting the bank's profits amount only to one franc on every eighteen risked. Yet even if the player bets the same amount every time on a simple chance, which is his most advantageous course if he will play, it is unlikely that he will ever be able to net a gain of seven times that amount, though he have the fortune of Rothschild at his back. Even if there is but a single zero, he is not likely ever to net a gain of thirteen times his bet; so that should he lose thirteen times his bet, he had better give up all hope of regaining it.

The most advantageous course of all is not to play at all, and the next to that is to make but a single bet. Thus, for every hundred players who should each make a single bet of 100 francs on a color at single zero roulette, 48.65 would gain 4,865 francs in all, while 51.35 would lose 5,135 francs. If, however, each player were to make one-franc bets until he had either won 12 francs or lost 100 francs, 52.15 players would win their 12 francs, making 626 francs, while 47.85 would lose their hundred francs, putting 4,785 francs to the other side of the account. We need hardly say that "Rouge et Noir" falls into most of the usual pitfalls which the doctrine of chances has prepared for those who have but a slight acquaintance with it.

The book is very prettily got up, and does not weigh a kilo, which ought to be the limit for a book to be read continuously.

Mr. Ashton's 'History of Gambling in England' is a beautifully printed volume and a work of sufficient research. Of its host of anecdotes, almost all are supported by contemporary testimony and the majority by good testimony. This is the only branch of history that is in an entirely satisfactory condition or about which we know all that a reasonable curiosity could, for the present, desire to know. But there is no other so blank for those who seek in history some consolatory or elevating aspects of human nature. Of the two classes that walk its stage, the sharpers are engaged in a business a good deal like other sorts of business in which great fortunes are amassed, and some readers may accord them some measure of esteem for not founding hospitals and universities or otherwise advertising their compassionate hearts. But there can be no doubt they would do so if it could in any way further their interests. They are really as unestimable and uninteresting a class of bankers as can be found. On the other hand, the pigeon is a simpleton so intent upon gaining some inequitable advantage that his small stock of good sense completely deserts him--a creature who seems put into the world by a beneficent Providence in order to serve as prey for business men, without offering any handle for benevolent regard.

The book will divert us all with its pictures of the manners and morals of worlds not too remote from our own, and by contrast brings into view the greater self-control to which modern conditions of middle-class life are training men. One regrets that the history should break off at the year 1845; but Mr. Ashton has rightly judged that it would be "inexpedient to give any modern instances." It would, indeed, be exceedingly inconvenient to publish an unvarnished picture of life in a high gambling circle.

68 (25 May 1899) 405: **Outlines of Industrial Chemistry**.

By Frank Hall Thorp. Macmillan Co. 1898. 8vo, pp. 541.

CSP, identification: Haskell, *Index to* The Nation. See also: Burks, *Bibliography*; Fisch and Haskell. *Additions to Cohen's Bibliography*.

Dr. Thorp is instructor in the Massachusetts Institute of Technology--a fact which we mention to avoid confusion with other Thorps and Thorpes in the chemical world. A treatise on industrial chemistry in a single volume, and that by no means a huge one, for it would contain only about 250,000 words if its pages were filled with solid text, is clearly not a book for general readers, and yet cannot contain as much as every chemist, whatever his specialty, ought to know about this branch of the science. It may, however, be as much as it is judicious to require as a condition of graduation. It is not a handbook, but a textbook, of a subject the whole soul of which resides in its details. Evidently, great judgment is needed to draw up such a survey usefully.

The volume is equally divided between inorganic and organic industries, with about two dozen chapters under each head. What shall we demand in such a volume? That serious flaws could not be picked would be too much to expect. It is rare to find a manual of a single industry of which that can be said. That it should explain the general principles upon which the economy of each industry depends, if not showing where the shoe most pinches, would be highly desirable. But these general principles are often the last thing that long experience in a particular branch discloses. If we will content ourselves with a general picture of the present state of the externals of industrial chemistry--with what is done, not with why it is done--especially in this country, so much Mr. Thorp has undoubtedly been very successful in presenting, though we will not say that he is not a year or two behind the times here and there. If such a vast subject as that of pigments has to be compressed into 25 pages, fermentation into 33, bleaching into 11, artificial dyestuffs (the mere list of which would fill the volume) into 9, the chlorine industry into 18, soda into 24, sulphuric acid into 14, we must not expect much more.

Let us look out in the index some subject that is neither large nor very small, say *charcoal*. We find two lines about its use as a pigment on p. 214, and the only other reference is to p. 26, where less than a page is given to the subject. There is nothing about the use of different kinds of wood, nothing about the economical effects of carrying the process further or not so far, and the space is mostly devoted to the barbarous charcoal pits, which certainly no intelligent chemist would employ. There is a reference to p. 257, where we find about seven pages devoted to the destructive distillation of wood. But this and charcoal-burning are, or ought to be, all one industry. At the end we find references to half-a-dozen manuals (which are not bad, if the student does not content himself with what they contain) and to a few memoirs, not the leading or classical ones. Industries which are more scientifically carried on in this country are certainly better treated. The book will answer its purpose very well.

68 (22 June 1899) 482-483: Stars and Telescopes:

A Hand-Book of Popular Astronomy, Founded on the Ninth Edition of Lynn's Celestial Motions.

By David P. Todd. Boston: Little, Brown & Co. 1899. 8vo, pp. 419.

CSP, identification: Haskell, *Index to* The Nation. See also: Burks, *Bibliography*; Fisch and Haskell, *Additions to Cohen's Bibliography*.

David Peck Todd (1855-1939) was a noted American astronomer. He was graduated A.B. from Amherst in 1875, and A.M. there in 1878. Upon graduation, Todd was offered a job by Simon Newcomb at the United States Naval Observatory. He is credited with having anticipated by 50 years Tombaugh's discovery of Pluto. Todd held teaching posts at Smith College and Amherst. He was the husband of Mabel Loomis Todd.

Mr. Lynn's original text, to judge of it as well as we can from this volume, seems to be a very popular treatise on astronomy, not at all confined to the subject of "celestial motions," but characterized by a severe avoidance of two things, first, of all that relates to research as research, which constitutes the life of science, as well as of everything in the least degree mathematical, as all celestial motions are; and, second, not only of all that approaches the sensational, but also of all that relates to philosophy or to matters of general human interest. We can understand how such a book should have gone through nine editions in England, for it meets the popular English delusion that what is "plain and substantial," or, in other words, is stolid and positive, is all that is essentially valuable. But the qualities which recommend a book to English readers may not find equal success here. It may be doubted whether Americans will see any particular appropriateness, for instance, in printing very prominently before a book like this a sentence from St. Augustine's chapter on the "Spiritual Creation of the Virtues," set up in English black-letter like a text on a wall of a church. Its inconsistency with the purpose of the volume makes it all the more charmingly English; but that charm may escape the people of the great West. This Latin paraphrase of the opening of a great psalm is put second to a specimen of Wilhelm Herschel's English diction and German nebulosity. In place of Mr. Lynn's "Et pulchra sunt omnia, faciente te, et ecce tu inenerrabiliter pulchrior, qui fecisti omnia," St. Augustine would have supplied many another quotation not wandering clear away from astronomy, and more consonant with the main virtue (such as it is) of Mr. Lynn's book, *e. g.*, "Interrogavi cœlum, solem, lunam, et stellas; Neque nos sumus Deus, quem quæris, inquiunt" (Confessions, x. 6).

The volume affords no precise indications of what is Mr. Lynn's and what Prof. Todd's; but the work of the latter seems to have consisted in adapting the text here and there to American readers, to adding half-a-dozen important chapters, as well as long notes throughout giving additional facts, to select references to the literature, especially of the popular kind, at the end of every chapter, to a list of asteroids, a full index, and a great many illustrations of celestial objects, instruments, observatories, and portraits of astronomers--all of which is of a rarely painstaking quality. In this way the book has been converted into a veritable hand-book--not a mere digest of facts, but a copious selection of those that are most important. It is hardly possible for a hand-book to be what we generally mean by "popular," nor does this work, perhaps, profess exactly to be a *popular hand-book*. It is a "hand-book of popular astronomy"--that is, a compendium of facts interesting to the astronomical amateur--that extremely variable species which ranges all the way from the idlers and semi-idlers through the Ulugh Begs, the Dembowskis, the Rutherfurds, through the Lowells, the Pickerings, the Lockyers, to the glorious company of Tycho Brahe and William Herschel.

To many persons, the most interesting part of the book, which is well got up, will be the illustrations, of which there are nearly three hundred. The author seems to have made it a rule to give no portrait of a living man--a practice which apparently tacitly reflects upon the susceptibilities of astronomers to a degree that we hope is unwarranted. Some of the likenesses are quite admirable, as those of Alvan and George Clark. A few seem needlessly blurred, as are some of the starclusters. Some of the figures of instruments, etc., are confused and dark. Only the professional astronomer can at all appreciate or understand a great part of the illustrations, which illustrate nothing in the text.

The work will deservedly enjoy a high degree of secondary authority among amateurs, owing to the good judgment and care which have presided over its compilation. Yet here and there we come across statements and reasonings which are not quite clear. For instance, on p. 219 we read, "Meteors, then, belonged originally to comets," and this conclusion is extended to "all meteoric bodies." But the only reason which is offered for this belief is that four comets have been known to break up under the influence of tidal forces into small fragments, forming meteors--an inference hardly parodied by the following: The 'Visible World' of Comenius, 'Nature Displayed,' by Dufief, Ollendorff, and Prendergast's 'Mastery' books, are all known to have produced many fluent speakers of different languages; hence, we may infer that all speaking of languages had its origin in phrase-books and the like. Such reasoning violates a logical rule of all induction, namely, that the sample by which the whole class is to be judged ought to be drawn at random from the whole of that class. This sample of meteors is, on the contrary, drawn exclusively from a part of the whole class which, owing to the mode of its limitation, cannot but possess that character which is inferentially, but unreasonably, extended to all other individuals of the class. What is a meteor? It is nothing but a cometary body which enters the earth's atmosphere. Why should all such bodies, without exception, be supposed to be broken from larger cometary bodies? In our present ignorance of the origin of things, it is not unlikely that of all the masses which wander through space a large proportion are very small. In such small bodies tidal forces would be very feeble, while their cohesion would be relatively powerful. It may be that Prof. Todd is in possession of some good reasons for thinking that no such bodies ever impinge upon our atmosphere; but if he is, they must be different from the premise he adduces.

68 (29 June 1899) 499-500: MARSHALL'S INSTINCT AND REASON

Instinct and Reason.

By Henry Rutgers Marshall. The Macmillan Co. 1898.

CSP, identification: Haskell, *Index to* The Nation. See also: Burks, *Bibliography*; *List of Articles*; MS L 159.119; MS 1513 (draft).

Under this title Mr. Marshall essays to furnish us with a psychology of religion. Its last words, printed in capitals, are, "Be religious." Why? Because analysis shows it is in the highest degree desirable. Very well; most of us already feel that keenly enough. The difficulty is that the first step towards being religious is to believe; and not only do the minds of most men nowadays not readily believe, but, bad as it is to be deprived of religion, it is unquestionably still worse to cook up a factitious, unfounded, and consequently unstable belief. True, Mr. Marshall persuades us that religion is an instinct, and, therefore, not a faith. But, like other instincts, the moment it is broken down in the particular form in which it has taken root in us, it is apt to evaporate altogether, or, at any rate, to retain little vitality. Thus, the book, in its practical upshot, leaves us just where the majority

of thinking men are already. Those who deem religion to be altogether baneful, had better avoid it if they do not want to change their minds. There are not many sentences in the volume as pro-religious as the following, which serves at once to illustrate Mr. Marshall's attitude and his way of constructing a period:

"We note also the existence of a continuous and partially effective opposition to rationalism by the established Christian Church; an opposition which seemed exceedingly perverse to the Church's opponents at the time, which appears extremely reprehensible to the body of thinkers to-day, but which, I am convinced, will not seem nearly so ill-timed nor so disgraceful to those who look back from the standpoint which will be attained in the future, however much they may deplore, as we all must do, the form which this opposition took and the methods employed to attain its ends."

It is certainly true that rationalism has been slowly declining. Since nineteenth-century science has taught us how truth becomes established, since Darwinian ideas have been applied to the history of mind, since Hegel's analysis of self-consciousness has been illustrated in studies of childhood, and since the phenomena of sub-conscious mind have become known, the pretensions of individual reason to solve the great problems have been at a discount. Yet, after all, it is our nature to reason, and we have to make the best of it. Progress never has come from the religious spirit, and never will, as Mr. Marshall acknowledges; so that the whole question is how far we are to carry rationalism, and at what point to check it. Mr. Marshall's doctrinaire anti-rationalism is rather amusing, considering that his whole book is rationalism undiluted with one single word dictated by authority or prejudice, or even instinct, as such. All that he contributes to the solution of that question is an argument to show that it is rational to pay great respect to our instincts, especially those of the broader kinds. We fancy one J. J. Rousseau said something like this, without being looked upon as the champion of unreason.

The author's method of philosophizing is heartily in the spirit of the times. He relies implicitly and utterly upon a line of evolutionary argument which may be sound and is decidedly fashionable. He assumes as unquestionable that every instinct must subserve, or have formerly subserved, some important need, and even sometimes seems to speak as if every tendency to reaction must benefit, or have benefited, the single cell as well as the whole organism. While he risks everything on such reasonings, he makes considerable general admissions as to the insufficiency of the evolutionary philosophy, going in some respects further than there was any need of doing. Thus, he grants to Mr. B. I. Gilman that "Darwinism" leaves the whole question of origins untouched. Darwinism, in the proper sense of the word, of course does so; but that a purely evolutionary philosophy can trace its way from a starting-point that logically demands no explanation back of it, is shown by the instances of Aristotelianism and Hegelianism, both of which philosophies are evolutionary and set out (or may set out) from complete actual indeterminacy. Equally needless and still more important is the confession that the idea of "progress" is purely relative to our own ideals.

If there be in nature a universal tendency in any definite direction, no matter what, that universal tendency will determine ideals; and therefore any such tendency would be a progress purely objective.

For reasons that appear decidedly feeble, Mr. Marshall concludes that there is something psychical--in his phrase a "psychic somewhat"--to which he gives the name of mentality, in all action whatsoever. The reasons for believing that the rudiments of mind are much more widely distributed than we can positively make them out are strong enough; but to select "action" (whatever that may mean) from among all the objective elements of the universe of phenomena as defining the limits of mind, is a very different inference. It may be doubted whether a concept so vague and confused as a "psychic somewhat" can be of any service at all in philosophy. It would seem to be the business of the metaphysician to discriminate between the radically unlike elements of psychoses, and to find good reasons, as soon as he is able, for identifying these with different elements of phenomena objectively observed. One of the psychic somewhats would appear to be immediate consciousness, or feeling; but there is little or no reason to opine that this is at all proportionate to the amount of motion of its subject, or to anything else of that sort which the word "action" may denote. Consciousness is defined by Mr. Marshall as "the field of inattention." We may grant that this remark is founded on a true psychological observation, without admitting that attention and inattention differ otherwise than in degree, or that there is any threshold, or *Schwelle*, between them. Considered as a definition of consciousness, the proposition simply restricts that word to a particular kind of self-consciousness. Consequently, when the author applies the definition to the criticism of James and others, he simply misses their meaning.

Mr. Marshall reposes too confidently upon what he calls "mental and physical parallelism"--a monistic theory familiar enough in its outlines, though somewhat modified by the author--as against the traditional doctrine of a soul. To hold to a substantial soul is neither more nor less than to hold that mind and body really react. Action cannot take place without reaction, even if it can be so conceived, which may be doubted; for what would "action" mean without reaction? Yet the only considerable objections that Mr. Marshall brings against the theory of the soul are, first, that the action cannot be exclusively of mind on matter, and secondly, that it cannot be exclusively of matter on mind. The whole question is one of fact, and must await positive scientific observations for its settlement. Comparing it with other mysteries which have eventually been cleared up in that way--with that of the chemical constitution of the stars, that of the Trojan War, that of whether diseases are entities, that of meteorites, etc.--we remark that in those cases the facts were found to have little regard for the fine negative theories; and the simple idea of a soul seems to be rather more like nature's habitual answers to experimental questions than is any highly metaphysical hypothesis of mind-stuff and the like. Perhaps we may ultimately find that mind and matter are of the same general nature, and yet that there is a real mutual reaction between body and soul.

We could not undertake to do justice here to Mr. Marshall's very elaborate main argument. It seems to be essential to his position to maintain a natural classification of instincts into those which conduce to the preservation of the individual, those which go to the propagation of offspring, those which support social life, and finally religion, which checks the tendency to reason--or to eccentricity, which is much the same thing, from Mr. Marshall's standpoint. Reason he holds to be conterminous with choice, and as broad as our psychic life, and not at all restricted to the more superficial consciousness. Here, of course, he must expect strong dissent. The work concludes with two strong chapters, antagonizing hedonistic and utilitarian ethics.

69 (6 July 1899) 18: **An Introduction to the Theory of Analytic Functions**.

By J. Harkness and F. Morley. Macmillan. 1898. 8vo, pp. 336.

Attributed to Peirce by Fisch in *First Supplement* (internal evidence). This item is unassigned in Haskell's *Index to* The Nation, vol. 1.

As a book to put into the hands of those students whose turn of mind enables them profitably to relish a spoonful or two of the odorous bouillabaisse that has been stewing on the mathematical range during all the generation last past, but who do not intend to become professional mathematicians, no other has yet appeared, or is likely for a good while to appear, we believe, half as good as this; unquestionably not, if we limit the comparison to works on the theory of functions, which has served as *pièce de résistance* during that period and longer. This is distinguished from other available elementary treatises by being in the main Weierstrassian--which means (as well as we know how to describe it in general terms) that it flies straight at the algebraic throats of fundamental problems, disdaining geometrical circumventions, and with a degree of logical precision which (whether it is of the essence of the method or only a natural concomitant of it) is certainly much superior to the previous habit of modern, or even of ancient, mathematics. This method offers special advantages over those of Cauchy and Riemann when the aim of the study is mental training, as it is with those students for whom this book is most adapted.

Such a book must aid in that disintegration of the traditional English idea of mathematics which has been going on of late years. For some reason the English have followed Euclid, Apollonius, and Archimedes more closely than have the Continental mathematicians. They have shared the Greek scrupulosity of logic, and, like the Greeks, seem to look upon all mathematicians with the eyes of geometers. They, more than others, for example, have been disposed to look upon a quarter-turn as an *interpretation* of algebraic imaginaries. It better accords with the Weierstrassian spirit, as well as that of the Lagrangian analysts, to regard the algebraic expression as an elucidation of the Euclidean geometry of the plane, as quaternions is of 4-dimensional geometry.

But Professors Harkness and Morley are by no means entirely given over to Weierstrass. The methods by which the theory of functions was originally rendered comprehensible are sufficiently illustrated in the book to make the student appreciate the large measures of beauty, strength, and truth that are in them. The authors are rather on their pilgrimage to Weierstrass, and perhaps already regret that they did not treat some problems in their recent larger treatise more after his example. They are not yet altogether incapable of lapsing into obsolescent modes of thought. Thus, they adhere to that opinion which calls the point at infinity in the plane of imaginary quantity an "artificial point." Wherever this phrase originated, it involves a logical slip with both feet. For first, it confuses a pure mathematical hypothesis with a matter of fact, and, second, it assumes that we are better acquainted with the infinitely distant parts of space than we are. For no matter what the shape of real space *is*, which is a question of physics, not of pure mathematics, it is undeniable that we may *suppose* a space which shall have but a single point at infinity. In such a space, a circular filament, or fibre, could be stretched into an unlimited and infinite straight line (as by a continuous bilinear transformation), although in the space of projective geometry, where there is a plane (or conic) at infinity, the filament could be continuously deformed only into two straight fibres (and that only by welding two particles of it together), which, however, may coincide. We can never ascertain how the infinitely distant parts of our objectively valid space are really shaped, except by inference from the parts near us; and since the hypothetical plane of the theory of functions and that of Euclidean projective geometry are precisely alike except at infinity, it would seem to follow that we never can decide that the former is not the shape of a real plane, unless the proper motions of the stars should prove that space is non-Euclidean, in which case the plane of the theory of functions would be everywhere unlike a real plane. It may be objected, however, that we cannot, from any observation of space about us, infer that the part at infinity has an essential singularity; and that which could never be inferred cannot be true. Whether or not there is a sound answer to this, it is hard to say. But this is not at all the idea embodied in the term "artificial point."

The book has most of the distinguished merits of its authors' larger work. It also shares the chief fault of that volume, that of being here and there not so perspicuous as it might easily have been made; a greater inconvenience in the more elementary treatise. We do not quite comprehend why the book need have been so very small, only 326 pages of large and open print, exclusive of the glossarial index. True, it had to be short enough for a college course; but a little amplification in some places might have abbreviated the time required to read it. An initial short chapter discusses the number-system. If that was worth doing at all (as we certainly think it was), it was worth carrying to logical perfection; for that is the sole *raison d'être* of such a chapter. But the reasoning is so abridged that it can hardly be said that this has been done. The number system is one thing, and the system of discrete multitude is an entirely different thing. The former is an affair of pure mathematics, the latter is rather a question of logic, but of the logic of mathematics; and the work would have gained in value, especially for the class of students for whom it is adapted, if the chapter on the number system (expanded by a few pages) had been followed by another developing the distinctions of enumerable, denumeral, and the grades of higher discrete multitude, together with the true conception of continuity.

69 (27 July 1899) 77-78: Old Clocks and Watches and their Makers. With 400 illustrations.

By F. J. Britten. London: B. T. Batsford; New York: Charles Scribner's Sons. 1899.

CSP, identification: Haskell, *Index to* The Nation. See also: Burks, *Bibliography*; *List of Articles*; MS L 159.118.

Horology is here secondary to the ornamentation of time-pieces, including mechanical marionettes and the like. The illustrations are interesting and may be useful. The general history of the art is a good compilation of no extraordinary accuracy. For example, we read, "The earth performs its revolution round the sun in 365 days, 5 hours, 48 minutes, 49.7 seconds. No account was taken of the odd hours till the year B. C. 45." The time given is the tropical year, not the period of revolution of the earth. The second sentence should read: *Among the Romans* no *regular* account was taken of the difference between the year and 365 days, etc. It would be easy to give other examples.

One of the main features of the book is a list of some three thousand former clock and watch-makers, with brief notices of them. Here also faults abound. A brief cursory turning of the leaves has brought to notice the following: The Neguses of New York and the Bonds of Boston are overlooked. Jürgensen is uniformly printed without the umlaut; and the first watchmaker of the family, Jœrgen, is omitted, as well as Jules Frederik and Jœrgen Urban Frederik. Louis Urban is said to have been born in 1790, when (according to a right statement just above) his father was but fourteen years old. The true date is 1806. Heinrik Johannes Kessels, as eminent a clockmaker as Holland ever produced, is omitted. The celebrated Jobst Bürgi is entered thus: Burgi, J. (de Burgi or Burguis), Prague. The "Burguis" is evidently a corruption of the Latinized Byrgius. His much longer residence in Cassel is ignored. One of the familiar names of French horology is, in its place of entry, thrice printed Bréguet, though elsewhere correctly. The date of birth of the first Breguet is wrong, and the information about the last head of the private firm is very insufficient. That his nephew passes unnoticed is not a matter for complaint. Ferdinand Berthoud is said to have been born in 1745, instead of 1727, and Louis Berthoud is omitted. Jodin is called Jean, instead of Pierre, and a posthumous Paris reprint of his chief book is referred to where the Geneva original should have been cited. Both the dates attached to the name of Oronce Fine (printed Fine, by a common error) are wrong. Bourgeois, the maker, if not the inventor of Vaucanson's duck, is not on the list, where we need hardly say that such names as Prasse and Steiner would be sought in vain. Of J. A. Lepante there is a nearly correct account, but no mention

of his distinguished wife, brother, and two nephews. The authorship of the 'Mathematicæ Clavis' is attributed to Benjamin Oughtred, instead of to William, who was not a watchmaker. Sir G. B. Airy is said to have been appointed Astronomer Royal in 1855, when he had really occupied that post for nineteen years.

It is a pity the list should so swarm with faults; but they are almost insignificant compared with its positive merits. For London names, the records of the Clockmakers' Company have enabled Mr. Britten to make a close approach to completeness, although we are bound to note that we have met with one omission and one error in the account of an eminent family of London watchmakers. He seems to have diligently searched the London and Edinburgh Gazettes, and to have examined an incredible number of old time-pieces with unwearied assiduity. If the exercise of greater care to avoid erors [[sic]] would have had the effect of preventing the publication or even of greatly curtailing the list, we ought to be thankful for the carelessness, for facts substantially new are certainly more valuable than minute corrections of facts already collected, while one little correction will oftentimes involve an amount of labor that might, in a fresh field, have been utilized for the collection of ten facts quite unknown. How much easier it is to learn that there is a sun in the heavens than to ascertain its precise diameter in seconds, and how much more important that easily acquired knowledge!

69 (3 August 1899) 97-98: LEIBNIZ REWRITTEN

La Nouvelle Monadologie.

Par Ch. Renouvier et L. Prat. Paris: Armand Colin & Cie. 1899. 8vo, pp. 546.

CSP, identification: Haskell, *Index to* The Nation. See also: Burks, *Bibliography*; *List of Articles*; MS L 159.110; MS 1417 (draft).

The nature of things would seem to have a screw loose if the powers of a Renouvier are not to be of service to the race. When we see a man far advanced in the eighties producing, albeit with a collaborator, a great volume of metaphysics, terse, clear, and well-oriented, it is certainly not on his shoulders that we can lay the blame if his industry should teach the world nothing. The principles of Renouvier's philosophy were published in the earlier years of the Second Empire, with the disadvantage of a too modest title. It was mainly other causes, however, that at first prevented the work from being as much studied as it deserved to be. Of late years it has in France been, perhaps, more studied than it deserved to be; but it has not been as *well* studied as it deserved to be. The author belonged to that group of schools in which the ideas of Kant were still paramount. Those schools never fully ripened their fruit, because the attention of the strongest men was turned away to the rich conceptions that the mechanical theory of heat and the Darwinian hypothesis about that time suggested. None of the Kantians had more thoroughly learned their master's great lesson than this Frenchman--the lesson that metaphysics can be solidly founded only upon the science of logic. Unfortunately, Kant, though a logical Samson, had yet treated

that science with what we can but call, in view of the importance he attributed to it, inexcusable levity; and his followers had always accepted his logical dicta most uncritically, just as they have ever since continued to do. The aliment contained in De Morgan's and Boole's studies had not been assimilated by anybody; nor had mathematical reasoning taken on its modern exactitude. What was meant, in that *Quarterly Review* period, by good logic had for its principal ingredient a forcible and imposing style of writing. Renouvier was, and is, not only an able logician in that sense, after the best French models, but even according to the more scientific standard of mediæval Paris. But now, fifty years after his acme, if we demand that he shall satisfy the requirements of the exact logic which has since grown up, it is not surprising that we find he falls so far short of it that his conclusions as a whole can no longer be accepted. At many points his well-elaborated thought would be extremely valuable if some modern logician of the first strength would take the trouble to disentangle it from other elements with which it is interlaced. It is lamentable that such a labor is not likely to get performed, since M. Prat has not proved adequate to the task, and yet one does not find where to lay the blame for its non-performance unless it be upon the logic of events and the nature of things.

The present work undertakes the noble task of rewriting the so-called 'Monadologie' of Leibniz, and of more fully developing its philosophy after indispensable corrections. The doctrine is very nearly the same as that of his first philosophical treatise from which he at one time seemed to be wandering. The 'Monadologie' is rewritten in a Kantian spirit; and as time increases the distance from which we survey the Kantian philosophy, its affinities with that of Leibniz appear closer than they formerly did.

We cannot give much idea of what has been packed into these five hundred-odd pages, further than to say that they discuss most of the usual problems of metaphysics and much besides. The main doctrine is that of Leibniz, that the universe is composed of units, indivisible and endowed with consciousness. The doctrine of pre-established harmony is retained--that the monads do not act on one another in any other sense than that while each one follows out its own destiny in the succession of its modifications of consciousness, these have been arranged so as to harmonize and to amount practically to actions upon one another. But here the authors bid farewell to Leibniz. The law of sufficient reason is hardly mentioned in the book, but is practically rejected in every aspect of it. Of course, with this law the bottom of optimism falls out. In place of Leibniz's principle of the identity of indiscernibles, that things other than one another must differ in some qualities, Scotus's doctrine of hecceity (substantially that of Kant) is adopted-that individual existence is no general character, but is an irrational act. An important departure from Leibniz is the rejection of all actual infinite multitude (and hence, of all continuity) as self-contradictory. Kantian nominalism is carried to an extreme, every conception of intellectual value (space, time, etc.) being regarded as untrue of substances. Kant himself allows us to surmise that there is some unintelligible root from which each special appearance springs, although all that makes them intelligible is our own embroidery. But in this monadism the nakedness of the thing in itself is laid bare, and it plainly appears that nothing exists but monads and their harmonizing dreams. A single monad, we are told, transcends the limits of possible experience, although some finite collection of them is cognizable.

The position of Renouvier concerning determinism has excited enough curiosity to make it worth defining. Five opinions on this subject are current to-day. The common one, which may be attributed to Boyle, is that nature is a machine working according to exact laws (like the differential equations of dynamics), while the conditions to which those laws apply (like the constants of integration) are entirely arbitrary. Or, this may be expressed by saying that Nature syllogizes in her action; the ultimate major premises being laws, and the ultimate minor premises irrational facts. There are two opinions more deterministic than this. The first is that even the initial conditions of the universe are perfectly regular. This opinion still leaves room for accidents, such, for example, as that a number of bodies should at one instant come into symmetrical positions. The extreme of determinism, held by Leibniz, supposes that every aspect of every fact is subject to reason, so that there is a special Providence in the fall of a sparrow. There remain two opinions less deterministic than the common one. One of these, which has been called Tychism, is that there are minute departures in nature from any general formula which can be assigned, so that there is a certain element of absolute chance. This is the position maintained by C. S. Peirce a few years ago in the *Monist*. It had already been held by Boutroux, quite incidentally, however, and not as a prominent feature of his argument in favor of the contingency of natural laws. But this is not the opinion of

Renouvier. He holds that all causality is exact and produces its component effect, but that, in addition, there are component influences which spring direct from the arbitrary action of the monad. Perhaps Boutroux had in mind something like this when he spoke of "une sorte de jeu laissé aux cadres logiques." These actions, though arbitrary in the sense of not being rational functions of preceding events, are all provided for in the pre-established harmony, and thus duly produce their component effects (or quasi-effects) upon other monads.

Three critical questions are apposite to the new monadism. The first is, Supposing we were to grant all its propositions, how far would it constitute a satisfactory philosophy? What is it that philosophy ultimately hopes to accomplish? It is, if we mistake not, to find that there is some intelligible truth, some absolutely valid reasonableness, to ascertain how far this reasonableness governs the universe, and to learn how we may best do its service. It may be this hope is not destined to be realized, although, being reasonable, it acts to strengthen itself. It may be that reasonableness essentially requires an element of unreason, a brute force, on which and with which to accomplish itself; but in that case we hope that this unreason may turn out capable of becoming infused with reason. There must be nothing hopelessly and finally unreasonable, or in so far philosophy is to no purpose and its hope is vain. But the new monadism presents many such irrational features. What possible reason can there be for the existence of the precise finite number of monads that there are, rather than

for one more? Since the monads do not metaphysically act upon one another, what rational purpose is subserved by the real existence of so many? The mere dream of them by one would do as well. Why should each monad have the three peculiar characters of intelligence, passion, and will, or why should any phenomena be as they are? In short, the absolutely inexplicable pervades the whole system, while one supremely antirational nominalism is supreme over the whole. Continuity is nothing but that modification of generality which is proper to the logic of relatives; and generality is of the essence of rationality. Yet this new monadism makes all continuity a false illusion and all generality equally so. Persuade him that this is true, and what is there for a philosopher but to hug a delusion to his heart as being, by virtue of its reasonableness, infinitely more real than the wretched abortion that the world of reality would so turn out to be? Rather hope that some corrected Hegelianism is the truth, or, better still, that, as the elder James taught, the Reasonable One sets off over against himself an irrational phantom upon which his warmth and light may be brought to pass.

The second question is, how far the reasoning of this work is sound. The opening section sets forth that conception of a simple substance which is the very cornerstone of monadism, without which the whole erection would crumble. Nobody is unaware that most thinkers now reject any such idea. The subject of an attribute, they say, is nothing but a group of phenomena differing from a metaphysical substance in not being permanent, like that old jack-knife. Even Kant declared the conception of substance has no validity beyond possible experience. It was incumbent on our authors, then, to begin by proving that there is any substance other than the universe as a whole. Instead of this, they so naïvely take the matter for granted as to give a definition of substance which would make it a mere way of thinking. They parade a pretended demonstration that a contradiction is involved in supposing a substance to be infinitely divisible, or, what is precisely the same thing (though they do not so treat it), in supposing an infinite multitude of substances. We will not stop to point out the glaring fallacy of that "demonstration." Modern logic enables us to show that it is absurd to say there is a contradiction in supposing an infinite multitude of substances. There is certainly an infinite multitude of finite whole numbers. True, these are only possibilities, not substances. But according to the principle of hecceity, admitted by the authors, mere substantial existence is no general character and cannot create a contradiction. In other words, what is possibly possible is possibly actual.

How far can this work be regarded as the natural perfecting of the philosophy of Leibniz? Leibniz had more sides than one. If we consider him as above all else an extreme nominalist, and expunge from his celebrated paper all that tends in the opposite direction, the development of what would remain might not be very different from the *nouvelle monadologie* minus its free-will doctrine. But if we deem a man to be best represented by that one of his ideas which shows most prepotency, it is in the direction of the differential calculus that we must look for the genuine Leibniz, and in philosophy we must regard the law of continuity as most Leibnizian. This principle would at once do away with the isolated monads,

and render the extravagant and unverifiable hypothesis of preestablished harmony superfluous by directly solving the riddle of the transitivity of causation, while it would form the basis of a philosophy in deepest unison with the ideas of the last half of the nineteenth century.

69 (10 August 1899) 118: **Through Nature to God**.

By John Fiske. Boston: Houghton, Mifflin & Co. 1899

CSP, identification: MSS L 159.120, L 159.126. See also: Fisch, *First Supplement*.

To watchers of the tides and currents of thought, just now setting decidedly against rationalism, the later turn of Mr. Fiske's philosophy is an interesting phenomenon, and none the less so where his argument seems insufficient. The present little volume, continuing the line of thought of the 'Idea of God,' has three disconnected parts, entitled, "The Mystery of Evil," "The Cosmic Roots of Love and Self-Sacrifice," and "The Everlasting Reality of Religion." Mr. Fiske's solution of the problem of evil is the familiar one, that evil is only relative, and that it is absurd to suppose good to exist without a correlative and reacting evil. Hardly more than a hint is afforded of how this thought is to be followed out, although it was developed at large more than a generation ago in James's 'Substance and Shadow.' In the second part, the author endeavors to show that "the cosmic process exists purely for the sake of moral ends"--quite too serious a proposition for so light a book. He has much to say of the prolonged infancy of man; but he does not attempt to refute the alleged facts that have again recently been put forth and tabulated, to show that the duration of man's infancy is related to the length of his natural life in the same way as that of all other mammals. We remark, too, the lack of any clear distinction between cerebral evolution taking place strictly by natural selection (the more cunning and, to some extent, the more good-natured individuals averaging in the long run the larger families) and intellectual development under the influence of tradition, which variations at birth can influence only so far as those individuals who are congenitally suited to accepting established customs, are likely to produce more numerous progeny than those who are congenitally ill adapted to the traditional ideals.

If a "cause," in the sense of an active body of sentiments, can be damaged by an argumentative defence that seems at first sound, but is sure at last to be found worthless, then it may be doubted whether the third part of Mr. Fiske's book is likely to do religion more good or more harm. The nature of his reasoning is sufficiently shown by the following sentences:

"Now if the relation thus established in the morning twilight of Man's existence between the Human Soul and a world invisible and immaterial, is a relation of which only the subjective term is real and the objective term is non-existent, then, I say, it is something utterly without precedent in the whole history of creation. All the analogies of Evolution, so far as we have yet been able to decipher it, are overwhelmingly against any such supposition. To suppose that, during countless

ages, from the seaweed up to Man, the progress of life was achieved through adjustments to external realities, but that then the method was all at once changed, and, throughout a vast province of evolution, the end was secured through adjustments to eternal nonrealities, is to do sheer violence to logic and to common sense. Or, to vary the form of statement, since every adjustment whereby any creature sustains life may be called a true step, and every maladjustment whereby life is wrecked may be called a false step; if we are asked to believe that Nature, after having, throughout the whole round of her inferior products, achieved results through the accumulation of all true steps and pitiless rejection of all false steps, suddenly changed her method and, in the case of her highest product, began achieving results through the accumulation of false steps--I say we are entitled to resent such a suggestion as an insult to our understandings. All the analogies of Nature fairly shout against the assumption."

There is much more of this. But it is mere reiteration. Every reader will see how all this heat and "shouting" contrasts with Mr. Fiske's quiet way of pushing his reasons when he *sees* their force clearly, instead of only *feeling* something, he knows not quite what. To say that "the analogies of Evolution are overwhelmingly against any such supposition" is quite the reverse of the truth. According to accepted ideas of evolution, species do not become adapted to their environment in so far as that environment enjoys abstract "reality" (if that means anything), but only in so far as that environment affects the continued propagation of the species. Correct notions about ways of getting food and the like are developed because the species would die out if they were not. But Mr. Fiske will not be able to point to a single idea which evolution has rendered true in any other sense than that it is favorable to the continuance of the species. He himself, in his second sentence above, defines a "true" step as an "adjustment whereby any creature sustains life"--which is approximately, though not accurately, a good definition for the purposes of evolutionary philosophy. But, in that sense, the development of a wholly erroneous conception of the sun or moon, or of another life, or of anything else which in some respects cannot really influence the species, may be a "true step," provided it be stimulating or tend to sustain life. If Mr. Fiske would content himself with saying that Truth, in any other sense than that of a valuable adjustment, is unattainable, if not inconceivable (as his Pragmatist friends, James and Peirce, contend), his reasoning would be considerably amended.

There are several passages in the book which remind us that Mr. Fiske is not a thorough-going evolutionist, but is a follower of Spencer, who holds that Evolution and Devolution ceaselessly alternate under the influence of an immutable law that knows no growth, no cause, no reason; so that not evolution, but immutability, according to his account of the matter, is the general characteristic of the universe.

69 (24 August 1899) 154: NOTES

Attributed to Peirce by Fisch in *First Supplement* (probably by Peirce). Peirce had reviewed Fraser's edition of Berkeley, and would have been a logical choice to notice this edition. This note is unassigned in Haskell's *Index to* The Nation, vol. 1.

--We are now presented with a fifth edition of the good Bishop's philosophical works in three volumes of Bohn's Libraries (New York: Macmillan); and this edition will best answer the purpose of the majority of readers. The new editor, Mr. George Sampson, has given us the complete philosophical Berkeley, and nothing but Berkeley, save for the indispensable brief histories of the several publications, and an old Biographical Essay by Arthur James Balfour, containing many fine observations--altogether quite a charming thing; not omitting very much, either. The works are, in this edition, printed in the order of their original publication, but with the author's own definitive text. In reprinting them, modern critical scrupulosity is carried to its highest pitch, quite beyond Fraser. The "Querist," for example, having been much changed in the second edition, is here printed twice, so as to exhibit both forms. Facsimiles of the original title-pages are given, and two portraits of the Bishop, one from the painting in the National Portrait Gallery, the other in the family group from a replica of the Yale portrait. The painter in both cases was John Smibert. The only thing we regret in this edition is that it should be confined, albeit not strictly (the Guardian papers being included), to the philosophical works. Perhaps a fourth volume will remedy that. The celebrated verses in which Berkeley predicts that America will be comparatively free from the conventionalities of schools and of courts are, however, inserted so as to give an opportunity for the conventional British sneer by Mr. Sampson.

69 (7 September 1899) 192-193: From Comte to Benjamin Kidd.

By Robert Mackintosh, D.D. The Macmillan Co. 1899. 8vo, pp. 312.

Better-World Philosophy.

By J. Howard Moore. Chicago: The Ward Waugh Co. 1899. 12mo, pp. 275.

CSP, identification: Haskell, *Index to* The Nation. See also: Burks, *Bibliography*; *List of Articles*.

Here are two answers to Kidd's 'Social Evolution.' It is a new indication of the usefulness of extreme positions in philosophy that that work should still be evoking refutations and replies. Dr. Mackintosh's book reviews the whole history of the application of biology to ethics from Comte down, and gives serious criticisms of the doctrines of Comte, Hatch, Spencer, Leslie Stephen, Miss Cobbe, Bagehot ('Darwinism in Politics'), S. Alexander, Huxley (Romanes Lecture), Drummond ('Natural Law in the Spiritual World' and 'Ascent of Man'), Sutherland, Ritchie ('Darwinism and Politics'), and Kidd--making a valuable history of this movement of thought. There was no decisive reason for beginning with Comte. The

author might as well have gone back at least as far as Cabanis, the original author of the phrase, "The brain secretes thought as the liver secretes bile," usually attributed to the compiler Büchner. Although Cabanis insisted that "the soul is not an entity, but a faculty" (a phrase, by the way, to be noted by those who imagine that all psychologists before Herbart regarded faculties as entities), and maintained distinctly that psychology is a branch of physiology and is to be studied in the physiological laboratory, yet he held that morality is, in some sense, obedience to the will of "the first causes." In truth, the idea of founding ethics on biology may be traced back to the beginnings of modern science. Its germ may be found in Servetus, who thought that, in order to understand the soul and its workings, the motion of the blood must be studied, and in Bernardino Telesio, the father of sensationalism. Still, Comte makes a convenient starting-point, being the earliest of the thinkers of this class whose works are still much read, whose influence is distinctly felt, and whose school survives.

Dr. Mackintosh devotes more than one-sixth of his book to Comte; a disproportionate space, considering how far the author of the Positive Philosophy was from anything like Darwinian ideas. Dr. Mackintosh's criticism seems to be animated by a spirit of fairness, and is certainly thoroughly studied. His ways of thinking, however, are not those of a scientific man. He sometimes intimates dark misgivings as to the foundations of what he calls "(finite) science"--suggestions about as profitable as inquiries on the part of a leper as to whether his leprosy was legitimate. What each generation has to do is to follow out the path that lies open to it-which for us is the path of scientific investigation. About the logic of scientific hypotheses and the logical status of natural selection and of evolution generally, Dr. Mackintosh's ideas seem pretty confused, as a long chapter on "The Metaphysics of Natural Selection," the weakest in the book, shows.

However needful biologists may find it to admit, for the present, that natural selection has been the main agency in the development of species, yet the presumption is that any hypothesis concerning so complicated a matter, let it seem at first to accord as well as it may with the facts, will come in time to be profoundly modified, just as the hypotheses of general physics are undergoing modification, although this is a far simpler subject than biology. The physicists are unwilling to admit that there can have been life on the earth for so long a period as the Darwinian theory seems to require, or that the history of the globe has been so uniformitarian as Darwin, with unwonted warmth, assumed. The biologists themselves tell us that the life history of the inidividual [[sic]] reproduces, in outline, that of the development of the race. Now, individual development is at one stage very rapid and at another very slow. Moreover, if Darwinism has any lessons for ethics, we must suppose that intellectual and social development is due to the same general causes as the development of species. But no form of psychical development has, so far as history can trace it, proceeded at a nearly uniform rate. Meantime, there are very few cases, if any, in which we can say of any observed phenomenon that it certainly would have resulted from the action of natural selection; all we can usually say is that it very likely might have so resulted. But, as Dr. Karl Pearson points out, almost anything might result from natural selection. It is too elastic a theory to be very

certain. For these and other reasons, though there is no doubt natural selection does act, yet we may not irrationally deem it unlikely that the adaptations of means to ends throughout nature are to be mainly accounted for by minute fortuitous variations together with the elimination of forms unsuited to those ends. Our direct observational knowledge of biological variations in reproduction leaves us altogether ignorant of whether there are any adaptations to needs in those variations, or whether they are quite fortuitous. We know no more about this than we do as to whether the ideas suggested by the law of mental association are, in their nascent state, at all adapted to ends or are purely random. To be confident that it is not so would be rash. But this does not justify Dr. Mackintosh in finding fault with Darwin's procedure in assuming the variations to be fortuitous. Science is not a fixed, unchangeable body of propositions. After a thousand years the general face of science may be modified past recognition. Scientific hypotheses are questions put to nature. In the game of twenty questions no skilful player begins by guessing what he thinks most likely. He seeks to fix one feature at a time. Scientific research is a much more intricate business, and various considerations go to determining what is the best hypothesis to try. But it is certain that if Darwin had made his hypothesis such as Dr. Mackintosh would have it, he would have blundered grievously in asking in one question what ought to have been asked in two.

Mr. Moore's book is as different from Dr. Mackintosh's as it well could be. Dr. Mackintosh does nothing but argue. Mr. Moore does not argue very much; he expresses his sentiments in a forcible and lively manner which is rather persuasive. Those sentiments, except, perhaps, in their strenuous intensity, are not particularly novel. He believes as thoroughly as Mr. Kidd in the natural egoism of man, and thinks that things never can go right until this is recognized, and until the main effort of education is directed towards its cure. He is particularly shocked at the manner in which man enslaves the brutes. He seems to forget that a horse must be treated as he is--not so very cruelly, by the way, with his valet and his every need provided for--or he would not be born at all. But human nature must be revised "with revolutional intent." Mr. Moore is quite sure "the sun will yet pour his fire upon an age . . . when it will be a crime for malfactives to beget." Mr. Moore spells *though* "tho" and *through* "thru." We know not how he would spell *height*, for his book does not, we think, contain the word. It is not one Mr. Moore would be likely to use. He would probably prefer *celsity*, or *altiment*, or *vertilation*.

69 (21 September 1899) 231: Mathematical Essays and Recreations.

By Hermann Schubert. Translated by Thomas J. McCormack. Chicago: The Open Court Co.

The Study and Difficulties of Mathematics.

By Augustus De Morgan. Chicago: The Open Court Co.

CSP, identification: Haskell, *Index to* The Nation. See also: Burks, *Bibliography*; Fisch and Haskell, *Additions to Cohen's Bibliography*; MS L

159.118.

Augustus De Morgan (1806-1871) was one of the most famous mathematicians of the nineteenth century. He was educated at Trinity College, Cambridge, and was fourth wrangler in 1827. He was appointed professor of mathematics at University College, London, in 1828. His chief works are *Formal Logic* (1847) and *Essay on Probabilities* (1838).

Prof. Schubert's reputation will not be enhanced by his 'Mathematical Essays.' His essay on circle-squaring, with its ungainly German style, has to compete with the 'Budget of Paradoxes.' It contains nothing more except ancient quadratures noticed in every recent history of mathematics, Lindemann's demonstration of the impossibility of the geometrical problem, which is far better treated in Klein's 'Problems of Elementary Geometry' (Ginn & Co.), and one vague notice of a Hamburg crank. An essay on the mathematically uninteresting subject of magic squares might easily have been compiled from President Barnard's old book on the subject. A paper on the fourth dimension is interesting only where it touches on the other world, where it ceases to be mathematical. Two chapters on number are commonplace and inferior. A discussion of the nature of mathematical reasoning seeks to carry us back to the ideas of the last century. It concludes with this remark:

"Mathematical knowledge, aristocratic as it may appear by the greater certainty of its results, will, so far as the advancement of human kind is concerned, never be more than a useless mass of self-evident truths, unless it constantly places itself in the service of the other sciences."

This condemns the great body of recent mathematics; but it is sufficiently refuted by Prof. Schubert's own chief contribution to mathematics, which in no way considers the desiderata of other sciences, but is crowded with such propositions as this: "The number of surfaces of the second degree touching any nine given surfaces of the second degree is 666,841,088." These are certainly not "self-evident truths."

De Morgan's book is one we are glad to see again. Though it was never a work of any moment, it might, especially when it was first printed in 1836, be useful to those whom all mathematics puzzle. Since that time, mathematics has been revolutionized, above all in its philosophical aspect, and nobody did more to refute some of the logical notions here put forth than De Morgan himself. The portrait of the author is a likeness, though a very wooden one, altogether inferior to the photograph in his widow's memoir of him.

These two books are the second and third volumes of an easy mathematical series of publications, of which Lagrange's 'Lectures on Elementary Mathematics' formed the first. The undertaking is commendable. We suggest that the editor would do well to have somebody at his elbow to prevent his speaking of Schubert as mainly distinguished as the author of a school arithmetic, or of De Morgan as a man to be compared with Huxley and Tyndall, but far higher than either of those two as an investigator of his science. The publishers should also understand that mathematics has been and is still advancing quite as rapidly as chemistry, and that an old survey of mathematics is of as little use, except historically, as an old text-book of chemistry. It does not need a Lagrange to write an introduction to elementary mathematics. A paper-weight will fulfil its function no whit the better for having been broken off the Great Pyramid. What is required is a sound, all-round understanding of the drift of modern mathematics, especially in its philosophical and logical aspects. There are many men and women in the country who could write much better books for modern beginners in mathematics than either Lagrange or De Morgan, or any other of the great men of the past.

69 (28 September 1899) 248-249: **Observational Geometry**.

By William T. Campbell. New York: Harper & Bros. 1899.

Attributed to Peirce by Fisch in *First Supplement* (internal evidence). This review mentions a distinctive set of mathematical issues upon which Peirce was working at that time. This notice is unassigned in Haskell's *Index to* The Nation, vol. 1.

Mathematicians and non-mathematicians have for generations been agreed that Euclid and Legendre do not furnish the proper introduction to geometry, but nobody has yet succeeded in producing any primer of the subject that is really satisfactory. Vast numbers of persons interested in education, but strangers to modern mathematics, consider themselves highly proficient in geometry, and fancy that they are amply equipped for writing an elementary text-book. In one thing they all agree: it is that the first steps in geometry should be observational. So far, they are doubtless right, but as to what course geometrical observation should take, how it should be directed to the strengthening of the geometrical imagination, and ultimately to the education of the logical powers, they are mostly not sufficiently well acquainted with geometry to judge. Whether or not Mr. Campbell belongs to this class of teachers, his book shows no trace of his ever having studied topology, or even having reflected much on perspective. It is surely one of the first principles of teaching that ideas ought to be inculcated one at a time. Now geometrical metrics, as every mathematician knows, involves the principles of graphics, and it is obvious that graphics, in its turn, involves geometrical topics, or topology, the doctrine of the modes of connection of the parts of different shapes--which shows us, for example, that if a half-twisted ribbon has its ends joined to form a ring, and is slit down the middle all around the ring, the result is a large ring composed of ribbon having a complete twist. Hence to begin the teaching of geometry with metrics, as Euclid and almost all other teachers do, including Mr. Campbell, is to huddle upon the unfortunate child three different orders of ideas at once. Topics, on the other hand, being undoubtedly the easiest part of geometry, the part in which demonstration has the smallest part to play and observation the greatest, the part in which the pupil is most inevitably, easily, and almost unconsciously led from observation to generalization, and the part in which imagination is most evoked, would seem, on every account, the most suited to the child.

But even if we agree to beginning instruction with metrics, we cannot assent to the extraordinary entanglement of different conceptions belonging to metrics which Mr. Campbell's book, even more than others, offers to the bewilderment of the pupil. It should be remembered, too, that the pupil, however tender his age, has already been a student of geometry, in his way, before he comes to the teacher. He must have been so in order to find his way about the house, for

example. Now it seems preferable that his new geometrical observations should be connected at the outset as closely as possible with those he has already made, instead of with such unfamiliar things as cubes and rectilinear figures.

69 (19 October 1899) 303-304: The Boy's Book of Inventions: Stories of the Wonders of Modern Science.

By Ray Stannard Baker. Doubleday & McClure Company. 1899. 8vo, pp. 354.

CSP, identification: Haskell, *Index to* The Nation. See also: Burks, *Bibliography*; Fisch and Haskell, *Additions to Cohen's Bibliography*; MS 1418 (draft).

Ray Stannard Baker (1870-1946) was an American author who also wrote under the pen name of "David Grayson." Baker's journalistic career spanned what has been called the muckraking period of the American press. His writings indicate a deep interest in the plight of those who fought against the industrial evils of the time. Baker was a close friend of Woodrow Wilson, who appointed Baker official representative of the American press at the Paris peace conference in 1919. Baker was awarded several honorary degrees, among them an LL.D. by the University of Michigan in 1917 and a Litt.D. from Amherst College in 1925.

Here is a fairly good book for boys, telling about automobiles, tall buildings, Lake's submarine boat, the new kites, the phonograph, Langley's aerodrome, wireless telegraphy, liquid air, and the Roentgen rays--subjects ranging from those whose principles are obvious for every boy, to those which must remain mysteries in his mind; from those which solve practical problems whose chief factors a boy can profitably consider, to those whose practical side is not yet understood; from those which depend upon no new knowledge, but only upon new economic conditions, to those which have startled the scientific world. Mr. Baker has made his book entertaining. He has not loaded it down with information. On the contrary, it must be an inactive-minded boy who is satisfied with what he finds here. The question of dollars and cents is, in most of the chapters, brought to the focus of attention. The purpose seems to be to turn the boy's love of the marvellous to account in order to impress him with conceptions of the great science of economy. Mechanical contrivances, his natural delight, are kept scrupulously out of view. Purely scientific matters are apparently not deemed important enough to call for any great accuracy of statement. Thus, to give one example out of many, the boy is told that heat, light, and electricity are all vibrations of the ether (p. 83); although, in fact, heat is the energy of relative motion of the minute parts of ordinary matter, and is not necessarily vibratory, while a charge of electricity, whatever it is, is certainly not a vibration. It would have been perfectly possible without making the book any the less entertaining, to have given it a high value for the boy's growing understanding of the scientific points involved in the different inventions, so that he should treasure and cherish it more and more with advancing years. Its thick, glossy paper, almost like Bristol board, suggests that it was intended to be read many times and pondered deeply; but such an expectation will be disappointed. A book after a boy's own heart, therefore, it is not quite, though a boy will be glad enough to get and read it. The illustrations are interesting and not extravagant.

69 (9 November 1899) 355-356: FORD'S FRANKLIN

The Many-Sided Franklin.

By Paul Leicester Ford. The Century Co. 1899. 8vo, pp. 516.

CSP, identification: Haskell, *Index to* The Nation. See also: Burks, *Bibliography*; *List of Articles*; MS L 159.122; MS 1419 (draft).

Paul Leicester Ford (1865-1902), a member of the American Hall of Fame, was the great-grandson of Noah Webster. During his prolific career, Ford wrote or edited over 70 books. His life came to a tragic end when he was shot by his brother, Malcolm Webster Ford, who committed suicide immediately thereafter.

Mr. Ford applies to our great diplomatist, politician, agitator, wit, moralist, inventor, and natural philosopher that same method of characterism he lately applied so successfully to Washington, and which, in a general way, had already been applied, for example, by Walewsky to Catherine II., and, still better, by Alfred Levy to Napoleon. That is to say, he considers Franklin successively under all possible aspects in as many separate chapters. As the progress of psychology gradually imparts to biography a deeper scientific seriousness, this method will, no doubt, be more and more applied and improved. Its merits are no less striking for artistic than for scientific purposes. It enables one to gain an intimate acquaintance with a great man that no chronological narrative of the events of his life could possibly confer. By always keeping in view some definite question, it holds the reader's attention without effort or fatigue for him. It is the artistic side of the method, apparently, to which Mr. Ford has been attracted. His design seems to have been, by skilfully fitting together a multitude of small items with little comment or cement, to produce the brilliant effect of a mosaic picture; and in this he has succeeded. The general effect is most lifelike. But a mosaic, however beautiful, always leaves much to be desired if we seek in it a representation of fact. Nobody would dream of employing it to illustrate the description of an animal or plant; and Mr. Ford, by his particular way of following out the general method he has selected, is forced to renounce all attempt at anything like a psychological analysis or explanation of Franklin's idiosyncrasies. He must stick to the concrete for the sake of his mosaic effect, and indulge in no other generalizations than such as everybody uses in speaking of any person's character. The result is that the work, considered as conveying information and regardless of picturesqueness, is more a conveniently arranged assortment of facts to serve as a basis for a thorough study of Franklin, than an essay towards a clear and unitary conception of his mental constitution.

The volume reproduces no less than seven portraits of the American sage, without counting the Boston medal (p. 86). The frontispiece shows the soft, characterless thing in the Harvard Memorial Hall. There is a work of the Scotch painter, David Martin (p. 266), very handsome and winning, but, as a likeness, unconvincing. There is (p. 435) a rough caricature, valuable as proving to those who have attributed the slightly projecting lower jaw to false teeth (a suggestion evincing small research into Franklin's family) that this was already a salient feature at the age of fifty-eight. These three portraits are all wigged, and are

doubtless earlier than the others. There is (p. 470) a miniature with an air of self-assertion, taken perhaps in 1774. There is (p. 40) a drawing in profile by the amateur Carmontelle, which quite bears out the reputation of the inventor of the *proverbe* as a producer of breathing and piquant likenesses. As might be expected, it exhibits Franklin as a wit. There is (p. 395) a profile sketch by West, seemingly very accurate. Lastly, and best of all, there is (p. 465) a portrait by West in an unfinished group of the American Peace Commissioners of 1783. This carries conviction in every respect but one--it is difficult to imagine that so vigorous a countenance belongs to an invalid of seventy-seven years. We give the pages on which these portraits are to be found, because everybody who looks over the book will wish to compare them. He will endeavor to form a mental composite out of them; and if he has enjoyed the acquaintance of a number of Franklin's descendants, some remembered features from those sources will contribute to the image. The same thing is true of one's efforts to realize the social impression that was so important a factor of Franklin's success. Here, too, one will, if he is in a situation to do so, avail himself of a class of facts that Mr. Ford could not very conveniently include, and which, not to be personal about men and women now living, we may content ourselves with exemplifying by recalling to those who knew him how much there was in the eminent geodesist, Dr. Alexander Dallas Bache, to persuade one that one saw in him something of the captivating mixture of geniality and finesse that must have shone in his great-grandfather.

Prof. Lombroso, in arguing his thesis that genius is a sort of insanity, does not shrink from mentioning William Shakspere [[sic]]; but he never once finds it convenient to draw his reader's attention to Benjamin Franklin. Is Franklin, then, not universally acknowledged to be a man of genius? If he was not so, one thing about him which produced many of the effects of genius was the strength and completeness in him of all the instincts of the normal man. Less hastily impulsive nobody could be. His colleagues complained of his excessive disinclination to come to any decision about most matters. That was because he habitually distrusted reasons. He was fond of joking about the deceptions of intellect. "So convenient a thing it is," he would say, "to be a reasonable creature, since it enables one to find or make a reason for everything one has a mind to do." But when the "subconscious self," as it is nowadays the fashion to call it, gave forth any utterance about men, in that he would confide; and the event almost invariably justified his confidence. In the noontide market-place of rationalism, as the Paris of his day surely was, though naturally irreligious, he continued steadfastly to believe in prayer and in future rewards and punishments. The very nature of the reason he gave himself for this belief, namely, that it was a wholesome one, suffices to show that something deeper than reason was his veritable guide unknown to himself. His common sense, the strength and normality of his unanalyzed judgments, his complete human nature, were what enabled him to acquire his knowledge of men and his skill in dealing with them; while his susceptibility, generosity, gentleness, and warmth sprang from the same root. "Friend," said a contemporary Quaker, "did thee ever know Dr. Franklin to be in the minority?"

It is plain enough that neither Franklin's wit nor his scientific sagacity, in which two powers his genius shone the brightest, could be an effect of instinct. Mr. Ford has a chapter entitled "The Humorist." Perhaps it is not quite accurate to dub Franklin a humorist. The French say of themselves that they cannot understand Anglo-Saxon humor. Certainly, in the days of classicism, a humorist proper would hardly have been relished in France as Franklin was relished. What is called Franklin's humor is a quality not altogether disparate from Voltaire's wit, albeit in buoyant gayety it may have fallen short. It would be easy to select samples of the two writers that should be, we will not say indistinguishable, but quite of the same stamp. This goes to prove how extraneous to the real man the accomplishment, wit, is; for it must be granted that two sons of Adam never were more utterly foreign to one another than the excitable Voltaire, so often childish, petty, wicked, and the simple, not too fine-spun Franklin. They had, no doubt, their curious points of contact, that might throw some light on both of them. Their wit was one such point. Whatever this was in Voltaire, in Franklin it was an artifice founded on a desire to say something cheery and animal-spirited in his newspaper, whittled to laconics for his almanac, perfumed with French essences for the diplomat's purposes, and usually decorating some reflection on human nature. The humanity of the man was an essential ingredient--the most substantial ingredient that was not quite factitious. That Franklin himself did not esteem his wit or humor as belonging to his inmost self is shown by his fancying he very strongly resembled a man so remarkably devoid of it as C. A. Helvétius, who, by the way, defined very well, in his own solemn fashion, the distinctions between the different genera of pleasantry in the last part of his principal work. According to those distinctions, Franklin ought, we think, to be called in English a wit and not a humorist.

We do not deem it needful to expatiate upon how well Mr. Ford has treated the literary side of Franklin, because that will be taken for granted by the entire reading public. The scientific side is less well done. To begin with, the mosaic art does not lend itself very well to this subject; and then Mr. Ford does not sufficiently distinguish between the inventor and the scientific discoverer. Thus, he speaks of the Franklin stove and the lightning-rod as important discoveries. He quotes, apparently with approval, at any rate without a jeer, Jefferson's stricture upon the chemists of his day as not sufficiently confining their attention to matters of human utility. That is, he would have had Lavoisier, Scheele, and Priestley tread the pathway of Boerhaave, and Lemery, and the Cadets, who were a sort of apothecaries. If they had done so, the creation of chemistry would have been postponed to a wiser generation. Jefferson must not be blamed for not seeing how the new chemistry was destined to revolutionize human life; but can any instance be imagined that should more completely refute the policy of restraining inquiries seemingly useless? The true devotee of science, so long as he enacts that rôle, never thinks or cares about Philistine utility. In his mind, to learn the ways of Nature and the reasonableness of things, and to be absorbed as a particle of the rolling wave of reasonableness, is not *useful*, but is the *summum bonum*

itself towards which true usefulness tends. At the same time, when one descends to the question of food and raiment, warmth and cleanliness, to decree that the scientific investigator shall pursue utility alone, can only mean that he shall pursue nothing but what appears to be useful in advance of investigation, usually among the less useful class of inquiries even in the most grovelling sense. Dr. Franklin ought to have considered that before he asked: "What signifies philosophy which does not apply itself to some use?" It was precisely that utilitarian spirit which made the eighteenth century a scientific desert. Franklin's remark, however, is valuable to us as showing what an unraised spirit of plain instinct and common sense was his.

Mr. Ford does not furnish sufficient data about Franklin's electrical researches to enable us to gauge his scientific powers. In eighteenth-century fashion, he puts the emphasis upon the identification of lightning with electricity--a contribution to meteorology and not to pure physics. The idea was not at all new, and probably not original with Franklin. His argument for it, which reads for all the world like an example out of the Port Royal Logic, was marked by his usual good sense and penetration. In the experimental verification he was anticipated by two other electricians, and his own showy demonstration was soon abandoned by him for their method. So far as the present state of electrical theory encourages us to venture an opinion, his single-fluid theory of electricity was probably substantially correct--at least, as against the two-fluid theory; but his argument about it has absolutely no value at all. He was led to the truth in this case (if it was the truth) by an operation of the mind of which he could give no rational account, so that this is another illustration of his subconscious strength. That which was really the best in his electrical work was his analysis of the phenomena of condensers; although he was not the first in this field. Here he was for the moment seduced from his eternal practicality, and appears as a genuine physicist. Mr. Ford gives a relatively better account of Franklin's studies of the Gulf Stream and of the effect of oil upon ripples and waves. But what strikes us most here is that, having got notice in advance of other scientific men of phenomena of great importance, he was only able to treat them in an amateurish and feeble way. There was, no doubt, every excuse for this; but the fact remains that these things illustrate better Franklin's sagacity in seeing that there was something important to be learned, than his power of bringing that something into the light of reason. The study of his scientific work strengthens our conviction that it was the general balance of the whole man that produced and still produces the impression of greatness. It was not reason, or focussed intellect, although he was eminent in that respect, too.

We shall not do Mr. Ford the injustice of making any excerpts from his book. Anecdotes that, when fitted into their places in the mosaic, are effective enough, would seem amazingly flat and dull if taken out and scrutinized by themselves; they have suffered enough in their first transplantation. The volume contains portraits of Franklin's acquaintances, facsimiles, and other valuable illustrations in such number that the search for a particular one in the unorderly list is a little

onerous. The index is copious. The book is printed with all the taste and pomp that Mr. De Vinne commands; the plate-finished paper is good of its kind. The volume has a cover of which the possessor will never tire.

69 (14 December 1899) 455: The Life of James Dwight Dana, Scientific Explorer,

Mineralogist, Geologist, Zoologist, Professor in Yale University.

By Daniel C. Gilman. Harper & Brothers. 1899. 8vo, pp. 409.

CSP, identification: Haskell, *Index to* The Nation. See also: Burks, *Bibliography*; Fisch and Haskell, *Additions to Cohen's Bibliography*; MSS L 159.125-126.

Daniel Coit Gilman (1831-1908) became the first president of John Hopkins University in 1875, having come to that post from the University of California, which he had served since 1870 as its first president. Gilman wrote for the *North American Review* and the *Cyclopedia of Political Science*. Among his published works are a memoir of James Monroe and editions of Francis Lieber and Joseph P. Thompson. Honorary degrees of LL.D. were bestowed upon Gilman by Harvard, Yale, Columbia, St. John's, and the University of North Carolina.

If the study of human character ever becomes a science and it is desired to treat the Man of Science as a distinct variety of the *genus homo*, no better type-specimen can be selected than James D. Dana. He opened up no new intellectual world, as Darwin and, in some measure, Helmholtz, did. Such men are not normal specimens of the Scientist. Nor were his achievements as brilliant, say, as those of Riemann, of Faraday, of Mendeléef. But that was owing to the nature of his branch of science, geology. Dana, the abundantly thorough geologist, he might have been called; the other four epithets of the title-page being swallowed up in this one. In Riemann's science, mathematics, achievement is the easier for the circumstance that only one kind of ability, the pure exercise of intellect, is called for. In tracing the laws of electricity and in other such nomological research, to some mathematical genius (which, as in Faraday's case, need not recognize itself as such) must be added a power of analyzing phenomena, together with those of devising and executing decisive experiments. The discovery of a true classification--a classification which, like Mendeléef's, is to resist the ravages of time--demands all the powers of the nomologist, and in addition a far finer observation--that observation which awakes to the significant thing like a mother to her infant's voice, and seizes upon characteristics which, though they be known and recorded, are by ordinary men passed over without appreciation of their bearing. But to pursue an explanatory science, like geology, with success, one most be provided with all those mental engines and more besides. After all, what is chiefly requisite in classificatory science is to sit down and listen to the voice of nature until you catch the tune. But concerning causes nature is not communicative. They are the secrets of the sphinx. She will vouchsafe no more than a terrible monosyllabic "no" to one guess after another whose making may have cost lives. The invention of the right hypothesis requires genius--an inward garden of ideas that will furnish the true pollen for observation's flowers. And the framing of the hypothesis is merely the preparation for the main work of verification--of pressing Nature with question upon question until she is forced to a tacit confession; a work demanding the most varied powers, above all that kind of observation which is called "shrewd."

Dana, for example, not only showed himself a good technical mathematician in his treatment of crystallography in the fourth and earlier editions of his 'System of Mineralogy,' but also was able, in an untechnical way, to produce a mathematical analysis of problems arising in geology. He early showed an aptitude for chemistry, and published two papers upon cohesive attraction, a subject inviting only to a man of nomological ability. One of the deepest-going exhumations of his research, the law of cephalization, belongs to this division of science. In two classificatory sciences, mineralogy and zoölogy, his superiority was acknowledged. In geology, while he would be the last of men to neglect details, yet his eye was always turned to the greatest problems--such as the permanence of continents and oceans, the general state of the earth's surface as a whole at different epochs, and the like. He always generalized. His studies included the moon as well as the earth, and he looked upon geological history as a type of evolution in general, or progress from the homogeneous to the heterogeneous.

Dana pursued the most difficult of the sciences, barring none, in a more thorough and comprehensive manner than any other man of his generation. At any rate, he and his scholars have made America the headquarters of geology. But that which, above all, renders Dana the type of the scientific man in general, taking physicists and psychicists (*i. e.*, psychologists, anthropologists, archæologists, philologists, historians, etc.) together, is that whatever matter seriously engaged his intellect, that he must study with the most systematic and laborious dredging. The inevitable effect of this is to bury a great part of the man under drudgery, so that the narrative of his life requires an intelligent running commentary to bring out the interest of it. For Dana, especially, whose habits were formed before the days of typewriters, and stenographers, and the other paraphernalia of modern wealth--a man who himself travelled to the post-office several times a day, and managed with such poor means as college professors before the war had at their command--this is peculiarly true.

The great amount of drudgery in Dana's life has, perhaps, given President Gilman the impression that his subject was not a very interesting one, or, at any rate, has led him to lose no opportunity of inserting matter that does not relate to Prof. Dana. Such are an extract from a sermon by Jowett about the universe, two of the three chapters on the United States Exploring Expedition, sketches of other officers of Yale College, an account of the foundation and early history of Silliman's *Journal*. We are willing to admit that it was the biographer's duty to show how the promoter, the author, of the United States Exploring Expedition, which went far to redeem America's reputation, being too single-hearted a man to blow his own trumpet, has been allowed to fall into oblivion, after the custom of republics, so that biographical dictionaries hardly know the name of John N. Reynolds. The other insertions, too, are decidedly interesting in themselves. Could they not have been compassed without creating the impression of fleeing to them for relief from the dryness of the main matter? We will say no more on that head. We might perhaps have excused ourselves altogether from adverting to the degree of literary

mastery employed, on the ground that the book is nearly made up of excerpts, and that those excerpts are very well worth reading. It is also fair to consider that the task of preparing this biography was one not sought by the author, and which he could not well have declined, remote as are his own occupations from those of the geologist. His well-known sympathy for science and scientists made it natural to select him rather than another friend for this office.

The book is pretty enough. To some eyes it would have been more so had there been somebody to see that the number of portraits of Prof. Dana the volume contains were rightly counted, that the names of persons mentioned, such as Benjamin Peirce and Daniel Huntington, were always correctly given, and the like. It is difficult to believe that one of the De Saussures made a mistake in French for every three lines of print his letter fills, or that Milne-Edwards should have doubled this proportion. But these are symptoms of a brief transition period in the history of a great publishing house, for which all readers must be inclined to kindly indulgence in remembrance of the benefits and pleasures of the past.

1900

70 (4 January 1900) 11: NOTES

CSP, identification: Haskell, *Index to* The Nation. See also: Burks, *Bibliography*; Fisch and Haskell, *Additions to Cohen's Bibliography*.

We can hardly more than mention the extensive 'Treatise on Crystallography,' by Prof. W. J. Lewis, published by the Cambridge University Press (New York: Macmillan). It will be indispensable to all who have to do with crystals. We will, however, permit ourselves a single observation. Seeing that every descriptive notation for a crystal-face is nothing but a way of writing the analytical geometer's equation for the intersection of that plane with the plane at infinity, it would seem that to undertake the study of crystallography without having first mastered the modern methods of plane analytical geometry would be one of those feeble, half-prepared ways of working that vernacular speech calls "slouchy," and which no course of instruction ought to contemplate as admissible. But a student who has thus prepared himself will find a great part of Prof. Lewis's work tedious, because it is needlessly prolix and is complicated with such inessentials as a third dimension. It is true that the final chapter partly remedies this fault, in the same sense that the fatigue of a five-act drama might be said to be relieved if, at the end, the green curtain were to be rung up for a one-act condensation of it. Yet even this supplementary chapter is not as modern as it should be.

70 (4 January 1900) 12-13: NOTES

CSP, identification: Haskell, *Index to* The Nation. See also: Burks, *Bibliography*; Fisch and Haskell, *Additions to Cohen's Bibliography*.

--Concerning the 'Lectures on Memory Culture, consisting of the Famous Lectures Delivered throughout the United States and England,' etc., by "Dr. Edward Pick, Ph.D., M.A.," etc., etc. (E. L. Kellogg & Co.), Prof. James certifies that "there is absolutely no element of charlatanry about them," and that they "are based on solid psychological principles." For a man who appeals to the million to unite those two contrary virtues is harder than to pass through the eye of a needle, since the profession of psychology, like that of medicine, inculcates a little imposing upon people. The mnemotechnic systems of Loisette and White, as well as earlier ones, whose absurdities Dr. Pick puts in a strong light, were likewise based on some sound psychological principles; only they unfortunately left others out of view. Dr. Pick does much better, and it will undoubtedly surprise most people to find, for instance, that in from three to five minutes they can indelibly impress upon their memory all the French nouns which are feminine by exception. Yet Dr. Pick does not entirely escape the old fault of leaving out of account some of the elements of the problem. The problem is to find a method of establishing a mental association such that a given kind of experience shall

enable us voluntarily to recall a number or a word. For this purpose, the first condition to be remarked is that association is between pairs of ideas, and of this principle Dr. Pick makes proper use. The second thing to be remarked is that the suggestion is to have two steps, namely: first, the suggesting experience is to awaken an act of will, not deliberate but spontaneous; and, second, that act of will is to call up the desired word or number. Now an act of will, or what is essentially the same thing, occurs only as an inseparable part of a physiological reaction; and consequently such a reaction must enter into the process of learning. If a person once has a tooth filled by a French dentist, he will never forget the word *plomber*, because a strong reaction has taken place in the learning. For this reason, not to speak of another, in learning a language we ought to associate the foreign words directly with experiences of the things or actions they signify, and not directly with English words. But this is not the only principle that Dr. Pick seems to overlook.

70 (4 January 1900) 18: **A Century of Science, and Other Essays**.

By John Fiske. Boston: Houghton, Mifflin & Co. 1899. 8vo, pp. 477.

CSP, identification: Haskell, *Index to* The Nation. See also: Burks, *Bibliography*; Fisch and Haskell, *Additions to Cohen's Bibliography*; MS 1421 (draft).

Agreeable and profitable reading is assured in any book by Mr. Fiske. The fourteen essays composing this latest volume may not make quite so stimulating a draught as some of his other writings, but what they lack in this they make up in their sparkling flow of information and refreshing good sense. The longest and, perhaps, the most interesting, is a eulogy of the historian Parkman, exhibiting the impressive picture of his life and labors. The two next in order of length are, with one exception, the slenderest in matter. One of these, an account of "Some Cranks and their Crotchets," makes one laugh a little, but the most irresistible things in it are quoted from De Morgan's incomparable 'Budget of Paradoxes.' The other is a refutation, neat as a proposition in Euclid, of the theory that Bacon wrote Shakspere [[sic]]. It is only a pity that the object of attack does not offer sufficient resistance to allow of much strength being put forth against it. Less courteous is a brief insession upon the Rev. Joseph Cook. Three of the remaining essays deal largely with the author's reminiscences. In an account of "Cambridge as a Village and as a City," it is the ancient history that is the most curious. There is little about the aspect of the modern village before its incorporation in 1846, since Mr. Fiske did not see it until fourteen years after that event, when its sweet childish ruralness had given place to an ungraceful hobbledehoy suburbanity. There are two obituary notices of the author's friends, Edward Augustus Freeman and Edward L. Youmans, the latter of autobiographical, as well as other, interest. In a discussion of the arbitration treaty we note that as lately as 1897 Mr. Fiske dreamed that our proud and successful rejection of a standing army might stand as an example that Europe must, in course of time, emulate. So it is that each step toward knowledge of ourselves consists in the dissipation of some illusion.

The author evidently attaches considerable importance to his paper on "The Part Played by Infancy in the Evolution of Man," and in his intimate dedicatory epistle to Prof. Thomas Sergeant Perry he properly reclaims for himself the title to the authorship of the theory that the prolonged period of infancy of men is the cause of the persistence of family relations and so of human society. The objection that man's infancy is not, in fact, particularly prolonged seems never to have received from Mr. Fiske an adequate reply.

An essay on "The Scope and Purport of Evolution" is a protest against the assumption that that doctrine is unfavorable to belief in immortality and in God. Here Mr. Fiske is not at his best. Instead of first endeavoring fully and fairly to state wherein lies the persuasiveness of the line of thought against which he argues, but which has deeply impressed a good half of the thinking men of our generation, he pitches upon a vague and shambling sentence of a German professor, as if showing inexactitude in that could serve to prove there was no justice in the thought behind that awkward mouthpiece. Moreover, instead of considering the natural tendencies of evolutionary philosophy, as such, he limits himself to Spencer's special doctrine, which makes the principle of the conservation of energy the root of all the phenomena of the universe. Now what this results in is a virtual splitting of the universe into two uninteracting departments of matter and of mind, and such a breach of continuity cannot satisfy men long, since it ignores the requirements of the logic of this kind of reasoning.

The strongest of the papers is that which gives its title to the book. This is intended to show that the nineteenth century has been intellectually the greatest of all ages, that the idea of evolution is the greatest product of this greatest age, and that Herbert Spencer is the greatest exponent of this greatest of ideas. Most readers will be ready enough to agree that the trick of inquiring concerning each generic phenomenon how it came about or could have come about, is, on the whole, the most cunning lesson that the nineteenth-century animal has learned. Many among our reading millions innocently suppose that Herbert Spencer invented evolutionary philosophy. They do not realize that what probably first magnetized the youthful Aristotle from a student of medicine into a student of the cosmos was the influence of the strong current of evolutionary thought that had been set up in his environment by Democritus--thought that must have seemed as novel and as scientific to him as that of Spencer first seemed to the youthful Fiske, albeit there is reason to suspect that it was even then ancient lore.

Aristotle, by the way, though he always retained a high opinion of Democritus, did not persist in such unparalleled devotion and faith of discipleship as Fiske retains for Spencer. Some of Mr. Fiske's readers, however, while willing to accord a good measure of applause to Spencer's early discernment of how much slow growth might bring about, will nevertheless remark two circumstances which will limit their admiration of him. The first is, that his 'Psychology' and 'First Principles' were not the earliest expressions of evolutionary philosophy, nor even of evolutionary philosophy of a quasi-scientific cast, for the nebular hypothesis, which to any thinking man carries along with it a general doctrine of biological evolution, was given to the world by Kant in the year 1755. The second circumstance is, that Spencer is really not an evolutionist of a thorough-going

kind, since he explicitly proclaims that, in his opinion, evolution is only one of two alternating processes, evolution and dissolution (the {genesis} and {phthora} of Democritus), which he places on a par, while he further makes both those processes alike mere consequences of an eternal law that never came about at all, the "law of the persistence of force"; and against any attempt to drive investigation into the origin of that, he sets up a warning notice of no thoroughfare. That Spencer saw and felt some truths before almost anybody else, nobody can deny; but how far his writings have really influenced the deeper thinkers of the century, which seems to be the true point in question, is something Mr. Fiske still leaves in doubt. One brilliant disciple the Synthetic Philosophy can boast; but, after him, we can call to mind only men whom it were flattery to call mediocrities in philosophy. In short, the nineteenth century has brought us all to agree that nearly everything is to be accounted for by evolution; but the question as to how evolution is itself to be accounted for, or what rank it is to take among the uniformities of nature or the categories of philosophy, looks to-day less like finding a speedy settlement than it did soon after the publication of the 'Origin of Species.'

70 (25 January 1900) 78: **A History of Wireless Telegraphy, 1838-1899**.

By J. J. Fahie. Dodd, Mead & Co. 1899. 8vo, pp. 325.

CSP, identification: Haskell, *Index to* The Nation. See also: Burks, *Bibliography*; Fisch and Haskell, *Additions to Cohen's Bibliography*; MS 1422 (draft).

Everybody who seeks general information about Marconi's method of telegraphing and its relation to those of Preece, by ordinary dynamic induction, and of Lindsay and others by conduction through water in two paths, will be able to gather a somewhat better idea of these matters from this book than from the little volume of a quarter of the size by Richard Kerr. He will also learn here of some thirty earlier attempts. Steinheil the elder, in 1839, first proposed to telegraph by means of radiation otherwise than by vision. Morse, in 1842, first had the idea of erecting ordinary telegraphs along the two banks of an estuary or strait in order to make two conducting paths through the water. O'Shaughnessy in 1849 tried naked wires under water. Mahlon Loomis in 1872 proposed to telegraph through the upper air. Henry early telegraphed by means of the induction of coils, a method which Stevenson in 1892 practically developed.

The book has two faults that are common to almost all writings of practical men. The first is that is [[sic]] is needlessly technical. A good many people would like to read it who would have been thankful for a glossary explaining what an Obach cell, a D'Arsonville galvanometer, a Theiler sounder, a Cardew vibrator, a Righi exciter, and the like, are; for they will probably search for such expressions in vain in their encyclopædic dictionaries. The other fault is a want of logical distinctness in the thought. The very title of the book illustrates this. Contrivances that depend upon the vision of distant signals are, we believe, the only strictly wireless telegraphs that have ever been proposed. These, too, are the types to which the word "telegraph" was first appropriated. Yet this subject, which has been much studied by soldiers

and sailors since 1838, is omitted by Mr. Fahie. On the other hand, Edison's contrivance (really due in its essence to Willoughby Smith) for communicating with a moving train cannot properly be called telegraphy, since it is only designed to close a gap of a few feet, while a telegraph is an apparatus for transmitting all sorts of messages faster than sound to distances beyond the reach of a man's voice. Nor do experiments with bare wires laid across the bed of a river come under the head of wireless telegraphy. The same defect is shown when Mr. Fahie professes not to understand what physicists mean by calling electricity a kind of matter, saying that engineers understand it to be a form of energy. If there were any real dispute upon such a point, we may be sure, from the nature of their studies, that it would be the students of theory who were right. But, in fact, it is a mere verbal discrepancy, best mended by confining the word "electricity" to vaguely denoting all phenomena depending on the luminiferous ether. Of course, optics is now universally acknowledged to be a branch of the science of electricity.

Notwithstanding these two faults, the work may be profitably read by anybody having a good elementary acquaintance with electricity.

70 (1 February 1900) 97-98: Le Mécanisme de la vie moderne. IIIme Série.

Par le Vicomte G. d'Avenel. Paris: Armand Colin & Cie. 1900. 18mo, pp. 340.

CSP, identification: Haskell, *Index to* The Nation. See also: Burks, *Bibliography*; *List of Articles*; MSS L 159.134-135, L 159.142; MS 1423 (draft).

If the first two volumes of this work are as good as the third, which we venture to doubt, it is well worth binding and putting on one's shelves. This volume treats of the house, of alcohol and *liqueurs*, of heating, and of horse-races. It tells all about these things as they exist to-day in Paris. For example, under the head of the house, it fully informs us concerning the different building-stones, where they come from, what precisely each kind is used for, what various defects may lessen their values, how they are tested, how cellars are dug, how long it takes a man to get out a cubic metre of earth of different kinds, how fast the wheelbarrows move, what the volume of the earth is after it is taken out, how long it takes to shovel it into a cart, where the rubbish is carried, how the foundations are constructed; all the different trades of workers in stone, plaster, and wood; the processes of stone-cutting; the laying of stones, plaster, mortar, cement, "ciment armé"; how the workmen live, how they spend their money, what they lay up, how they bathe, how often they change their sheets; contractors, their methods, their overseers, their profits, the legal requirements relative to different kinds of walls, the different styles of plastering and its substitutes, the different trades of carpenters; roofs of titles, slate, "zinc blanc"; the locks and hardware, the marbles, paint, wall-paper, window-glass, mirrors, bow-windows, elevators, bath-rooms, closets, the dissection of the space into rooms, and so forth, with surprising accuracy. The only errors we have detected are quite unimportant. In short, the general reader will rise from the perusal knowing infinitely more about the way things are done in Paris than in New York.

At the same time he will be supplied with a classified treasury of French terms relative to the arts treated of, where they can be readily recovered when they are needed for use in writing French.

Nor must it be supposed that the account is dry and tedious, for it is anything but that. As the author proceeds, he everywhere compares the present state of the arts of life with those of previous centuries in a way that evinces not only industrious study but real research. The whole is enlivened by many anecdotes and bits of history that are at once instructive, curious, and amusing. The French art of making a book was never more consummate. It is a mass of facts and statistics dressed up to make a charming "article-Paris." The author is of the opinion that it was not during the dark ages that dirt flourished most richly, but in *le grand siècle* of Louis XIV. We confess we can hardly believe he is right about this.

However, he argues that, even in the later Middle Ages, the innumerable bagnios still served their original purpose, while the palace of Versailles had 274 *chaises percées* and not a bathtub. He tells how an aged lady under the Restoration, visiting a chateau impregnated with bad smells, which had already at that date become rare, but which in her youth were usual in the best houses, remarked, "Voila une odeur qui me rappelle un bien beau temps et de bien doux souvenirs!"

Chapter xi., about the house, is so much the best that we suspect the earlier volumes are not comparable to the third. Chapter xii., on alcohol and liqueurs, is, however, also very entertaining and full of information, and chapter xiv., concerning horse-races, will interest a great many readers; for, as M. d'Avenel remarks, of all topics that supply fuel to fashionable conversation, this is the only one about which one can discourse at length without danger of being deemed either a muff or a pedant.

70 (8 February 1900) 109: NOTES

CSP, identification: MS 1424 (draft). See also: Burks, *Bibliography*.

No. 40 of "The Religion of Science Library" (Chicago: Open Court Co.), entitled 'Kant and Spencer,' by Dr. Paul Carus, being a criticism of Spencer and his agnosticism, and a comparison of his metaphysics with that of its true author, Kant, is well worth attention. In some recent remarks on Kant as an important precursor of modern evolutionism, we were probably influenced unawares by one of the chapters of this brochure which had appeared in the *Monist*. We shall not express approval of the acrid tone of the criticism, which is of a kind obsolescent even in Germany. To say that Herbert Spencer has been a man who "shirks the toil of research" is not to invite philosophical discussion, and is really too much. It is difficult to conceive how such personalities can be to the taste of a philosopher, *i. e.*, of a man intent mainly on supplying the defects of his own knowledge. In this country, they will not even serve the purpose of the man who is only eager to teach. Mr. Spencer has surely put out his talent to usury if ever any man did; and what he had succeeded in accomplishing must, in any fair estimate, be called immense--an epithet that leaves room for an infinite shortcoming, of course.

70 (8 February 1900) 114-115: LYON PLAYFAIR

Memoirs and Correspondence of Lyon Playfair, First Baron Playfair of St. Andrews.

By Wemyss Reid. Harper & Bros. 1899. 8vo, pp. 487.

CSP, identification: Haskell, *Index to* The Nation. See also: Burks, *Bibliography*; *List of Articles*; MSS L 159.133, L 159.135; MS 1425 (draft).

Sir Thomas Wemyss Reid (1842-1905) was a British journalist and biographer. He worked as a reporter in his early years and as an editor later. Reid was the manager of Cassell's publishing firm from 1887 until his death. In addition to the work on Playfair, reviewed here by Peirce, Reid also wrote biographies of Charlotte Brontë (1877), W. E. Forster (1888), Lord Houghton (1890), and William Black (1902).

The ambition of Lyon Playfair was at first directed towards scientific eminence. A pupil of two of the greatest chemists that a man born in 1818 could have for masters, Graham and Liebig, he made several discoveries, as all chemists do. The chief of these was that of the nitroprussides. A coffee-colored solution is formed by adding nitric acid to ferrocyanide of potassium, and something peculiar and interesting was known to be contained in it. To neutralize the acid and isolate the resulting salts was no great feat, especially since they yield magnificent crystals. Had Playfair gone on to elucidate the constitution of their acid, he would have taken a considerable step; but this he failed to do. Apart from this, his two chief researches were carried on in collaboration, the one with Bunsen, the other with Joule--men of such fertility that one naturally attributes the main ideas of the work to them. One at least of Playfair's own theories--that the specific gravities of the elements are proportional to different powers of their atomic weights--is downright absurd. On the whole, the evidence is that his purely scientific genius was not extraordinary. That this was the opinion of Faraday is indicated by a letter in which, wishing to recommend Playfair, he limits himself to testifying that "you are able to expound the truths of experimental science in a clear, logical, audible, and to me satisfactory manner."

On the other hand, statesmen like Sir Robert Peel and Prince Albert were struck at their first interviews with him by the evidences of his practical resource, tact, *coup d'œil*, power of elaborating a workable plan, and energy; and, having once tried him in those tasks where physical science and the art of government overlap, would never thereafter allow him leisure for the pursuit of pure science. Thus he was carried in spite of himself into the business for which he seemed born. The ease with which he surmounted difficulties, both on great occasions and on little ones, was pretty to see. "I arrived," he says, in the fragmentary autobiography upon which Mr. Reid builds, "at Kranichstein on Sunday morning. The Prince and his visitors having gone to chapel, and also the servants, Princess Alice kindly remained behind, in order to welcome me on my arrival. While we were conversing, a note came from Prince Louis to say that he would bring the Lutheran minister to the midday dinner. This seemed to disturb the Princess, who told me that her table was small, and that there was absolutely no room for an additional guest, and as all the servants were at church, the table could not be relaid." What could the Princess expect Playfair to do about that?

But people had learned to feel that there was no kind of difficulty which this politico-scientist was not fit to cope with. "I reminded her that she used to entertain me at the Swiss Cottage at Osborne when she was a child, and that I knew she could lay a table better than servants. She was pleased with the suggestion, and we went to the dining-room, took all the things from the table, put in a new leaf, and rearranged everything before the party returned to the house." A man of such American efficiency could not but be liked in palaces; and a little incident is as good evidence as a great one of his practical resource.

Feats of nice tact are not to be explained to all sorts of readers in a few words. Suffice it to say that functions were continually devolving upon Playfair to exercise which, without disastrous friction, was a task harder than ever the guardian of a fairy princess set to semi-miraculous suitor. But Playfair had the art of applying a little drop of lubricant good sense just where it would insinuate itself into the closest bearing. He usually went straight to the persons from whom antagonism was likely to develop. When the Great Exhibition of 1851 was preparing, an executive committee was first appointed by the Society of Arts, and as this did not succeed in executing much, it was supplemented by a Royal Commission of politicians. Still the manufacturers hung back, and at last Playfair was appointed "Special Commissioner," with powers just large enough to insure perpetual jealousies, but not large enough to make these jealousies unimportant. Sir Henry Cole had been the mainspring of the Exhibition from first to last. The reminiscences contain the following narrative:

"When I joined the Executive of the Exhibition, Sir Henry Cole scarcely knew me, and, like the other members, was naturally displeased that I was placed in a position of confidence superior to theirs. On the second day after my appointment I met Sir Henry Cole in Whitehall, at the door of the Home Office. He told me frankly that he was going to see the Secretary of State to resign his connection with the Exhibition, and that his letter of resignation was then in his pocket. I took his arm and walked up and down Whitehall. On asking him whether he believed the ship was sinking, and that the Exhibition would be a total failure, he frankly admitted that he did, as the state of indifference of the manufacturing districts rendered failure almost certain. I then urged that, as he was the real pilot of the vessel, it was a wrong act to desert the sinking ship. The country could be aroused to the importance of the undertaking, and my work could be well separated from his, for I intended to visit the chief manufacturing centres in order to create a public sentiment in its support. Our conversation was mutually satisfactory, and we walked to the Exhibition office together, and his letter of resignation was destroyed. Had the accidental meeting not taken place, the Great Exhibition would never have been held, for its mainspring would have been broken. After this interview, if jealousies still continued, none were ever shown, for all the members of the Executive worked loyally to bring the undertaking to a successful issue."

The excellence of Playfair's *coup d'œil* was shown in this same business. That he should go straight to the manufacturers was only what his invariable habit dictated. But that he should have seen, in the midst of a confused state of affairs,

that what was requisite to bring the manufacturers into hearty cooperation was simply to present to them a new classification of the objects of the Exhibition, such that each of them should clearly comprehend the parts that concerned him-this was what nobody else but Playfair had had the perspicacity to see, or was even able to see after it was pointed out, until the result proved his insight. The same affair illustrated also Playfair's capacity for elaborating a feasible plan. For he had not only to construct a detailed classification in which everything offered for exhibition should find a suitable section that seemed to ask particularly for that very thing, but he had so to construct it that the manufacturers would approve of it; that the Prince, who was wedded to a highly German classification of his own (in which most things either had no place or several places), would yield to it; and, most difficult of all, such that French and other foreign commissioners would surrender their own prepossessions, and cordially accept the new arrangement. But Playfair had all the elements of his problem so thoroughly studied and well in hand that, when it came to the execution, there was not a serious hitch.

As for his energy, we can compare it to nothing but that of a terrier in a room full of rats, with such incredible swiftness did the wickedest difficulties get their quietus under his action. It cannot be better evidenced than by enumerating say a dozen of the main achievements of his life. First, he greatly stimulated scientific agriculture in England by translating Liebig's books and conducting Liebig himself through the country; second, he considerably reduced the death-rate in England by his activity upon the Commission on the Health of Towns; third, if it had not been for him, the Great Exhibition of 1851 would certainly have been a failure, and probably no great international exposition would have been held--to the immense loss of material civilization; fourth, the whole science department, at least, of the establishments at South Kensington is entirely due to his management, together with the School of Science; fifth, he gave the initial impetus to technical education in England; sixth, the regulations of 1874 for filling places in the British Civil Service are due to him; seventh, he invented post-cards in 1870, and caused them to be brought into use; eighth, he considerably furthered first steps towards realization of the great ideal of general international arbitration; ninth, he was instrumental in bringing the Venezuelan imbroglio to a peaceful termination, which was finally effected by the adoption of his suggestions; tenth, he determined the choice of coal used by British steamers; eleventh, he saved the causes of vaccination and vivisection; twelfth, he stopped the cattle plague in England by severe measures. This is not all he did, but we stop the list at a dozen achievements.

At the close of the Exhibition, Playfair received a gold medal, the companionship of the Bath, and the office of Gentleman Usher in the household of the Prince Consort. The Exhibition produced a profit of £ 190,000, and the question arose what should be done with this money and with the building. It was the Prince's idea that an institution should be founded. The House of Commons granted £150,000 additional, and the South Kensington estates were purchased. Playfair now made a tour of Northern Europe and Austria in order to study their educational systems, with special reference to science and technology. Coming

home, he made a crusade in favor of what is called in England "technical education." This he did for the sake of its effect on British industry and civilization rather than for the young men to be educated; and the benefit near and remote to Great Britain at large has been immense. In 1858 he was appointed Professor of Chemistry in the University of Edinburgh, but he was so weighed down with duties imposed upon him by Government that he accomplished little purely scientific work. He did a great deal for educational methods there. In 1868 he was elected member of Parliament for the University of Edinburgh, being a Liberal representing a Conservative constituency; and he gave up his professorship. In 1873 he was made Postmaster-General. In 1874 the Gladstone Ministry went out, and Disraeli appointed Playfair Chairman of the Civil-Service Commission, which established the new system called the "Playfair Scheme." In 1875 he was influential in the selection of Hartington as leader of the Opposition, though he would have preferred Forster. In 1877 he first visited the United States, and the next year married a young lady of Boston. In 1880 he was tendered the post of first whip, but declined it, unwisely. He was appointed Chairman of the Committee of the Whole, as we should call it, and was obliged to bear the brunt of the *clôture* odium. In 1883 he resigned and was made Knight Commander of the Bath, or rather he supposed he had been, for he neglected the formality of receiving the accolade, so that he never was legally Sir Lyon Playfair. In 1885 he was elected to Parliament from South Leeds. In 1886 he was appointed Vice-President of the Council, practically Minister of Education in the House of Commons. In 1887 he headed a deputation who presented a memorial in favor of general arbitration to President Cleveland. In 1892 he was raised to the peerage, and made Lord-in-Waiting. In 1895 he received the honor of the Grand Cross of the Bath. He died in May, 1898.

It is delightful to read the biography of a man to whom life must have afforded a constant series of surprises to find himself so much cleverer than he had supposed. No wonder he was gay, sunny, sociable, entertaining, and affectionate. It is to be remarked that his peculiar good fortune never corrupted him in any way; for certainly a little healthy conceit is no fault, but a necessary quality. Without something like this, Playfair could not have passed through two periods of extreme unpopularity with such perfect equanimity as he did. The Life by Mr. Wemyss Reid brings out Playfair's character quite thoroughly, considering that it is one of those biographies which are prepared with the cooperation of intimate relatives of the subject. No little skill in the art of bookmaking has been put forth upon it. It is crammed with Playfair's amusing anecdotes, and is altogether a difficult book to drop. The index is thoroughly well executed.

70 (15 February 1900) 128: NOTES

CSP, identification: Haskell, *Index to* The Nation. See also: Burks, *Bibliography*; Fisch and Haskell, *Additions to Cohen's Bibliography*.

Dr. Philip Atkinson's 'Power Transmitted by Electricity' (Van Nostrand) has been fully revised; and obsolete matter has been cut out. There is nothing useless in it, and it well represents the present state of the art.

70 (15 February 1900) 128: NOTES

CSP, identification: Haskell, *Index to* The Nation. See also: Burks, *Bibliography*; Fisch and Haskell, *Additions to Cohen's Bibliography*.

Prof. William Ripper's 'Steam-Engine Theory and Practice' (Longmans), though too technical to be criticised here, is, nevertheless, so well put together and so concisely expressed as to deserve notice for its literary merit; and the matter of it is equally judicious and strong. So far as we have tested it, we have found it unusually accurate. But that students who mean to make it their business to understand the steam-engine, and who have gone so far as to know what differentials and integrals are, should shirk the labor of going on to learn enough mathematics thoroughly to master the theory of thermodynamics, and should find themselves sufficiently numerous to compel the adaptation of a work like this to their half-and-halfness--this, we must say, bespeaks some great fault in the methods of teaching. Perhaps it is ultimately traceable to the neglect of scientific logic, in consequence of which teachers of mathematics, not fully understanding its logic themselves, are unable to impart it to others, unless their pupils have a natural gift that makes them independent of teaching. But, given the conditions, we cannot see how Prof. Ripper could have done better than he has.

70 (15 February 1900) 128: NOTES

CSP, identification: Haskell, *Index to* The Nation. See also: Burks, *Bibliography*; Fisch and Haskell, *Additions to Cohen's Bibliography*.

A new volume of "The Specialists' Series" is 'An Introduction to the Study of Central-Station Electricity Supply,' by Albert Gay and C. H. Yeaman (Macmillan). It is to some extent a work of reference, but is much rather a book to be read, dealing broadly with all sorts of points which arise in the conduct of a central station, but which are distinct, from direct problems of electric lighting. There is no attempt at treating details exhaustively; but we feel sure that the discussions the work contains will be highly appreciated and found serviceable by those to whom they are addressed.

70 (15 February 1900) 128: NOTES

CSP, identification: Haskell, *Index to* The Nation. See also: Burks, *Bibliography*; Fisch and Haskell, *Additions to Cohen's Bibliography*.

Just one year after the first appearance of Dr. Frank Hall Thorp's 'Outlines of Industrial Chemistry' (Macmillan Co.) comes a new and revised edition. If the ditermination [[sic]] is to revise this publication yearly, it will undoubtedly gain vastly in importance. In the early years it will be requisite not only to keep up with the march of improvement, but also to remedy inevitable faults of the original preparation. We find, however, only two considerable changes in this issue: namely, on p. 446, a paragraph of eleven lines on carbonizing wool is inserted at the expense of a shorter paragraph on scouring wool, two others being more concisely expressed; and on p. 51 an account of the Herreshoff Pyrites Burner takes the place of descriptions of the Perret-Ollivier Furnace and the Hasenclever-Helbig Burner. The other changes that we have remarked do not amount to a score of slight corrections. The index is amended by the addition of a single entry, besides those which the above-mentioned insertions in the text required. The lists of authorities are almost absolutely unchanged. Not even new editions have been noticed. At this rate, the work, far from being improved, is barely maintaining its place in the line of march. Its external appearance is even handsomer, owing to the paper taking the ink better; and figure 26 has been redrawn to advantage.

70 (1 March 1900) 163: NOTES

CSP, identification: Haskell, *Index to* The Nation. See also: Burks, *Bibliography*; Fisch and Haskell, *Additions to Cohen's Bibliography*; MS 1426 (draft).

Prof. W. Watson of the Royal College of Science in London has produced 'A Text-Book of Physics' (Longmans), which, by means of full explanations of matters of difficulty, sets the principles of the subject in as clear a light as a one-volume treatise could well do. Here and there it becomes almost brilliant. We notice in it, too, sundry recent items that probably here make their first appearance in a textbook. Yet we cannot say that it is the ideal treatise we are awaiting from the hands of some man born for such sort of work. In his 896 pages of fine print, the author might have found room for less meagre tables of constants and for references to the classical memoirs. In places the distinction between words and facts is not sharply drawn. Occasionally we meet with such statements as that "Thales, who lived about the commencement of the Christian era, discovered that amber, when rubbed, acquires the property of attracting light bodies." Possibly some German higher critic may have suggested that the report of Diogenes Laërtius about that discovery may refer to some later, unknown Thales. But, if so, this late Thales was not the first discoverer of a fact mentioned as well known by Plato, in that passage of the "Timæus" which almost anticipates Le Sage's conjecture about attraction; and no critic has impugned the authenticity of the "Timæus" since Schelling abandoned his doubts about it. To say that Thales of Miletus lived about the commencement of the Christian era is like saying that Roger Bacon lectures on physics in the Royal College of Science with mediæval exactitude about ancient history.

70 (15 March 1900) 203-204: NOTES

CSP, identification: Haskell, *Index to* The Nation. See also: Burks, *Bibliography*; Fisch and Haskell, *Additions to Cohen's Bibliography*; MS L 159.138.

A second edition of Prof. Karl Pearson's 'Grammar of Science' (Macmillan) makes a much bulkier volume than the first; but the additions to it are inconsiderable. Prof. Pearson had contributed to the theory of evolution much that is of great value, in addition to highly important work in mathematical physics. But, notwithstanding that, the new edition of the present work, like the first, contains a good deal that is simply untenable. The author seeks to ally idealism and scientific philosophy--an attempt which is, in itself, altogether praiseworthy. But the work is not sound either on the one side or on the other.

70 (15 March 1900) 204: NOTES

CSP, identification: Haskell, *Index to* The Nation. See also: Burks, *Bibliography*; Fisch and Haskell, *Additions to Cohen's Bibliography*; MS L 159.138.

Heinrich Hertz, who revolutionized current conceptions of electrical action by developing the theory of Maxwell, and who was incontestably one of the most extraordinary intellects that have illumined nineteenth-century science, left behind him, at his early death, an unfinished work, of which a translation now appears under the title of 'The Principles of Mechanics Presented in a New Form' (Macmillan). The manuscript had been laboriously edited, at the desire of the lamented author, by Prof. P. Lenard, and the present translation, by D. E. Jones and J. T. Walley, has received the care of Prof. Lenard. The book is an attempt to elaborate with strict logic a conception of dynamics which excludes action at a distance, substituting for it concealed connections. Accordingly, we find energy defined as kinetic energy. At this rate our famous doctrine of energy is dissipated into thin air by being reduced to a mathematical truism. It is curious that the work is preceded by an introduction from the pen of Helmholtz, than whom, for all his ineluctable admiration of Hertz, nobody could naturally be less disposed to accept the doctrines of the present essay. Nothing in the notable volume better deserves to be pondered than this same introduction as a lesson in scientific calm and openness to every idea. A logician trained in modern conceptions will not have to read far in Hertz's Spinoza-like presentation to see that it abounds in logical crudities. But these are not inseparable from the doctrine; and perhaps continued health and vigor would have eliminated them before publication. It is certainly a book to be reckoned with and an historic monument. There is no complicated mathematics to be dreaded in it.

70 (22 March 1900) 230: The Teaching of Elementary Mathematics.

By David Eugene Smith. [Teachers' Professional Library.] Macmillan.

CSP, identification: Haskell, *Index* to The Nation. See also: Burks, *Bibliography*; Fisch and Haskell, *Additions to Cohen's Bibliography*.

David Eugene Smith was a prominent American mathematician and historian of mathematics. He received his education at Syracuse University, and then accepted a post as professor of mathematics at Michigan State College in 1891. In 1901, Smith received an appointment as professor of mathematics at Columbia University, where he remained until his retirement in 1926.

This attractive-looking volume makes pleasant reading, too, for it contains many a curiosity. A further merit is that it directs its reader to many books well worth his examination, although others of the greatest importance are overlooked. Most of the recommendations of the writer are well enough; but they are, on the whole, trivial. Vulgar arithmetic is, after reading and writing, unquestionably the most practically important subject taught in the schools. For the immense majority of scholars, it would conduce far more to their success in life to be good arithmeticians and bad spellers than good spellers and bad arithmeticians. The effort of the schools should, therefore, be largely concentrated upon making practical cipherers, at any rate, in the teaching of arithmetic. Yet, having looked over forty or fifty of the arithmetics in vogue, we are in a condition to say that pupils never are made really skilful at figures, and Mr. Smith betrays the fact that he, like the other pedagogists, is not himself a master of this low but necessary art. He discourses about trifling matters, and neglects most of those that are really weighty from a practical point of view.

About teaching geometry, too, he seems to us to be quite at sea. All that is of direct practical importance in the geometry usually taught might be put into a nutshell. But it is an indispensable preliminary to mathematical reading, and has always been acknowledged to be of great value as a mental discipline. If geometry were properly taught, it would train and strengthen a number of faculties: first, an important species of imagination; second, ratiocinative invention; third, logical precision of statement; and, most important by far of all, the power of generalization. But, to those ends, it is requisite that other branches should be taught than merely metrical, or ordinary, geometry--branches not essentially less elementary, but rather more so.

That Mr. Smith should have any notion of the educational treasures of topical geometry is more than we could expect. But we should think that a teacher from whose mouth the names of Desargues and Steiner and Von Staudt drop quite glibly (though we notice that the more available book of Cremona passes unmentioned) might recognize that even projective geometry is more fundamental and nearer the beginning of the matter than metrics, and that at least so much of it as is involved in perspective could advantageously be taught before attacking the somewhat artificial, and therefore confusing, logic of Euclid or Legendre. But even within the old, traditional limits we find nothing very useful in these chapters.

In regard to algebra, it is not easy to go quite wrong, owing to the perfection of the science. But there is here no symptom of a power of inculcating a real comprehension of the methods of analysis. As to the author's objection to applied problems of algebra, we must emphatically dissent. He quotes the dicta of some English mathematicians; but these are entirely misunderstood unless we are aware to how great an extreme this sort of thing has been carried in England, and that in reference to higher parts of mathematics. In moderation, it is now generally recognized by Continental mathematicians that the English practice is right. Besides, we are at present speaking only of the instruction of school-boys; and, in our opinion, considered simply as logical exercises, those practical problems that Mr. Smith contemns [[sic]] are most useful in training the power of disentangling a state of things.

The volume has an introduction by the editor in three pages, of which two are devoted to the logic of mathematics. We shall not imitate his brevity by undertaking to say here what we think of his views, because to do so might smack of superficiality.

70 (5 April 1900) 267: The World and the Individual: Gifford Lectures delivered before the University of Aberdeen.

First Series: The Four Historical Conceptions of Being.

By Josiah Royce. Macmillan. 1900. 8vo, pp. 588.

CSP, identification: Haskell, *Index to* The Nation. See also: Burks, *Bibliography*; Fisch and Haskell, *Additions to Cohen's Bibliography*; MSS L 159.130-131, L 159.143; MSS 1426a, 1461 (drafts).

We can do no more than explain in untechnical language what this important book is about. Its purpose is to say what it is that we aim at when we make any inquiry or investigation--not what our ulterior purpose may be, nor yet what our special effort is in any particular case, but what the direct and common aim of all search for knowledge is. This is a question of fact. Prof. Royce has clothed the matter in such academical guise that a reader untrained in philosophy might suppose it was a mere dispute about a definition, and therefore a profitless discussion; but, stripping off technicalities, we find this question of fact beneath them.

The only opinion on this subject generally held at this day that Prof. Royce considers to be essentially different from his own, is one which may be attributed to Bishop Berkeley more justly than to any other individual. It is the opinion of Possible Experience. Though this has taken slightly different shapes with different thinkers, it will suffice, in order to explain the purport of Prof. Royce's book, to state it in one of its forms. The answer, then, generally given, or virtually given, to the question what any inquiry is instituted for, is approximately that it is intended to settle doubt on the subject. Did Sir Philip Francis write the Junius letters? I can imagine, as the handwriting experts say, that he did. I can imagine, as most of the recent inquirers say, that he did not. I feel no compulsion to attach either idea to my mental representation of the historic world. There are some images which I am forced, whether I would or no, to attach to mental objects-such as a dark skin and jealousy to Othello. The course of life has developed certain compulsions of thought which we speak of collectively as Experience.

Moreover, the inquirer more or less vaguely identifies himself in sentiment with a Community of which he is a member, and which, includes, for example, besides his momentary self, his self of ten years hence; and he speaks of the resultant cognitive compulsions of the course of life of that community as Our Experience. He says "we" find that terrestrial bodies have a component acceleration towards the earth of 980 centimetres per second, though neither he nor many of his acquaintances have ever made the experiment.

Now, such being his state of mind, two hopes motive his inquiry: the first is, that the course of "our" experience may ultimately compel the attachment of a settled idea to the mental subject of the inquiry; and the second is, that the inquiry itself may compel him to think that he anticipates what that destined ultimate idea is to be.

Such, approximately, is the ordinary opinion of Possible Experience, in one of its modes of statement. According to it every inquiry is directed toward the resultant of certain compulsions; and, therefore, so far as a sense of compulsion is an immediate knowledge of something outside of self, exerting a brute force on self, this opinion is that every inquiry relates to a brute something without the mind. It was substantially on this ground that Kant opposed the anti-materialism of Berkeley. But, regarded from another side, this opinion is that the only object to which inquiry seeks to make our opinion conform is itself something of the nature of thought; namely, it is the predestined ultimate idea, which is independent of what you, I, or any number of men may persist, for however long, in thinking, yet which remains thought, after all. The whole course of life within which the experiential compulsions appear is a purely psychical development. For the gist of the opinion is that the flow of time consists in a continual assimilation into "our" inwardness, the Past, of a non-ego that is nothing but the ego that is to be--the Future. The Past acts upon the Future intelligibly, logically. But those blind compulsions are glimpses of an unknown object. Now, the unknown, according to this theory, is nothing but what is bound, as our hope is, to emerge in the future. Those blind compulsions, then, can be regarded as actions of the future on the past. From that point of view, it is seen that they can but be brute and blind, and, further, that in the course of time they must be seen to rationalize themselves and fall into place as the cognition develops.

To Prof. Royce's thinking, this opinion is unsatisfactory. He finds four faults with it, and sets them before us with his own argumentative lucidity and admirable mastery of the subject. Of the nature of three of them--that the opinion under examination makes the object of knowledge to be no more than a "would-be"; that its "experience" is no experience for an inquirer; that it seats an abstraction on a throne of reality--we can here find room for no clearer hint than those phrases may convey. Whatever solid skeleton the three objections may clothe is pretty much the same as that of the fourth and strongest, that if the non-ego to which the inquirer seeks to make his ideas conform is merely an idea in the future, that future idea must have for its object an idea future to it, and so on *ad infinitum*. There is no escaping the admission that the ultimate end of inquiry--the essential, not ulterior end--the mould to which we endeavor to shape our

opinions, cannot itself be of the nature of an opinion. Could it be realized, it would rather be like an insistent image, not referring to anything else, and in that sense concrete. Passing from the consideration of a single inquiry to that of the aggregate of all possible inquiries, the phantom ultimate issue of them all would be the real universe. To be that, however, it must include the mental world as well as the physical, and must set forth to itself all laws and modes of conception. It must, above all, exhibit to itself the whole course of time, with that process of complete rationalization of ideas upon the assumption of which the very hypothesis of a fated ultimate destination of opinion is based. It must, therefore, be conceived as a perfect rational consciousness. In short, it is such a conception of Deity (necessarily a one-sided one) as considerations limited to the Theory of Cognition could reasonably be expected to yield.

This inevitable outcome of the doctrine of Possible Experience is the very same goal, roughly speaking, to which Prof. Royce's explorations have brought him, too, by a path nearly parallel to that for which we have set up a sign-post for whoever may care to follow it out, though the hedgerows of thought may prevent the traveller over the one from being aware how close he is to the other. Prof. Royce reaches his conclusion by analyzing the nature of the purpose of an idea. Now this same conception of the purpose of an idea ought equally to be seized as the guiding thread to the doctrine of Possible Experience, although Prof. Royce believes his position to be quite foreign, even hostile, to that. One divergence is, that where another thinker might speak of a hope, as we have done above, Prof. Royce would substitute a *reductio ad absurdum* of the contrary opinion--a diminution of man's natural sublime attitude to a sorry "A is A." Fortunately the logic of those arguments is never impeccable, so that the hopes retain their matter and are not reduced to mere formulæ.

Two other views are examined. One is that of cognitive Dualism, which Professor Royce calls by the objectionable name Realism (as if the Dualists alone admitted outward realities). The other is that of Mysticism, which is less an opinion than an attitude of mind, of which Professor Royce gives an exceedingly penetrating analysis. There is a long and technical supplementary essay on the One, the Many, and the Infinite, which is very important.

The dress of the book is as charming as that of one so sure of being long and often perused ought to be.

70 (19 April 1900) 302-303: GROSSETESTE

Robert Grosseteste, Bishop of Lincoln.

By Francis Seymour Stevenson. Macmillan Co. 1899. 8vo, pp. 348.

CSP, identification: Haskell, *Index to* The Nation. See also: Burks, *Bibliography*; *List of Articles*; MSS L 159.126, L 159.145; MS 1427 (draft).

The beauty of a little volume, entitled 'Historic Personality,' that appeared in 1893 may bring it to the recollection of some of our readers. Its author was this

same Mr. Stevenson. It was a book to be confidently recommended to persons suffering from aneurism of the aorta, to whom the smallest excitement might prove instantly fatal. To our apprehension, and we actually read it, it was an examination of the efficiency of the different means which a modern reader can use in order to recover an idea of the personality--the characteristic mental flavor, if we may say so--of any historic figure. That the author devoted thought enough to that problem to write a book about it, even if it was not a remarkably vigorous production, before he actually undertook a biography, will certainly predispose his former readers to sit down to the present work with appetite; and from the Addisonian style of the earlier writing they will expect to find his pen now gliding along as smoothly as a canal-boat. In truth, it moves somewhat quicker now, without always stopping to choose a classical word. Expressions such as "Grosseteste was not popular with the King," the displacement by "state" everywhere of "assert," and the like, seem not to annoy the denizens of a humid isle whose sky is never very bright and where nerves grow rank. The pen is impelled by a purpose now, an animating one. The reader certainly cannot but become mightily impressed with the historic personality of this Robert of Lincoln, who, in the first half of the thirteenth century, when the conception of truth was but half-developed, had such a dominating sense of the reality of facts, especially of the deeper facts of human life, as to shake every man that came near him into earnestness and reverence. It is startling to come across in that age his two maxims, never to accept a sentence from an authority without taking account of its relation to the entire substance of the book from which it is quoted, and never definitively to accept any statement of natural fact without having tried the experiment to see whether it be really so. Very modern, too was his reply when accused of acting without precedent: "Every new thing that instructs and advances a man is a thing fraught with new blessing." His very style reflects his sense of truth, so weighty is it and so free from the exaggerations and flamboyancies of contemporary writing. Here is a fair specimen translated by Mr. Stevenson:

"I know that the perils of an exalted station are neither few nor inconsiderable. I know its pitfalls, how hard it is to repress pride, how rare is the sense of one's own weakness, how easy it is to feel contempt for others, how difficult to adapt oneself to the needs of the weaker brethern [[sic]]; it is the shadow of power, and the reality of servitude. I know also from experience, and still suffer from the knowledge, how many thorns there are in riches, how many occasions they afford for acting wrongly, how often they are misused, how true it is that they impoverish instead of enriching their possessors, and how those same possessors, who are really themselves owned by the wealth of which they are the reputed owners, find their intellects blinded and rendered torpid and dormant."

The see of Lincoln, to which he was soon after raised, was in those days one of the richest in Christendom.

In his early years, before the star of Albertus Magnus had culminated, he was acknowledged to be the greatest scholar and philosopher in Europe, pitiable as it is now to consider in what that pitch of learning consisted. He was a man of rare urbanity, refinement, and cultivation. All this must have disposed him to sympathize with the Benedictines and other monks, both as against the friars and as against the secular clergy. The people, too, loved the monks, and he was sprung from the lowliest of the people. But, to his eye, the moral well-being of the commonalty of England was an object so immensely greater than any learning could be, whether in theology or in other science, that he seemed to the monks to be systematically persecuting them, so insistent was he that their duties to the common people should be done. He was sedulous to invite educated men to his diocese; and perhaps half-a-dozen cases might even to-day be specified in which he made allowances to young men from his private purse to enable them to take the course in theology at Oxford preparatory to receiving livings under him. The large amounts of such allowances are notable, by the way. He wished them not only to live, but to live respectably. Yet when William of Cerda replied to his invitation that he was giving a course of lectures in theology at Paris, and therefore could not come, Grosseteste, after praising his zeal for teaching, reminded him that "Our Lord said to the chief of the apostles, 'If thou lovest Me, feed My sheep,' not 'If thou lovest Me, lecture from a professor's chair to the shepherds of My sheep.' The pastoral office is of more importance than the professional." Matthew Paris hated Grosseteste with a true monkish hatred, letting slip no opportunity to tell an anecdote to his disadvantage. Yet the chronicler's reverence for the Bishop, and manifest sense of his superiority to other men, are the best possible evidence of the greatness of his personality.

Another respect in which Grosseteste was superior to his age was in not believing in the sanctity of squalor. He was a hearty friend to the mendicant orders--with the Franciscans, quite bound up from the time when they landed just as his more active life was beginning at fifty years of age. He had welcomed the Dominicans four years earlier. Nothing angered him more than to see a friar cutting a dash in fine raiment and luxury. He was well acquainted with poverty, and knew its sweet uses. But he equally knew the utility of the comforts of life. When the Franciscans built a house in Oxford, he vainly urged them to consider sanitary conditions. With all his inflexible sternness where the well-being of the people was attacked, and though he had proved himself man enough to return almost to penury from wealth upon a point of conscience that he held to be doubtful, he wished his clergy to live well, and expended the reservoirs of his tremendous energy to bring that state of things to pass. "Three things," he said to a Black Friar, "are generally necessary to salvation temporal--food, sleep, and good humor." He once borrowed the Countess of Leicester's cook, and, concluding that a good table was more important to his position than to hers, asked leave to retain the man permanently, to which she responded, like the great lady she was, that were her servants good enough she would rejoice at any opportunity of placing them all at his disposal. Having once occasion to impose penance upon an ascetic, he directed him to drink a cup of Burgundy, and told him that if he would often undergo the same mortification he would have a better ordered conscience.

His courage never found its limit. An accomplished diplomatist, he ever preferred an arrangement to a quarrel, but not always to the postponement of a quarrel. There can be little doubt, we think, contrary to Mr. Stevenson's opinion, that upon one occasion he bargained with the Pope that he, as Bishop of Lincoln, should favor and aid in the collection of an almost intolerable tax upon the English church, in return for a decision in his favor of a process before the papal court. We think there can be little doubt of this, for the reason that it is as plain now as it was then to all parties, that the decision would infallibly be rendered in favor of the one who offered the higher bribe. But the truth was that, notwithstanding the burden upon his clergy, and notwithstanding his intense disgust with a Pope and court that could comprehend no motive but cupidity, that which he agreed to do as a part of a bargain he would have felt obliged to do in any case, since the very existence of the papacy was in imminent danger from the atheist emperor, Frederic II. A little later, however, he drew up a scathing memorial setting forth, without extenuation or palliation, all the injury to the Church and the papacy worked by the iniquities of Rome. He appeared before the Pope and his chief counsellors, and caused this plain exposure to be read to their faces, leaving also copies of it on file. He well knew, of course, that he thus made a deadly enemy of every man who heard him except the Pope himself, who, though he was as bad as the rest, could easily see that his own bread was not buttered on the side of corruption. Nor was this by any means an isolated instance of his courage.

We cannot here mention other interesting traits now first brought before the general reader in this biography. Nothing at all is said about Robert's personal appearance. The author is even in doubt whether he had a large head or not, saying that a signature "Master Robert Grosseteste" seems to imply that it was a family name. He allows the insignificant fact thus inferred (if it be a fact) to obscure from his mental view the significant circumstance that two writers who had seen him many times, and one at least who may have seen him, took it for granted that Grosseteste was a personal appellation. If they were in error as to how he came by the name, their assumption all the more proves that his head was large, since they would otherwise certainly have inquired why he had been so called. It is inconceivable, for instance, that his devoted disciple, Roger Bacon, should have continued through the long residue of his master's life after first meeting him, and subsequently to his death, to style him "Robertus dictus Grosseteste"-"Grosseteste" being, in truth, merely his family name--unless there was something in his personal appearance to confirm Bacon in the erroneous impression that it was a to-name. The only other thing we learn of his person is that his body was frail. If Mr. Stevenson's reader begins by vaguely figuring him as a sort of Napoleon, he will have a rough-hewn image that, after a good deal of subsequent shaping, as acquaintance ripens, will represent pretty well his "historic personality." He was a natural master of men and an administrator with both *coup d'œil* and capacity for details. He went to the heart of every practical problem, and almost invariably managed to have his way, as far as the nature of things allowed. In his anger he struck terror into all around him, while he charmed whomsoever he wished to charm. On the other hand, he was no actor, but most intensely sincere; he was on his guard against applause, outward and inward; he accurately foresaw the natural course of events; he knew perfectly what things are desirable; and he lived to a ripe old age, which one can scarcely conceive Napoleon as doing.

Grosseteste's public career began with his elevation to the episcopate in 1235, when he was over sixty years old, and continued till his death in 1253. For this period Mr. Stevenson's narrative was compiled without much difficulty from a few books. No material doubt can hang over any part of it, and nothing of importance can well have been omitted. It is, therefore, not open to criticism, except in minutiæ. The part of Grosseteste's life before 1235 is mainly that of a scholar. It could be adequately treated only by a person critically versed in the learning of that age, after a diligent study of all the works of Grosseteste. Such treatment it still awaits. Moreover, in the present state of our information, a large proportion of the facts of this division of Grosseteste's life, including all the principal dates, must remain conjectural. We cannot think that Mr. Stevenson has used great logical power in drawing his inferences from the few data we possess.

70 (10 May 1900) 366: Inorganic Evolution as Studied by Spectrum Analysis.

By Sir Norman Lockyer. Macmillan Co. 1900. 8vo, pp. 198.

CSP, identification: Haskell, *Index to* The Nation. See also: Burks, *Bibliography*; Fisch and Haskell, *Additions to Cohen's Bibliography*; MS L 159.145.

Some thirty years ago Sir Norman Lockyer discovered that the spectrum derived from incandescent metallic vapors enclosing a sufficiently hotter core of the same vapors differed by additional and enhanced lines from that of the same vapors without the core; whereupon he incontinently espoused the hypothesis that this was due to a dissociation--whether depolymerization or decomposition--of substances in our list of chemical elements; and he has been occupied ever since in defending this hypothesis, one might almost say with every means that God and Nature have put into his hands; at any rate with arguments, good, bad, and indifferent, snatched from every side. He has, of course, been assailed with objections of like promiscuous quality; but we must declare that such of his arguments as were drawn from his own observations were, in so far, fashioned of sterling metal, which is more than can be said of his antagonists, on the whole. If Lockyer's hypothesis should ultimately be disproved, posterity will rate him as a man with a fixed idea; while if, as is more likely, it is ultimately confirmed, he will be extolled as one of the most sagacious of prophets, a confidant of Nature, more than a generation in advance of his times. One cannot imagine Lockyer as otherwise than ardent, vivacious, and brimming with new ideas and new observations. If the physiologists could only expedite their promised prolongation of human life in time to save him for another thirty years' work, it would be an immense satisfaction to the scientific world and to him. We fear, however, that he intends this book to mark a slackening of his activities; and some rest he ought, certainly, to take, for this volume is ominously marked with signs of overwork. It reads as if it had been dictated to a typewriter, without calm preconsideration and without careful correction. Its faults of both kinds are so glaring that we shall simply dismiss them without further remark.

The original hypothesis of dissociation at length gave birth to another in Lockyer's mind, namely, that all the elements of our chemists are derived from one pristine matter, having an atomic weight some hundreds of times less than hydrogen, of which the recognized elements are polymers or compounds of polymers; and that the same matter exists everywhere throughout the stellar system in a few different grades of evolution--that is, of polymerization and combination of polymers--depending upon the temperature to which it is subjected. This is an acceptable working hypothesis, for it accords with our existing general conceptions of nature, and it is favored by a goodly squad of facts. This is Lockyer's Inorganic Evolution. That the relations among the chemical elements are to be explained by some sort of evolutionary process is the only idea we can at present entertain. We ought to begin, then, with trying how the hypothesis of the simplest kind of evolution that could answer the purpose will fit the facts, and adhere to that until it is refuted. Lockyer's seems to be that simplest hypothesis. At present, it is confirmed by but a few facts, over and above those required to suggest and give form to the theory. We cannot expect that it will stand unmodified by future discoveries; but how far or in what respects it will require alteration only time can show.

70 (17 May 1900) 384-385: **History of Ancient Philosophy**.

By W. Windelband. Authorized translation, from the second German edition, by Herbert Ernest Cushman. Charles Scribner's Sons. 1899. 8vo, pp. 393.

CSP, identification: MS L 159.125; MS 1428 (draft). See also: Burks, *Bibliography*.

A manual of the history of Greek philosophy in one volume, rather large than small, but not redoubtable in bulk, by giving as much upon this subject as nine out of ten intellectual persons care to read (unless it be Plato and parts of Aristotle, with dippings into later writers) may, in view of the extent to which it will be used, be a more important publication than if it were larger. At any rate, greater care is incumbent upon the reviewer, since many of its readers will be less able to judge of its merits unaided. Here, then, is a judicious work, on most points up to date, whose author does not plume himself so much on brilliant theories that set all the evidence at defiance, as upon giving a clear insight into the development of ancient philosophy according to the best established opinions of to-day. As for those *teretesmata* so universally found in German books, he is rather fond of transfixing them with the pins of good sense. This feature makes the book enjoyable, and worth consulting even by those who are *au fait* in the controversies. In short, if there is another compendious manual of ancient

philosophy in any language as illuminative and trustworthy as this, we have not the advantage of acquaintance with it.

The faults of Windelband's presentation are not trifling; but the worst of them are common to all works that are based on the modern critical treatment of ancient history--the method that has received so many hard knocks from archæology. In the first place, notwithstanding what has been just said of the *relative* merits of this history, it does not always escape being drawn into the common German fault of discarding all the premises in our possession in favor of what the critic is disposed to think likely. We note one place where this tendency is betrayed by a single word. Speaking of the simple propositions in geometry that the Greeks attributed to Thales, he says:

"It may be safely concluded in every instance that these elementary propositions were generally known to the Greeks of his time."

Concluded? A conclusion requires premises; but such premises are altogether in default. Windelband would more accurately have said *guessed*. There seems to be an ineradicable confusion in the minds of German philosophers and critics between what is concluded and what is guessed. In the present instance, our own guess, founded on psychological considerations (without which we would refuse even to guess), would be the opposite of Windelband's, and it would be backed up by positive tradition, which, without being at all conclusive, is certainly worth more than nothing. It is very true that upon most points in the history of Greek philosophy, more especially before Socrates (but often later, too), the testimony is open to so much suspicion that if we accept it we may be morally sure we shall often be led into error. But when a conclusion to which all the premises converge is, nevertheless, open to grave doubt, the situation cannot be mended by reversing that conclusion. Uncertainty is simply unavoidable in such cases; and we may as well make up our minds at the outset that the only way to escape being often deceived about the history of ancient philosophy is to abandon the study of it altogether. "This is not demonstrated," is the laughable phrase that is perpetually running from the tongues and pens of modern critics. Do they imagine, then, that any of their dicta about ancient philosophy *are* demonstrated? A large proportion of them are pet hobbies which nobody but their authors ever accepted; and perhaps half of the rest are things which it has become the fashion in the universities to assert without any ratiocinative process whatsoever--sheer guesses, like the above about Thales.

For the pre-Socratic philosophy, Aristotle's authority is so all-important that it is impossible to discuss it intelligently until his status has been settled, and the doubts that are rife concerning the authenticity of what some scholars are fond of calling "the so-called Aristotelian writings" have been duly weighed. Windelband does not half inform the reader upon what those doubts are grounded, while he shows that he is not free from the fault that we have been criticising by calling the positive affirmations of Strabo (partly confirmed by Athenæus) to the effect that Aristotle's original manuscripts lay *perdus* in a cellar for a century and a half--by calling these positive assertions "a very venturesome theory." But there is a

part of the story, so much as happens to suit him, that Windelband accepts. Again, he denies that the "Parmenides," "Sophistes," and "Politicus" were written by Plato, contrary to his usual respect for the testimony of Aristotle even regarding matters remote from his purview, though really one does not see why he should respect that testimony, if it consists of an aggregation of irresponsible glosses, as the theory is. As for the stylometric proofs, strong as iron, of the authenticity of the three dialogues, they smack so much of archæology as to put a "higher critic" quite out of sorts at their mere mention. While Windelband rejects the three dialogues as spurious, he is inclined to accept their testimony (if such it can be called) as to Parmenides and Socrates having had a discussion.

We have used so much space in setting forth this complaint against Windelband, that we can only just indicate several others. An effect, perhaps, of the German professors' habit of using a hundred words to disguise an idea that might have been precisely expressed in ten, is that Dr. Windelband, instead of endeavoring to carry the reader back to the naïve thoughts of the ancients, sometimes reports the ancients as expressing opinions about questions of modern philosophy. He also, like other historians, is given to reading into ancient philosophy a degree of consecutiveness and consistency which there is not only no satisfactory evidence for, but which is contrary to such evidence as we possess. Thus, he rejects the three dialogues of Plato just mentioned because they are contrary to the doctrine of ideas contained in the 'Republic,' etc., although Aristotle and others inform us that Plato changed his mind on that subject. Finally, we may justly complain that though this book has now been before the public for eleven years, some of its most singular positions have never yet been fully argued out by the author, anywhere.

The translation is authorized; but it need not therefore be correct. In many places correct it cannot be, if the author had any meaning at all. Here are a few random examples of what one finds in numbers on every page: "The Pythagoreans seem to be the first independently to discover the spherical shape of the earth" (p. 23). "There were men, otherwise favorably conditioned in life, who took a direct and immediate interest in knowledge" (p. 25). This "Otherwise" is not English. Read: "Men in good circumstances, too." "The fact that a cloud of myths should thicken from century to century around him, makes it necessary to go back to the oldest accounts" (p. 29). What should this "should" mean?

Of course, such phrases as "He was born as the son of Mnesarchus," and "Here was still a more motley mixture," patter upon us like rain. Inverted sentences, tempests of conjunctions and phrases having the force (or forcelessness) of conjunctions, *ifs* used in place of *although*, *ifs* within *ifs*, *all the mores*, *just thereins*, come upon us topsy-turvy in a way to make the perusal like trying to study while suffering from seasickness, such uncontrollable nausea does the unwonted tilting and pitching of the sentences produce. If the publishers had set their foot down about this matter, they might have done Dr. Cushman and his readers a signal service. We can only wish now that something may happen to the plates, because the book is one which is destined to be in use for a long time.

70 (31 May 1900) 417: NOTES

CSP, identification: Haskell, *Index to* The Nation. See also: Burks, *Bibliography*; Fisch and Haskell, *Additions to Cohen's Bibliography*; MS L 159.150.

Mr. S. R. Bottone's 'Wireless Telegraphy and Hertzian Waves' (Whittaker & Co.), otherwise not a reliable book, has the peculiar merit of describing, in every detail, how a beginner can construct for himself all the apparatus. A mechanical turn is one qualification for a modern electrician; but it will not go far without it be backed by an intellect that will not falter before the most complicated mathematical problem. Mr. Bottone, writing a little later than Kerr and Fahie, mentions a few inventions that those writers could not know.

70 (21 June 1900) 480-481: **Introduction to Ethics**.

By Frank Thilly. Charles Scribner's Sons. 1900. 8vo, pp. 346.

CSP, identification: Haskell, *Index to* The Nation. See also: Burks, *Bibliography*; *List of Articles*; MSS L 159.151, L 159.154, L 159.158; MS 1429 (draft).

Frank Thilly (1865-1934) was a prolific writer on subjects of philosophy. He was graduated A.B. from the University of Cinncinati in 1887 and later studied at the University of Heidelberg, were he was graduated A.M. and Ph.D. in 1891. Thilly taught at Cornell, the University of Missouri, and

Princeton University. His *Introduction to Ethics*, reviewed here by Peirce, became a popular work and was often used as a textbook.

The logical analysis of the conceptions connected with morals is one of the very best whetstones for the wits ever found; and it has never caused anybody to be burned at the stake! It is this of which Prof. Thilly has put together a convenient little handbook, in nearly alternate chapters historical and defensive of his own positions. In the latter parts, many things are well and forcibly put, yet we are not impressed that the volume will be treasured for their sakes. There is at least one long portion in which thought of no very forcible logical cohesion is administered in pretty dilute solution. In the historical parts, by separating the accounts of the controversies over separate questions, and by separating, under each question, the divergent lines of thoughts, without, however, mincing the matter very fine, the rationale of the sequence of opinions and the gradual penetration of thought further and further into the problems are brought out with force and clearness. This method of presenting the history of such a subject is amazingly superior to the chronological plan; but its full success would call for a very thoroughly considered taxonomy of the opinions. In this respect, the present volume is not quite what we could desire. Thus, the classification of the doctrines concerning the basis of right and wrong is substantially borrowed from Wundt (without acknowledgment, by the way). It is true that a tabular view on p. 128 shows some trifling departures from Wundt's scheme; but these have no perceptible effect upon the history.

Wundt's arrangement may be exhibited as follows:

Theories of the Basis of Morality.

A. The Moral Law is externally imposed.

B. The Moral Law is rational:

I. Its end is happiness:

1. that of the agent,

2. that of the community.

II. Its tendency is improvement:

1. of the agent,

2. of the community.

The most serious defect of this classification lies in its subdivision of rationalistic theory into only two branches, splitting upon the insignificant question of whether the end is completely attainable or not. This results in several inconveniences. It is very unjust to utilitarianism (one of the few theories of morals which have manifestly brought about any amelioration of society), by separating it only slightly from hedonism proper, or the doctrine that the lowest motive from which a rational being can act is at the same time the highest possible, and in short the only possible, motive. It overlooks entirely the very familiar view which makes the prolongation of the agent's conscious life the highest end. It confuses the morality which takes as its end the perfection of the individual man in a predetermined respect--say, by the substitution of altruistic for egoistic motives--with the morality which aims at the perfection of the individual in the sense of giving him whatever characters the future study of the question may show to be most desirable; and it falls into a like confusion in regard to theories which aim at the perfection of society. Moreover, it altogether fails to mark the worldwide difference between taking the perfection of society or of the individual as the *ultimate* end, and supposing a perfectionment to be brought about, so far as it is brought about at all, by natural selection, in which case the ultimate end is not perfection, but that toward which alone all natural selection works, to wit, the virtual fecundity of the race. Finally, it leaves out of account the possibility of so conceiving the ultimate end that it shall not be limited either to the individual or to human society. If we conceive that there is a methodical ideal--like order, or rationality--neither specifically psychical nor physical, which somehow has a power of developing itself in thoughts and things generally, then whatever furthers this progress is good, and *vice versa*; and such a conception refuses to be limited to any particular matter of realization.

Considering the imperfections of the classification with which Prof. Thilly has worked, it is much to his credit that he has, with little departure from accuracy, made the history appear clear and rational. We shall note a few small points to show that this book, like every other, has to be read critically. In the history of the theory of conscience, Hartley is placed after Bentham--a chronological displacement induced by the imperfection of the classification, and aggravated by the fact that the dates of publication are not commonly given, but only those of the different writers' birth and death. Some write their most characteristic works early, others later. Kant is placed among the perfectionists, contrary to his own energetic protests. He maintains that one must not act to bring about any definite result, but simply from the idea of duty. Herbert Spencer is refused a place among the evolutionistic moralists. He is, in truth, so vacillating that it is hard to say whether this is correct or not. It would have surprised Leslie Stephen to find himself in quite a different class from Spencer; and, whether this is right or not, neither he nor Darwin ought to be placed among perfectionists. True, they hold that conduct ought to realize an ideal, but not as its ultimate end. On the contrary, the ideal itself is, according to them, simply a result of natural selection, which acts solely to make some race or races dominant. Thus, the ultimate end for them is not inward but outward. Hume, in reference to his theory of conscience, is classed with Hutcheson. But he really followed Hartley, in the main; and where he disagrees with Hartley, he disagrees still more with Hutcheson. To Bernard Mandeville is attributed the proposition that greed and other selfish passions contribute more to the public good than benevolence does; and this proposition, being placed in quotation marks, will be understood to be the *ipsissima verba* of that author. This is approximately the opinion of some modern political economists of repute, but it was categorically repudiated by the author of the 'Fable of the Bees,' who was acute enough to see that it no more came within the scope of his inquiry, than it does into that of political economy, to determine what is and what is not for the public good. That which he undertook to prove was, that *if a nation desires expansion and splendor*, then it must have a rich and vicious class as the condition precedent to success in that career; but he added his private opinion that expansion and splendor do not really conduce to the happiness of a people, and therefore not to their "well being," if by that is meant their happiness. The last words of the fable are:

"They flew into a hollow tree,

Blest with content and honesty."

70 (28 June 1900) 502-503: Illustrations of Logic.

By Paul T. Lafleur. Boston: Ginn & Co. 1899. 8vo, pp. 97.

CSP, identification: Haskell, *Index to* The Nation. See also: Burks, *Bibliography*; Fisch and Haskell, *Additions to Cohen's Bibliography*; MSS L 159.141, L 159.163.

Prof. Lafleur has taken the trouble to ransack more than a hundred good writers and cull from them three hundred specimens of arguments, of which the greater part are valid or invalid according as they are free or not from any confusion between the ideas of *some* and *all*. That is to say, they are arguments that can be tested by Aristotelian syllogistics. The preface supplies unintentionally a three hundred and first example, for Mr. Lafleur there remarks that an instructor "finds difficulty in convincing his hearers that the logic of the class-room bears any relation to thought as met with in ordinary discussion and books"--implying that these illustrations ought to convince them that that logic supplies all that is requisite to judge of the validity of ordinary reasoning. Now, it is certainly true that most students, confusedly perceiving that any argument which requires close attention to apprehend its force depends upon something more than the *some* and *all* of traditional syllogistic, jump to the conclusion that there is no important element of right inference of which that doctrine takes account. This confuses *some* illative principles with *all*, and the conclusion is not true, since training in ordinary logic will almost insure a man against confusions of that kind. But, on the other hand, those who teach the old logic, finding that a great many important arguments can be thrown into syllogistic form, which really only proves that they involve a syllogistic element, jump to the conclusion that no important principle of reasoning has been overlooked, thereby falling into the very same fallacy that ensnares their pupils. The conclusion is not true, since, upon the examination of the syllogistic statement, it will often be found (not to say always, if the argument is at all difficult) that the whole gist of the reasoning has been thrown into the premises, so that the question of its validity is untouched by any criticism of the forms of the syllogisms. Hence, the need for a logic of relatives. The following three hundred and second "illustration" will show, if it be syllogistically treated, the truth of the above; ordinary syllogistic being incompetent to decide whether it is a sound argument or not:

If our duties on Cuban sugar were abolished, either our consumers would buy their sugar much cheaper, or else Cuban planters would get a far better price for their sugar. But the latter event would create a powerful stimulus to the production of sugar in Cuba, and since what now limits the production there is not so much a lack of suitable land or of anything of which the possible supply is already near exhaustion, but only the cost of machinery, etc., which can be had in almost any quantity at present prices, or lower, it follows that the production would be immensely increased if much better prices could be got. Then, since this country must remain the principal market for Cuban sugar, either much more sugar must be sold here, or else the supply from other quarters (which is not now nearly so great as that from Cuba) must be very greatly curtailed. But, on the one hand, our people already use extravagant amounts of sugar, almost as much as they would if it cost nothing. Hence, a very considerable reduction of price would be necessary in order to increase the consumption in any large proportion. Nor, on the other hand, could the production of cane-sugar elsewhere than in Cuba be greatly curtailed unless there were a motive for partially abandoning its production, in the shape of a considerable diminution of the profits of that production. In either case, therefore, the price of sugar to consumers in the United States would be very considerably reduced.

Let anybody who thinks that, even granting the facts alleged, the above is not a sound argument (as most of our readers will probably agree it is not), endeavor to detect the flaw in it by any ordinary syllogistic rules, or let anybody show it is sound reasoning by those rules (without throwing the gist of the argument into the premises), and in either event we will admit that something has been done to rehabilitate the logic of the schools.

Mr. Lafleur may remonstrate that he puts forth no argument in his preface, but merely states a fact. John Dryden might on the same ground protest against Mr. Lafleur's Illustration No. 1, which is Dryden's couplet--

"All human things are subject to decay,

And when fate summons, monarchs must obey."

But the compiler would rightly reply, "Mr. Dryden, you perhaps had no definite intention of arguing, but in fact you did argue essentially as in the stock example, 'All men are mortal; Sortes is a man; hence, Sortes is mortal.'" In like manner, Mr. Lafleur's statement of fact does convey to the reader's mind an argument, and, if insidiously, so much the more dangerously.

After all this tirade, we desire to say that Mr. Lafleur's little book will certainly be an enlivening and useful agent in the classroom. We wish that somebody would supplement it with a collection of real illustrations of relative reasonings, of striking problems in the doctrine of chances, of moot cases in inductive reasoning, and of examples in hypothetic investigations.

70 (28 June 1900) 504-505: A Short History of Free Thought, Ancient and Modern.

By John M. Robertson. London: Swan Sonnenschein & Co.; New York: Macmillan Co. 1899. 8vo, pp. 477.

CSP, identification: MS L 159.141. See also: Fisch, *First Supplement*.

John Mackinnon Robertson (1856-1933) was a British writer and politician. Serving as a member of Parliament, liberal party, Tyneside division, from 1906 until 1918, Robertson was widely known also as a Shakespearean scholar, literary critic, and social scientist.

Free thought about religion has, far more than science or philosophy, been broken up into a hundred separate movements; and this circumstance compels, in any single-volume review of them, so succinct a treatment of each movement that, though this volume is not small and a good deal of it is in fine print, the history is rightly named a short one. Had sufficient references been given to other works to make this a guide to the literature of the special topics, it would have been a valuable manual; but, that not having been done, it remains a short history and nothing more.

Some general theories regarding the course of free thought that the author seeks constantly to illustrate, serve to connect the different morsels and to maintain the reader's interest. Mr. Robertson's own rationalism is extreme. It does not seem to have struck him as possible that thinking for one's self may bring a man to entertain a higher respect, in the matter of religion, for those elements of our nature that bind men together than for the speculations of his individual reason. Nor does he seem to recognize any conceivable outcome from rationalistic speculation but unbelief. Nor does he remark, what everybody perceives, that prevalent unbelief in any age is quite as gregarious and wanting in independence as prevalent belief, the vanity with which it is tinctured being rather a levitating quality than adding to its weight. He has not a word to say in favor of religion, but deems its influences irredeemably bad from first to last. It always tends, he finds, to grow more and more corrupt and corrupting. Christianity he seems to rank pretty low in the scale of religions, and thinks it could have gained predominance

only after paganism had become very grossly superstitious, as it did after the establishment of the Empire. Christianity was itself a relative free thought. Free thought can, says Mr. Robertson, arise only out of the conflict of faiths. Still, skepticism is found everywhere, even among primitive and savage peoples. Persecution of it begins only when the material interests of the priests appear to be in jeopardy. So, religious wars cannot break out without political causes. Mr. Robertson holds to the unity of human nature in all ages and among all races, and says that pretending to explain an historic phenomenon by "national characteristics" is like explaining the action of opium by a soporific virtue--a remark which his readers may think becomes just or otherwise in proportion to the failure of the explanation to colligate different phenomena.

The modern part of the history, though drawn up with some ability, is not detailed enough to bring out its full interest. French and Dutch free thought, from Descartes to Robespierre, fills only thirty pages, and English deism another thirty. The ancient part is much more entertaining because of the author's theories.

71 (5 July 1900) 14: NOTES

CSP, identification: Haskell, *Index to* The Nation. See also: Burks, *Bibliography*; Fisch and Haskell, *Additions to Cohen's Bibliography*; MSS L 159.152-153.

--Perhaps no small fact is more pregnant with the character of our anæsthetic age than the decline in the popularity of the celebrated treatise of Boethius. It is not to philosophy that people who fancy themselves terribly stricken nowadays go for consolation. They more often, whether rich or poor, betake themselves to travel. They cannot any longer understand how such indisputable woe as that of the author of the 'De Consolatione Philosophiæ' could have the heart for such an artificial style of composition. The reason is, that the Gorgon terrors of adversity are no longer remembered. They are among those picturesque features of the good old times which the prosaic nineteenth century has expunged, except from seats of war. Nevertheless, we have to record the appearance of the third English edition of the 'De Consolatione' within as many years, and that without counting Middle-English or Anglo-Saxon recensions. Dr. Walter John Sedgefield, editor of King Alfred's version for the Clarendon Press, has now brought out, through the same press (New York: Henry Frowde), a translation into modern English. Nobody would seek acquaintance with the work itself in this volume. On the contrary, its interest lies precisely in its great departures from the original--the ruthless excision of many of its fine passages, which marks King Alfred as a regular blue-pencil editor, and the still more remarkable and numerous insertions, which certainly throw a great deal of curious light upon the character of the English hero. Dr. Sedgefield has provided the volume with an entertaining introduction of over forty pages, which, after dealing with Boethius and with Alfred, branches off upon the subject of the previous English versions of the book. It gives specimens of most of them which are decidedly curious, and are doubtless intended to be reprints literatim,

which unfortunately they are not--quite. There are brief but sufficient notes, and an alphabetical index. The volume is even prettier than James's edition of 1897. Neither is equal to Lord Preston's of 1695, which, if one likes the grandiloquence of that generation, is tho [[sic]] most agreeable English presentation of a book that affords food for various reflections.

71 (19 July 1900) 59: **Theory of Differential Equations**.

By Andrew Russell Forsyth. Cambridge: At the University Press; New York: Macmillan. 8vo. Vol. I., 1890, pp. 340; Vol. II., 1900, pp. 344; Vol. III., 1900, pp. 391.

CSP, identification: Haskell, *Index to* The Nation. See also: Burks, *Bibliography*; Fisch and Haskell, *Additions to Cohen's Bibliography*; MS L 159.145; MS 1430 (draft).

The previous 'Treatise on Differential Equations' (Macmillan, 1885) of Prof. Forsyth, now Cayley's successor in Cambridge, presented the subject in an elementary way, suitable for a beginner. The present work is addressed to such as have at least a thorough elementary knowledge of the subject, and invites the attention of all who have penetrated more deeply into it. It does not, perhaps, display as much high talent as Emile Picard's treatise (which, of course, is of a different nature); but the thoroughness with which the substance of the memoirs has been worked over, quietly improved in many places, and set before the reader in a compact and easily read form, can be known only to those who will put themselves to the trouble of making the comparisons. The reader is everywhere referred to the greater memoirs, as well as to any other presentations of the several branches of the subject that may have been found specially luminous by Prof. Forsyth. Lesser papers, even in cases where they add something material, are, for the most part, passed by. For example, at the end of chapter ii. of volume ii., which is devoted to Cauchy's existence-theorem, there are fifteen references to thirteen different memoirs and treatises. Among these, of course, figures the classical work of Madame Kovalevsky; but the connected investigations of Delassus are not mentioned, nor does his name appear in the index. It is true that these, like Madame Kovalevsky's, relate mainly to partial differential equations, and are thus, in a formal view, foreign to the subject in hand. But then, would not the whole question of the existence of integrals have been advantageously set forth, at least in its outlines, connectedly?

This brings us to remark that the arrangement of Prof. Forsyth's matter seems to have been determined by the circumstance that such and such a part was ready for publication. Certainly, it was most desirable that as soon as any division of the work was ready it should be set before the mathematical world at once; nor do we intend to imply that the selection of parts to be prepared was a random one. Volume i. treats of total equations and mainly of Pfaff's problem; volumes ii. and iii., of ordinary equations not linear. Now, on the principle of treating the general before the special, it was certainly desirable to place Pfaffians thus early; and with them a great part of the subject of partial differential equations. But a considerable part of the latter subject remains, and unless Prof. Forsyth returns to it, will remain,

unconsidered. There is also room for doubt as to whether it was expedient to postpone ordinary linear equations to equations not linear. However, no arrangement could have been adopted that would clean up the whole matter to be disposed of at one systematic sweep. There must inevitably be considerable odds and ends requiring a subsequent gleaning. The author proposes to treat ordinary linear equations in "an additional volume." A complete collection of all that has been contributed to that subject would make a library exceeding the entire impedimenta of many an able mathematician; and as to its being compressed into one volume, a doubt may be permitted. So, considering how much else remains over, we may hope that two, if not three, volumes are yet to be added to this admirable and beneficent work.

This is not the place to consider it more in detail; but seeing that the doctrine of differential equations is one of the most practical of the branches of mathematics, which every young man who aspires to apply exact principles to the affairs of life more than his predecessors have done, has to know at least as well as he knows how to spell, we will point out to beginners the advantage there now is in seeking their first introduction to this discipline at the hands of Forsyth's 'Treatise,' since his 'Theory' is now at hand to supplement it in directions in which they may desire to push their studies further. A better practical mastery of differential equations can be attained by beginning with Forsyth's 'Treatise,' followed by his 'Theory,' than by the aid of Jordan, Königsberger, or any other extant guide. It is a lucid and delightful work, and has the advantage of offering a careful selection of those exercises for the student which are much more needed here than in other equally advanced branches of mathematics, for the reason that there is no perfect calculus enabling one to tilt at a differential equation like a knight in armor. Strategy and experience are nowhere more demanded.

71 (26 July 1900) 78-79: A History of Modern Philosophy:

A Sketch of the History of Philosophy from the Close of the Renaissance to Our Own Day.

By Harald Höffding. Translated from the German edition by B. E. Meyer. Macmillan Co. 1900. 2 vols. 8vo, pp. 532, 600.

CSP, identification: Haskell, *Index to* The Nation. See also: Burks, *Bibliography*; Fisch and Haskell, *Additions to Cohen's Bibliography*; MS L 159.145.

At the nineteenth century's midday, a doubt used to be entertained whether human powers were adequate to writing the complete history of a great branch of science entirely from primary sources. If the requirement is that the whole matter shall be thoroughly well digested, the feat remains, probably, unaccomplished yet. The larger histories of Modern Philosophy have hitherto belonged to one or other of two varieties. There have been essays, long and heavy, on the leading philosophical tendencies of the successive generations, mentioning none but the supreme productions. These have been too vague and too much colored by their authors' predilections to repay more than a skimming. There have, besides, been set chronicles, one painfully like another, each containing a quasi-bibliography of the subject, with more or less penetrative criticisms of a few writings, adopting current views of the rest, and enumerating a throng of publications with which the compiler's acquaintance seems limited to their names and some guess at their affiliations. These are extremely useful works, but no more entitled to be called histories than a gazetteer to be called a geography. Höffding is the first to furnish Modern Philosophy with a history, in the high sense of the word, an intelligible and interesting chart of the course of the main stream of thought, based upon actual soundings of his own, upon studies minute, critical, and mature. Nor have these been jotted down as in a notebook, but generalized with unusually good literary judgment. If he calls his work a sketch, it must be because only the great questions are touched upon, and because minor writings that he has not himself carefully read and considered are left unmentioned. For in other respects it is no sketch at all, but an elaborately finished work of literature.

The very table of contents promises freshness, especially in dwelling most upon neglected men and movements. The "philosophy of romanticism," as the author rather too wittily (and a little anachronistically) calls German post-Kantian idealism and its accompaniments, is confined to a seventh of the whole space, instead of its regular quarter. Two hundred of the eleven hundred pages go to Positivism, J. S. Mill and Darwin by themselves getting ninety, against the 6, 8, 10, 26, which are the respective proportional allotments of the two combined in four reputable histories of modern philosophy that happen to lie at our hand. A still greater novelty, and a most welcome one, is the allowance of 160 pages to the philosophy of the Renaissance. This is treated under two heads, "The Discovery of Man" and "The New Conception of the World." Thomæus and Cæsalpinus being passed by without mention, which can only be because Höffding has not read them, we come to Pomponazzi, of whose half-dozen books interesting to the historian of philosophy only that one of which there is a modern reprint is noticed. We discover how broad is the author's notion of a history of philosophy when we find eight of this large pages consigned to Macchiavelli, whose 'Discorsi' as well as the 'Principe' have been studied. Althusius is another figure not before introduced into the history of philosophy. "The Discovery of Man" ends with a chapter on Jakob Boehme precisely equal in length to the account of Macchiavelli. Under "The New Conception of the World" we meet only familiar names; but the treatment continues to be original. Of the forty pages occupied with Giordano Bruno not one would be willingly spared by the reader.

It was an excellent idea, after the Renaissance, to appropriate a space equal to that which Bruno fills to Lionardo, Keppler, Galileo, and Bacon. Copernicus had been already treated. For the scientific men, however, the execution is not at the height of the conception. It is no light task, even for an astronomer, in these days, to read Keppler's great book 'De Motu Stellæ Martis' so as fully to appreciate the motive and import of each step of the investigation. Yet no work of special science can be more significant for philosophy, in that no other describes with anything like the same fulness all the steps of a difficult hypothetic reasoning; and, perhaps, man never achieved another reasoning of any kind so elaborate and so triumphant, while its historical influence was of commensurate importance. It cannot be concealed

that Höffding has no idea of what the work really was. The chapter on Galileo is not quite so inadequate. The author has evidently run through

Galileo's correspondence, and has not contented himself with the famous dialogues. Still he does not begin to do Galileo justice. When one knows next to nothing of the matter except what the celebrated *Giornata Terza* of the 'Discorsi intorno a Due Nuove Scienze' discloses, the founding of the science of dynamics seems such an easy matter! The *ars celare artem* was never carried further. It is requisite to have other reading in order to place one's self where Galileo set out, in that state of mind in which it was considered manifest that when we throw a ball, that which causes it to move on after it leaves the hand can only be the rush of air behind it. It is requisite to have some experience in physical experimentation to appreciate the fineness of those observations of Galileo, made with almost no apparatus, by which he refuted that false notion. It is requisite to be one's self something of an investigator to realize how far he yet stood from his final clear understanding of the matter, even after he had refuted that error, though the little-read 'Sermones de motu gravium' come in to help us here. The whole investigatory procedure of Keppler is laid bare to whoever chooses to peruse it, while we can gather what that of Galileo was only from slight indications. Those are, however, quite sufficient to show that quantitative experimentation played a much greater part in it than the reader of the *Giornata Terza* would gather that it did, Höffding might as well have read nothing else, as far as his conception of Galileo is concerned.

We can notice in this interesting and original work only a few points here and there selected to illustrate its characteristics. Equal space is allotted to Descartes and to Hobbes. Spinoza gets more, as much as Bruno; and Spinoza is pronounced to be as *the* thinker of the seventeenth century--high praise considering that the author rates seventeenth-century philosophy as far more accurate and valuable than that of the nineteenth. Newton, with his doctrine that space is a peculiar entity, receives some notice, which is more than he does in most histories of philosophy. But Professor Höffding evidently has no suspicion that Newton's proposition is less purely gratuitious than the opposite notion that position and motion are entirely relative. Hartley is disposed of in two pages. The other father of the association of ideas is mentioned only as "a little-known author called Gay." Rousseau is treated with some respect, and at greater length than either Locke, Berkeley, or Hume. Kant has evidently been studied with the utmost thoroughness; and a good many small points are made which appear to be new. Thus, the suggestion that the 'Kritik der reinen Vernunft' must have been largely a patchwork of detached papers strikes one as happy. Again, in regard to his metaphysical dualism, several passages are cited whose bearing upon this question might easily escape the most attentive student. Yet, after all, Höffding apparently fails to see that the decisive consideration in Kant's mind was no other than that for which, in his second edition, he invented the most prominent situation he could contrive. But it is curious how insensible some men are to this argument, although to others it seems a knock-out. The chapters on the German philosophy of the Hegelian period are the only ones where a little skipping can ever enter the reader's thoughts. But interest is thoroughly roused again when the more recent Germans are reached, especially in the dozen pages

about Dühring, whose 'Natürliche Dialektik' is treated with admiration, and much preferred to his later doctrine. Nothing subsequent to 1880 enters into the *cadre* of the work; and much that is previous to that date is too close to us to be accurately sized up. There is not a word about Renouvier, whose principal work was published in the fifties.

The second-hand translation, though authorized, is regrettable. It has been executed by one of those types of German humility who undertake to improve upon our language and rules of style before they know English well enough to avoid effects, now comical, now ungainly, now indecent, now enigmatic, now self-stultifying, now merely silly. He improves a little, however, as the work proceeds; and, besides, the reader becomes inured to his lingo. A book that in another dress would have been a literary treat has, we are bound to warn the reader, thus been rendered somewhat unpleasant reading. The volumes, though large, are comfortable to hold and read. The print is good, the paper particularly so. Each volume is provided with a sufficient index.

71 (26 July 1900) 79: **The Kinetic Theory of Gases: Elementary Treatise with Mathematical Appendices**.

By Dr. Oskar Emil Meyer. Translated from the second revised edition by Robert E. Baynes. Longmans, Green & Co. 1899. 8vo, pp. 472.

CSP, identification: Haskell, *Index to* The Nation. See also: Burks *Bibliography*; Fisch and Haskell, *Additions to Cohen's Bibliography*; MS L 159.141; MS 1413 (draft).

The kinetical theory of gases is without doubt the great triumph of that corpuscular philosophy which endeavors to explain the universe by turning its splendid tapestry wrong side out, and showing that it is all nothing but bits of matter moving under their attractions and repulsions, according to the three laws of motion. As an ultimate explanation, even of that which it contemplates explaining, the corpuscular doctrine was doomed from the moment when the seed of evolutionism was dropped into the intellectual soil, since it provides no possible way in which the state of things it supposes, the existence of the atoms and their attractions, could have come about--not to speak of its leaving inexplicable the laws of motion, with space and time, the semi-rationality of which things calls loudly for further elucidation. But the spirit of science (which much better deserves the name of philosophy, or pursuit of knowledge, than any cock-sure metaphysical system does) is to adopt provisionally the simplest promising hypothesis--which, in being simple, is necessarily thereby extreme, radical, and skeptical--to follow it out rigidly to its last consequences, and, by carefully comparing these with the phenomena, ascertain what amendments of the hypothesis may be requisite. In the case of the kinetical theory of gases, no positive disagreements of the phenomena with the hypothesis have ever yet been met with; but, on the contrary, each new deduction that the restless activity of the mathematicians brings to light is still found to fit into its place among the facts, as each piece of a boy's dissected map finds its place as

soon as he has once begun to fit the pieces rightly together. There is in all science no other theory so interesting in these three respects at once: first, for the logic of the inductive argument, as it has been historically followed out; second, for the subtle deductive reasoning by which the consequences of the hypothesis are, one by one, getting discovered; and third, for the insight it affords into the ways in which different forms of phenomena may arise in nature.

The work of Oskar Emil Meyer has had a great charm for the generation of physicists now beginning to grow old. They were not, for the most part, such accomplished mathematicians as are those who are now taking the stage; and Meyer's plan of dividing his book into two parts, of which the larger called for no more mathematical processes than anybody may perform in his head, while the other set forth deductions in the most elementary way that the subject permitted, suited those men very well. But this is not all. The first division shows how the consequences of the theory compare with observation; and to see one phenomenon after another falling into the ranks of the theory's defenders, as Meyer skilfully, yet simply, shows they do, is mighty pretty and delightful. This division of the work is, from the nature of the case, very nearly a history of the whole course of the investigation; and we do not know what more instructive basis for a course of lectures on the logic of explanatory science could be found than the history of the theory of gases. Whoever cares to see how explanatory research proceeds cannot fail to be interested in this handsome volume. As for the mathematical division, that, too, affords excellent exercises in reasoning, though of a different kind of reasoning, and exercises of quite an opposite character, since this division is infested with subtile fallacies.

To give a single plain example, the author professes to demonstrate that the mere assumption that the molecules of a gas move in broken straight lines suffices by itself to account for Bayle's law that at constant temperature the volume of a gas is inversely proportional to the pressure. Anybody can see that if the molecules are incompressible bullets, this is not so, whether their motion is rectilinear or not, since in that case there is a minimum volume which no pressure, however great, can diminish. Meyer's wrong conclusion arises from his assuming, in his mathematical work, that any infinitesimal volume within the gas is filled with matter in all respects like, and moving like, that in any other such volume; in flagrant conflict with the theory that the gas consists of separate molecules. In the revised edition such fallacies are not so easily found as in the first, but there remains abundance of game for the logical sportsman.

We may remark, by the way, that this translation is much to be preferred to the original, since the additions have been incorporated with the text, and the translator's notes, though infrequent and short, are always pertinent and useful. The English bears some disagreeable traces of being a rendering from German, but they are not very offensive. The non-mathematical division has been brought nearly to date with all the old ability of its author. There are a great number of useful references to memoirs, and, considering how rich the book is, we are inclined to regret that it should not have been made a repertory of all that is really needed to compare the theory with the facts that have been brought to light. But nobody at all acquainted with the subject can fail to remark that much work has either been passed by unmentioned, or too slightly noticed, both on the side of theory and on that of observation. The author's motive evidently was not to fatigue the reader, or choke with dry details the life and movement by which the work is eminently distinguished. There are books as our readers know to their sorrow, that are neither popular nor scientific, but fall between the two: this one is at once popular, or, at least, very readable, and highly scientific, at the same time; for, though called elementary, it leaves no important feature of the theory untouched, excepting where the theory crosses the border into the domain of liquids.

Among the special points which will be found important for those who are up in the subject, we may mention further argument concerning the author's views of diffusion, a discussion of the effect of dissociation upon viscosity, a theory of the resistance of the air and of the reaction of a jet, and a synthesis of all the evidence concerning the size of molecules.

71 (16 August 1900) 131: THILLY AND WUNDT

TO THE EDITOR OF THE NATION:

SIR: In a recent number of your journal the reviewer of my 'Introduction to Ethics' states that my classification of ethical systems, as presented in the table on page 128, is substantially borrowed from Wundt. In order that the reader may judge for himself, will you kindly permit me to give Wundt's scheme, and then append my own? I quote from the English translation of Wundt's 'Ethics' (vol. ii., p. 164):

"We thus obtain the following classification: I. *Authoritative Ethical Systems*.

These may be divided into *politically* and *religiously* heteronomous systems. They either avoid taking any account of ends, or affiliate with some one of the autonomous systems as regards the question of ends. II. *Autonomous Ethical Systems*. (1.) *Eudæmonism*, under the form of (a) Individual Eudæmonism or Egoism; (b) Universal Eudæmonism or Utilitarianism. (2.) *Evolutionism*, under the form of (a) Individual Evolutionism; (b) Universal Evolutionism."

Here is my scheme: "What makes an act right or wrong? The *Theological School* says: The will of God; The *Common Sense School*: Conscience; The *Teleological School*: The effect of the act. What is the effect? Pleasure, says *Hedonism*; Perfection, says *Energism*. Whose Pleasure? Pleasure of self, says *Egoistic Hedonism*; Pleasure of others, says *Altruistic Hedonism*. Whose Perfection? Perfection of self, says *Egoistic Energism*; Perfection of others, says *Altruistic Energism*. The *Theologico-Teleological School* says: An act is good because God wills it, and God wills it because of its effects."

I also request the reader to compare the classifications given in the following works with Wundt's and my own: Bain's 'Mental and Moral Science,' 1868; Lecky's 'History of European Morals,' 1869; Sidgwick's 'Methods of Ethics,' 1874: Jhering's 'Der Zweck im Recht,' 1877; Paulsen's 'System der Ethik,' 1889; Seth's 'A Study of Ethical Principles,' 1894; Hyslop's 'Elements of Ethics,' 1895;

Dorner's 'Das menschliche Handeln,' 1895; Külpe's 'Einleitung in die Philosophie,' 1895; Lipps's 'Ethische Grundfragen,' 1899. A perusal of these books will show that there is little difference in the classifications of the different authors, and that modern Ethics has reached a certain degree of fixity with respect to its divisions. My own scheme resembles the schemes of all of these writers in some points, and differs from them in others. So does Wundt's. Mine is no more like Wundt's than Wundt's is like Bain's and Sidgwick's and Jhering's. It is to be noted that not one of the writers mentioned in the above list gives credit to anybody. And there is no reason why he should; no more than why a modern biologist should give credit for using the current zoological classifications.

Yours respectfully, FRANK THILLY.

COLUMBIA, MO., August 9, 1900.

71 (30 August 1900) 178: **The Theory of Electrolytic Dissociation, and Some of its Applications**.

By Harry C. Jones. Macmillan. 1900. 8vo, pp. 289.

CSP, identification: Haskell, *Index to* The Nation. See also: Burks, *Bibliography*; Fisch and Haskell, *Additions to Cohen's Bibliography*; MS L 159.141.

The theory of electrolytic dissociation was originally proposed by the penetration of Clausius as far back as 1857, not as an hypothesis, but as a deduction from acknowledged facts. Clausius reasoned that since the smallest electromotive force suffices to decompose an electrolyte into its ions, without violation of Ohm's law, it followed that the current did not do the work of decomposing the molecules of the electrolyte into its ions. Whence it further followed that these molecules must be already decomposed; in other words, that in a solution of common salt, a part, small or large, of the molecules of chloride of sodium must be decomposed and be present as isolated atoms (not ordinary molecules) of sodium and of chlorine. But this was such a startling idea--it seemed so incredible that an innocent solution of common salt should contain such powerful reagents as nascent sodium and chlorine--that the theory found no acceptance, especially as the proposition did not seem to lead to any further correlation of facts.

In 1877 the botanist Pfeffer published a book (translated in Harper's Science Series) in which he gave the results of numerous experiments by him upon a phenomenon first discovered long before by Traube, that of osmotic pressure. Namely, when a membrane forms a partition between a solution and a quantity of the pure solvent, if it happens that the solvent can pass through the membrane, while the dissolved substance cannot, then the pure solvent will flow into the solution, until a certain difference of pressure has been established, called the osmotic pressure. Now Pfeffer's experiments showed that for dilute solutions of any given substance the osmotic pressure was proportional to the quantity of that solvent in solution--a fact which, if it attracted his attention at all, probably appeared natural enough to him, and to which he attached no particular significance. But one day, as the physical

chemist Van't Hoff was going home from his laboratory, he met a colleague who had been repeating some of Pfeffer's experiments, and who mentioned to him the proportionality of the osmotic pressure to the concentration of the solution. To Van't Hoff's trained mind, this meant no less than that the dissolved substance had a pressure proportional to its density, or, in other words, was in a quasi-gaseous condition.

To many a man familiar with the kinetical theory of gases, this analogy would have seemed strained, because he would have regarded it as the essential characteristic of gases, to which the law of Bayle is due, that the molecules have rectilinear paths. The exacter conception of Van't Hoff was that that law is the consequence, not of the rectilinear paths, but of the fact that the molecules are so far separated from one another that their mutual attractions and repulsions have no considerable effect. But now, according to the law of Avogadro, the pressure of a given gas must be the same as that of the same number of molecules of hydrogen in the same volume and at the same temperature. Was the osmotic pressure, then, of the right amount? After a preliminary inquiry into the applicability of the law of Charles, or Gay-Lussac, Van't Hoff succeeded in showing that this was precisely true for many substances in solution, while for many others it was not at all true, the osmotic pressure in these cases being always too great. At this point Arrhenius, whose pupil Van't Hoff had been, and who had privately been informed of his results before their publication, remarked that all the exceptions were electrolytes, and that excessive pressures in those cases would be required by the kinetical theory of gases, if the deduction of Clausius were admitted. That was in 1887, and from that moment the theory of ionic dissociation began to fulfil the function of correlating facts.

In the volume before us, Mr. Harry Jones, known by his contributions to this branch of chemistry, sets forth the evidences of the theory, and shows that it must be accepted as positively proved. Of course, we do not know in what state the dissociated atoms may be. Possibly, for example, they may be combined with atoms of the ether, which may be of different chemical kinds. But it does seem that chemical physics is now upon a path which may probably bring us out to a clearer view of the nature of atoms and of molecules.

Mr. Jones's book is, in some respects, not unskilfully put together. His argument is clear, consecutive, and convincing. We think his readers will generally regret that he has not placed before them more extended synopses of the facts, in lieu of a few at each point that have been selected as being favorable to the theory. It may be doubted, too, whether the representation of the doctrine of electrolytic dissociation as the pivot upon which all the physical chemistry of the day turns, is quite accurate in its perspective. The non-electrolytes are of a good deal of importance.

About a third of the volume is occupied with applications of the theory to physics, chemistry, and physiology. The most interesting of these have been before the English reader for some years--as has, indeed, the whole subject, though it has not before been so well set forth.

71 (6 September 1900) 192-193: NOTES

CSP, identification: MS L 159.157. See also: Fisch, *First Supplement*.

--Prof. John Perry, in *Nature* of August 2, has opened a discussion which ought to prove important. His 'Calculus for Engineers' was a pretty radical departure in mathematical teaching. It was difficult to approve of teaching only such fragments of mathematics as the engineer could not possibly dispense with; but one must acknowledge that it was a work of thought, of something like power. Now, in a page and a half, he considers the general principles that ought to govern mathematical teaching, and then proceeds to draw up in a page of fine print a scheme of instruction headed "Practical Mathematics: Elementary Stage." He solicits criticism from three classes of persons: first, from those who think his method fit only for evening classes; second, from those who think it should be adopted in every school; third, "from other persons." We cannot disregard the summons in this third clause to testify concerning this universal interest. Let us say, then, that the destructive part of Prof. Perry's doctrine, vigorous as it is, is not a bit exaggerated. No other subject ever was, from the beginning of the world, so absurdly taught as mathematics in the old schools; and the new pedagogical methods have in this department been at their worst. In Professor Perry's constructive proposals, there are excellent features; but they relate to details into which we have no space to enter. He belongs to the generation of those who think that the final end of education is to enable students to make money, and who look upon pure science with some contempt, except so far as it may subserve that great aim of life. Such, at least, is the general impression his various writings have left upon us. In his last article, for instance, he says: "Those engineers who can most readily apply mathmatics [[sic]] to engineering problems, almost invariably descend to the position of teachers and professors in schools and colleges." This may be taken as a statement of the obvious truth that those who delight in the exercise of intellectual powers more than in the business side of the profession with its rewards, attach themselves to positions where they can enjoy their preference. But the word "descend" applied to a passage from moneymaking to the cultivation of mathematics betrays Professor Perry's opinion about a fundamental question of ethics. Men of science of the old school will say that here is the worm at the root that threatens the decadence of the twentieth century.

--Still, the most elementary mathematics ought, no doubt, to be taught in a practical spirit, mainly. More pertinent, therefore, is another general objection, namely, that Professor Perry's scheme is a mere piece of tinkering, not professing to ground itself upon any thorough analysis of the evil it seeks to remedy, not by any means erected as an engineer like Professor Perry would construct a real suspension bridge, resorting to every light of science, but rather in the old no-method by which dark ages built that crazy structure that the very asses balked at, the *pons asinorum*. Broken up into educational junk, it would afford some valuable half-ideas and lesser fractions. We can only give a single example of what we mean. Professor Perry is quite right in protesting against the notion that logarithms should not be taught until their theory is first mastered. One might as well forbid people to take photographs until they perfectly understand the molecular actions involved. But the other half of this idea is, that you should never teach the theory of logarithms until interest has been excited by seeing what marvels they will accomplish; and, in general, you should try not to teach any theory, especially if it be one requiring a good deal of effort to comprehend, until the subject it explains has been, as far as may be, brought out of cloudland, so that there may be an intellectual incentive to seeking the why of it. This is particularly important all through mathematics. Not only is it permissible, as Professor Perry demands, not to keep the Pythagorean proposition a secret until it is proved, but it should be suggested and applied to measurements of some triangles in such a way as to create a doubt as to its exactitude. Then the demonstration will mean something.

71 (20 September 1900) 235-236: **Bordeaux and its Wines, Classed by Order of Merit**.

Third English (from the seventh French) edition. By Édouard Feret. Bordeaux: Feret et Fils. 1899. French 12mo (English 8vo), pp. 846.

CSP, identification: Haskell, *Index to* The Nation. See also: Burks, *Bibliography*; Fisch and Haskell, *Additions to Cohen's Bibliography*; MSS L 159.155-156, L 159.159.

Admirable is the talent for condensation without loss of accuracy or of detail displayed in the last edition of this standard work, commonly known as Cocks's 'Bordeaux et ses Vins classés par ordre de mérite.' A humanly complete handbook and directory of the wines of Bordeaux has been brought under one cover, although sixty pages go to the indices and two hundred are covered by the scattered illustrations. The text occupies less than six hundred pages; and though a large proportion of it is in print so fine as to give a thousand words to the page, yet it contains less than the matter of Humboldt's 'Cosmos,' which the true connoisseur will consider a beggarly allowance enough, considering the relative importance of the two subjects. But where lack of space has debarred M. Feret from entering into full details, he has not failed to refer the reader to whatever authority is most accurate and copious upon the special point of Girondine enology in question. Perhaps fifty titles are so cited, so that this work becomes a trusty guide to all that can be learned from books concerning its subject; and book learning is an essential part, albeit a small one, of this science, as of every other.

The whole area of the departmet [[sic]] of the Gironde is nearly four thousand square miles, or twice that of the State of Delaware. But a fifth of it is water, and nearly half waste *landes*, and, in short, only 625 square miles, we are told, are more or less devoted to the culture of the vine, not equalling the surface of such a county as Van Rensselaer, Greene, or even Madison, in the State of New York. The population of the whole department is 800,000, so that its density is about that of the population of New Jersey. These 625 square miles may be said to embrace six different wine countries. Beginning with the worst, there is, in the first place, the district known as *Entre-deux-mers*, lying between the Garonne and the Dordogne, but restricted to parts well away from both rivers, and also not extending down below Créon. Its wine is chiefly used for distillation. Secondly, there is the alluvial soil on the borders of the Garonne and Gironde, the Dordogne, and the Isle, hardly reaching back a mile. It is known as the *palus*, and the use of it for the growth of wine is post-phylloxerine. Thirdly, there are the parts called the *côtes*, embracing everything on the right of the Garonne and Gironde not included in the *palus* or *Entre-deux-mers*, together with the Bazadais, which, on the left bank of the Garonne, from its entrance into the department some dozen miles down to Langon, extends eight or nine miles back from the *palus*. The *côtes* wines are of various character and quality. Quite the best of them are those of St. Emillion, light but *recomfortant*, with a peculiar bouquet and slightly bitter taste, rejoicing in the title of the Burgundy of the Gironde, while the comparatively modest price is prohibitory. The remaining three wine-countries of the department are situated like the Bazadais, that is, on the left bank of the Garonne or Gironde, and reaching six or eight miles back from the *palus*. Next below the Bazadais is, fourthly, the *pays de Sauternes*, a district about seven miles square, whose white wines are made with extraordinary pains, the vintage lasting two months, and the grapes being selected, in some instances, one by one. Fifthly, next below the *pays de Sauternes*, for twelve miles down, as far as Bordeaux, comes the gravelly; channelly bottom (one hesitates to say soil) denominated the *graves*, where the delicious white wines of that generic name used to be grown. But those vines having been totally destroyed by phylloxera, at present more red wine than white is produced. Some of it is of the very finest quality; for New Yorkers' favorite claret, (judging by the price they have run it up to), the famous Château Haut-Brion, is a *vin des graves*. This château stands only about a mile outside of Bordeaux; and the traveller who arrives at that town in order to study its wines, is forcibly impressed with those of the *graves*, since the neighboring communes produce some very fine *crus*, while near to Bordeaux, those of Médoc, the sixth and most famous of the Gironde wine countries, are quite inferior. Most of the chateaux which produce the incomparable wines of Médoc that are the chief glory of the Gironde--of France, a true lover of them would have us say--stand upon interminable straight gravelly ridges, their vineyards growing close about them lest the precious soil should be wasted, and are remarkably business-like places to be called chateaux. There is, however, along the Gironde, a stretch of nine miles, from the Châteaux-Beychevelle to St. Estèphe, where there is hardly any *palus*; and here the finest vineyards extend to the water, as the name Beychevelle, or "lower the sail," may remind us. This stretch is bisected by the village of Pauillac, in whose commune some of the very greatest wines are produced, and some of the estates are magnificent. The Château-Pignon, in Hector Malot's 'Un Mariage sous le Second Empire,' was probably drawn from the Château-Pichon-Longueville, which stands on one side of the mouth of the Juillac brook with the Château-Latour on the other.

The first hundred pages of the present volume give all general information about wine-making in the Gironde, beginning with the different soils and sub-soils and their suitabilities for the vine, going on to the different varieties of vine employed to make Bordeaux wines, the chief diseases and parasites of the plants, vying in multitude with those of the horse, as *coulure*, *attelabe*, the *écrivain*, the *procris*, the *pyrale*, the *euchlore*, the *ver-blanc*, the *apate*, the *puce de la vigne*, *loches*, snails,

érinose, *grillage*, mildew, the *oïdium*, the *phylloxéra*, *anthracnose*, *pourridié*; rot, brown, white, and black, *cochylis*, etc.; the whole process of cultivation, planting by five systems, grafting, the arrangement of the vineyard, pruning, etc., special methods of the Médoc, estimates in detail for a superior bourgeois vineyard, and a small artisan or *paysan* vineyard; special methods in the *graves*, in the *pays de Sauternes*, in the *côtes*, in the *palus*; methods of vintage and wine-making in Médoc, the press-house, separating the grapes from the bunch by the *trémie* and by a *grillage*; wine-presses, vats, and filling them; *décavage*, second wine, press-wine, *piquette*, the methods in the *graves*, in the *pays de Sauternes*, in the *côtes*, in the *palus*; cultivation of yeast, pasteurization, electrification, weighing musts; the treatment of the wine after it is made, the *chai*, or cellar, the cask and its bung, racking, the *coup de fouet*, with a calendar showing what the cellarer is to do each month of the year; treatment of the wine preparatory to bottling, the art of bottling, the art of drinking wine (but this important part of the treatment of wine is the only one which is decidedly evaded by M. Feret); a description of the character of each vintage from 1795 to 1897 inclusive, and statistics. The main body of the work takes up each district, or *arrondissement*, and gives a general description of it, its methods, its soil, its wines--under each district, each commune, describing it, its soil, and its wines in general terms; and under each commune is given a full list of the vineyards, with the proprietor of each, its average yield, the exact grades and character of its wines, often with a great deal of historical and other interesting information.

What has attracted our attention to the book more than anything else is the astonishing precision and accuracy with which, in very few words or even none, but merely by the order of arrangement, upwards of four thousand kinds of wine are characterized, so that the reader who has at all studied the subject will know each one almost as well as if he had tasted it. We close by remarking that the total annual yield of the five classed *crus* of Médoc is from one to two thousand tons of about 240 United States gallons each, enough to give several thousand persons a bottle every day--say, all the royalties, dukes, and marquises, and a privileged selection of counts, together with all persons who are suffering from alarm lest they should be disgraced by dying rich. The total annual export of wine from Bordeaux is, pretty steadily, thirty million United States gallons.

71 (27 September 1900) 257: **Acetylene**.

By Vivian B. Lewes. Westminster: Archibald Constable & Co.; New York: The Macmillan Co. 1900. 8vo, pp. xxvii + 978.

CSP, identification: Haskell, *Index to* The Nation. See also: Burks, *Bibliography*; Fisch and Haskell, *Additions to Cohen's Bibliography*; MSS L 159.161-162.

If, not so very long ago, when French and German chemists were disputing as to whether acetylene was most economically prepared from the bromide of ethylene or by the imperfect combustion of a Bunsen burner, some *diseuse de bonne aventure* had told them that within forty years the seventh handbook to be devoted exclusively to this subject would contain over a thousand pages and yet

be rather too condensed than otherwise, they would have smiled at the absurdity of the prediction. Yet here it is; and it is incomparably the best upon its subject, and one of the most thorough, intelligent, and judicious digests that have ever been made of our knowledge of a single chemical product, in a narrow sense of that term, so as not to include water, iron, and such things. It is divided into three parts--the first, Scientific, occupying 170 pages; the second, Technical, 525 pages; the third, Legal and Miscellaneous; the rest in small print. In order to show that our praise of the work is well deserved, we may mention such of the defects which we have noticed in its scientific part as are not too technical for our columns.

Considering how important Professor Lewes's own work on the luminosity of flames has been, it is singular that he should fail to remark that Edmund Davy, the original discoverer of acetylene, noticed that the fact that its flame is brighter than that of olefiant gas is contrary to Henry's theory of the luminosity of flames. Neither does Professor Lewes call attention to Davy's not noticing any bad smell about the gas, which shows that he made it purer than it was ever made afterwards down to recent years.

Professor Lewes says: "In 1830 Torrey noticed in the gas mains of New

York, which at that time were made of copper, the formation of a brown deposit which could be exploded by a blow or by heat, and which was probably the acetylene-copper compound." This does not do full justice to Dr. John Torrey. One of the workmen engaged in replacing the old copper pipes by iron ones, took up one of the old pipes, raised it to his mouth, and blew through it. Instantly, an explosion shattered his jaw, so that he died in a few hours. Some pieces of the pipe having been sent to Dr. Torrey for examination, he came to the conclusion (not stated by Lewes) that the brown explosive powder they contained was a compound of copper with some hydrocarbon, and he would have gone further had not Mr. Chilton claimed a prior right to investigate the problem without being able to advance towards its solution, himself, in any measure, great or small.

Prof. Lewes says that "during 1859 Boettger made some researches," etc. But in fact these researches were commenced in 1852, and were published in 1858 in the *Jahresbericht des physikalischen Vereins zu Frankfort-am-Main*. And he not only "was of opinion" that the precipitate formed by leading lighting gas into an ammoniacal cuprous solution was "a compound of copper with a hydrocarbon," but he proved it, and proved further that the hydrocarbon in question was one not well known to chemists, and that it existed in small proportion in the illuminating gas.

The fact that Adolphe Perrot made acetylene in 1858 is not mentioned; but it was hardly worth mention, since he failed to recognize its true nature.

The first of Berthelot's researches is said by Prof. Lewes to have been published in 1860. In fact, a note by him on the subject appeared in *L'Institut* for 1859. This is worth mention, because three days before Berthelot's first note was presented to the Académie des Sciences on April 30, 1860, Victor Sawitsch had shown that monobromethylene treated with caustic alkali would give this new gas, as it was then understood to be, Davy's discovery having been forgotten.

Lewes does not mention this paper of Sawitsch, nor does he mention that Bacologlio in 1860 detected acetylene in illuminating gas. Other facts not noticed are Vohl's method for preparing acetylene from American petroleum, proposed in 1865; Thénard's discovery of the conversion of acetylene into a horny substance by the silent electrical discharge; Coquillon's studies of the combustion of acetylene made in 1878. The question of how far acetylene is poisonous is fully discussed, though Hatton's observation that it does not act upon bacteria is omitted. The physiological action being treated under "Chemical Reactions of Acetylene," contrary to the general usage of chemists, it would have been well to refer to the discussion in the index under Acetylene, Physiological, and Poisonous, and not merely under Toxic. Nothing is said about the refrangibility of acetylene, investigated by Mascart in 1878, and shown by Brühl in 1887 to have an important bearing upon the problem of its chemical constitution. But this all-important problem is not attacked at all by Professor Lewes. For that reason the discussions of Paternò and Peratoner, in 1890, of the iodization of acetylene are passed over in silence.

Some of these faults are certainly to be regretted; but the fact that we can bring no more serious criticisms will be sufficient to show how admirably Professor Lewes has executed his task.

71 (11 October 1900) 295-296: **Joseph Glanvill**.

By Ferris Greenslet. New York: The Columbia University Press (Macmillan). 1900. 12mo, pp. 235.

CSP, identification: Haskell, *Index to* The Nation. See also: Burks, *Bibliography*; Fisch and Haskell, *Additions to Cohen's Bibliography*; MSS L

159.159-160.

Joseph Glanvill was that fashionable divine and Fellow of the Royal Society at whose house in Bath our friend Samuel Pepys, four years his senior, used to sit up into the small hours with fair Mistress Pennington. The reader may remember that on one such occasion she was in her smock and petticoats by the fire. Glanvill cuts a small, but decidedly interesting, figure in philosophy's ice. Höffding devotes one of his large pages to him; Lechler, half a small one. Other such authorities dispose of him in half a dozen lines. Very few of those pundits, it is safe to say, ever found time to read connectedly in his works, and those few confined themselves to one book that is far from bringing out the whole character of his thought. If such has been his treatment at the hands of those who have devoted themselves to the history of the subject, what is likely to be the state of information about him of the average student of philosophy? The manuals of English literature pass him by, we believe, without recognition.

The very title of his best-known book, 'The Vanity of Dogmatizing,' ought to suggest to the well-read metaphysician that he was probably inclined to Platonism. For that has been the way with the majority of genuine skeptics, as contradistinguished from the dogmatists of skepticism. It may not be obvious why it should be

so; but, as a matter of historical fact, so it has been. Montaigne himself was given to Platonizing. Nor could the precise hue and shade of skepticism proper to the Platonist be more nicely marked than in the phrase, "the vanity of dogmatizing." Yet so little thought has been bestowed upon

Glanvill that is has been left to Dr. Greenslet in this year of grace to inform the public of his affiliations with Cudworth, Henry More, Cumberland, and other contemporary Platonists. A downright Platonist he himself was not.

The Glanvilles came over with the Conqueror; and, beginning with that Glanvil, Chief Justice and Crusader, who first treated of the law of England, they have ever been (if only by chance coincidences) somewhat singular in their inflexible adherence to the outcome of their logic, be it agreeable or otherwise. Even the author of the popular mediæval encyclopædia, 'De Proprietatibus Rerum,' Bartholomæus de Glanville, if his logic served him no other good turn, was delightfully unfailing in the support of his theme that every object in nature conveys a moral lesson. Joseph Glanvill was, before all else, a logician, and an exceedingly sane one. To put the right value upon a man of this stamp, one needs to be alive to the rarity of genuine logical reasoning, if by that we mean proceeding from our premises to a conclusion determined by a rule that, faithfully adhered to in all cases, is manifestly calculated to hasten our approach to the truth. So defined, reasoning is a case of acting upon principle in restraint of our natural inclinations. We all know that real, genuine acting upon principle is exceptional; that at best what is so called is mostly only the working of a good habit, and at its worst of an odious inclination. Just so, what people dignify by the name of their reasonings are mostly mere passages from one judgment to another in a way in which natural bent, habit, experience, the example of the wise, half-unconsciously move them to think. In such fashion, opinion surging to one side and the other, does, from age to age, lumber along the road to truth, even through the sloughs of great questions, and rattles over firmer ground at a brisk trot. But were mankind disposed to listen to those who adjure them to scrutinize the nature of their methods of thinking, and to consider whether or not they are governed by rules that are necessarily bound to expedite the discovery of truth, perhaps opinion might progress a little faster and with fewer mishaps than it does.

It must have been when Glanvill was in Oxford, where Aristotle's voice still reverberated above all others, that he became dissatisfied with peripateticism. But instead of attacking it in large round style, as Bruno and Ramus were remembered to have done, he brought forward the piddling objection that its leading principles were not sufficiently proved. A Columbia boy of to-day might espouse Hegelianism or any other glittering generality without giving occasion for surprise; but imagine that, not doing anything of that sort, he were to attack the doctrine of the conservation of energy on the cold ground that it was not sufficiently proved, would he not show--supposing it were not affectation--a remarkable impulse to weigh arguments? 'The Vanity of Dogmatizing,' published three years after the author left Oxford, was, in the revised editions, entitled 'Scepsis Scientifica,' doubt in the interest of discovery, hopeful doubt. Poor Glanvill was out in his reckoning, if he ever looked to getting a bishopric or deanery after giving his book such a dreadful title. Its general substance may readily be imagined. One

sentence of it is quoted in every history of philosophy, about succession being all of the process of causation that can be directly observed, and the inference of a nexus being a dangerous one. This may have given Hume a hint. Glanvill's point, however, has been mistaken when this remark has been spoken of as an anticipation of Hume. For Glanvill's interest is logical, or in the furtherance of science; Hume's, psychological, or in setting up a theory of science.

After making himself well known by several publications as a "virtuoso," or cultivator of modern experimental science, Glanvill produced his logical *chef d'œuvre*. This is his 'Saducismus Triumphatus,' in which he contends for the reality of witchcraft and of ghosts. It is divided into two parts. In the first, he attacks the position of those who scout such superstitions as absurd and *a priori* impossible, and maintains, on the contrary, that the case should go to the jury to be decided according to the evidence. Almost any reader who was sincerely convinced by that first part would be sure to be completely gained over by the second, in which the evidences themselves are set forth. We mean a contemporary reader; for we shall not pronounce upon what ought to be thought of apparitions now. Grant, for the sake of argument, that all the eminent scientific discoverers who belong to the psychical-research party are superstitious fools. Still, that does not condemn Glanvill's logic. Probable reasoning is not *ipso facto* bad because in the special case it leads to a wrong conclusion. For the question is whether or not it pursues a method which would, in the long run, bring a faithful adherent of it to the truth sooner than another method or unrestrained tendencies of thought would do. If Glanvill accepts testimony too readily, that is not so much due to a defect in his logic as to ignorance of the strange and startling facts about human nature that modern studies of hypnotism and allied phenomena have revealed to us. Even at this day, men who treat the idea of any connection between this world and another as a subject for simple derision, are not those who are best acquainted with the state of the question. At any rate, Glanvill's rare logical sincerity is unmistakably evinced by his advocating with such ardor conclusions to which, as a skeptic and man of science, he must naturally have been averse.

Dr. Greenslet has made a diligent study of his subject, and has put into a compact and entertaining form all that ordinary readers, including students of philosophy and of literature, will generally be particular to know about Glanvill. It cannot be said that Dr. Greenslet is a mind of the sort to comprehend and fully appreciate Glanvill; and his attempts to trip him in his argumentation are serviceable mainly as illustrating the difference in logical grounding between the disputant of the old university and the Doctor in Philosophy of New York. But he presents the man in his proper relation to his environment, omits nothing of great interest, and draws up his account with method, care, and skill. Particularly good is his chapter upon Glanvill as a man of letters, for Glanvill was a writer of no mean skill. His style is so marked that it may be exemplified by a single sentence--a short one like most of his sentences. It is among those given by Greenslet: "He is a wonderful man that can thread a needle when he is at cudgels in a crowd; and yet this is as easy as to find Truth in the hurry of disputation."

The portrait that serves as frontispiece is full of character.

71 (18 October 1900) 314-315: A Brief History of Mathematics:

An Authorized Translation of Dr. Karl Fink's Geschichte der Elementar-Mathematik.

By Wooster Woodruff Beman and David Eugene Smith. Chicago: Open Court Publishing Company. 1900. 8vo, pp. 333.

CSP, identification: Haskell, *Index to* The Nation. See also: Burks, *Bibliography*; Fisch and Haskell, *Additions to Cohen's Bibliography*; MSS L 159.152, L 159.156.

The original of this work enjoys the sort of reputation that the approval of students in German universities can confer. Whatever that may be worth, it may probably show that readers similarly situated in this country, wanting some information about the history of arithmetic, algebra, geometry, and trigonometry, provided it be compressed and generalized, may find this translation will answer their purpose; although there is a good deal about old German books of no importance which had better have been replaced by notices of English writings that really had something to do with the development of mathematics, such as those of Shirewood, Bradwardin, Tonstall, Sacrobosco, Dee, Recorde, Digges, Oughtred, Blundevill, etc. Those readers who do not approve of an historian's wasting time in trying to make out how one event led to another, will find less of that sort of thing in this volume than in anything called a history that can easily be brought to mind. Those who are curious to know in what the mathematical interest and value of any works of mathematicians of the past really consisted, must, of course, seek their information elsewhere than in this little manual.

Its admirers praise its "breadth," and in truth it carries this quality to a high pitch--so high that the reader may oftentimes gather quite a false notion, until he becomes accustomed to the way in which Dr. Fink, in common with many another German professor, uses language, after which the same sentences will convey no notion at all. For instance, we read: "The earliest writer giving us information on the arithmetic of the Arabs is Al-Khowarazmi. The borrowing from Hindu arithmetic stands out very clearly." Now, considering that that writer was no "Arab," nor even an Arabian, but a foreigner called from Chorasmia because, within two centuries from the Hejra, the wild tribe of Mohammed had become possessed of such treasures that some sort of accounts had to be kept, and Chorasmia was the country where the art of computation had been most perfected; and considering that what was set forth in his treatise was substantially all the arithmetic the Arabians ever had--to speak of that work as giving us information about "the arithmetic of the Arabs," if it conveys any definite idea, is likely to convey a wrong one. So it is with the sentence that follows. It is true that Brahmagupta represents an earlier stage of development of the same arithmetical art as Al-Khowarazmi; and so does Bhaskara, although he wrote long afterward. But instead of there being any clear borrowing from the Hindus, many facts lead to a strong suspicion that it was in Bactria or some country north of India that the art in question originated and developed. Again, speaking of the celebrated mathematical papyrus, which we now know goes back in substance, as it professes to do, to the Tenth Egyptian dynasty, Dr. Fink says, "as the

measure of the area of the isosceles triangle with base *a* and side *b*, . . .

1/2 *ab* is found, and for the area of [etc.]. These approximate formulæ are used throughout and are evidently considered perfectly correct. The area of the circle follows, with the exceptionally accurate value {p}=$(16/9)^2$ = 3.1605." Now, anybody not acquainted with the German professorial style of expression would suppose that the papyrus contained something about an isosceles triangle; but it is not so. There is a figure of a triangular field of which two sides are marked as 4 "che" and 10 "che," intended perhaps for 400 and 1,000 cubits. The figure is not badly drawn, for the shorter marked side is very near 0.4 the longer one, perhaps 0.408 or 0.409. Being so accurate in that respect, it can hardly have been intended to be isosceles, for the angles at the base differ by about 11°, being 84° and 73°. Multiplying half the base by the side inclined to it by 84° after all made an insignificant error, less than half of one per cent. What is that in the value of a piece of land, presumably agricultural? It is not enough to dig a grave on. Now there was nothing theoretical about the Egyptian of the Tenth Dynasty. A Cincinnati porkpacker could not have a greater contempt for small quantities. He measured his field in a common-sense way. So, likewise, the Egyptian says nothing about the ratio of the circumference of a circle to its diameter, but calculates the area of a circular field by squaring 8-9 of its diameter, which is not a very awkward way of getting a 1 per cent. approximation. What Dr. Fink means by calling 3.1605 an exceptionally accurate value for {p} must remain a secret, for few of the vast number of evaluations that have been published have been so far from the truth; and if a comparison with equally ancient evaluations is intended, there can be nothing exceptional where there are not instances enough to base a rule upon.

The same sort of "breadth" pervades the modern parts of the book. Thus, on p. 250, we read that "the results of Desargues were more important for theory than for practice. More valuable results were secured by Taylor with a 'linear perspective' (1715)." One would hardly guess from this that Desargues applied his method to stone-cutting quite in the style of Monge, while Brook Taylor's mathematically admirable work (why is its title not printed with capital initials and in italics instead of so as to suggest some optical instrument rather than a book?) involved no application except to drawing. On p. 131, after several pages devoted to the writings of the great Grassmann and to others closely related to his, mention is made of

'Grassman's Formenlehre,' with no hint that this book, in which a microscope could hardly detect any originality, was by quite another Grassmann. Remarkable English and American contributions to the same branch of mathematics are passed by in silence. The editors ought not to have permitted this. About tables, no notice is taken of the valuable 8-place French logarithmic tables, nor of Mansion's and other tables for finding 12-place logarithms and their anti-logariths; and, if our memory does not play us false, the statement about Dase conveys a false impression, his name alone being on the title-page of an assigned part of the great factor table, which the book seems to say he did not calculate. The circumstance that he was insane all the time he was making the calculations is interesting enough to insure its not being mentioned by Fink.

There is, to be sure, a conclusive reply to all such criticisms. It is simply that the book is neither intended nor adapted for the use of persons who care particularly to have their information minutely accurate. Readers for whom "breadth" will cover pretty much all other sins, will find it to their taste. There is a good deal of human interest in the history of mathematics; but all such unscientific stuff has been ruthlessly excised by Dr. Fink, no matter what its significance might be for the development of mathematics. So, those who found fault with Cajori's book for being too entertaining ought to find this one perfectly unexceptionable in that respect.

The translation is excessively literal, in many places too much so to convey the precise meaning of the author. We do not doubt that the accomplished mathematicians who have executed it have in many places improved upon the original, in point of accuracy. They seem to us, however, to have committed a grave mistake in changing the title, albeit they give notice of having done so on their title-page. They justify their step by saying that the author in many places strays so far beyond the bounds of the elements that "the original title is misleading." That may be, but what of it? Is this the logic of modern mathematicians? Because the book is very badly laid out for a history of elementary mathematics, does it necessarily follow that it must be well describable as a history of mathematics in general? Perhaps there is nothing about which the sort of readers to whom the book will most appeal wish more to be informed than the origin and early history of the differential calculus, probably the most important of all those events concerning which the history of mathematics can afford us any satisfactory account. To call a book, however brief, a history of mathematics, without a fuller narrative of that revolution than this book gives, seems a terrible compromise. It was a good idea to append an alphabetical list of mathematicians with brief notices of them, had it only been longer; and as it is, to look it over and see who is in it and who is not in it, will supply a little of the amusement which the body of the work sternly refuses to bend to. There is a satisfactory index.

On the whole, the volume will probably prove one of the most useful of the meritorious series to which it belongs; although, had the translators been willing to take the trouble to produce a history of their own, they would have earned a larger measure of gratitude from American readers.

71 (22 November 1900) 410-411: **The Individual: A Study of Life and Death**.

By Nathaniel Southgate Shaler. D. Appleton & Co. 1900. 8vo, pp. 351.

CSP, identification: Haskell, *Index to* The Nation. See also: Burks, *Bibliography*; Fisch and Haskell, *Additions to Cohen's Bibliography*; MSS L 159.168-169; MSS 1432, 1432(s) (drafts).

Nathaniel Southgate Shaler (1841-1906) was an American geologist and educator. He received private instruction from Agassiz while at the Lawrence Scientific School. In 1864, Shaler was appointed assistant professor of paleontology at Harvard and from 1865 until 1872 was in charge of the Lawrence Scientific School. Shaler wrote mainly on geology, intellectual property rights, and nature studies.

Thinkers of to-day, as compared with those of thirty years ago, are much more disposed to look at matters in a spiritualistic light, and considerably less opposed to the acceptance of personal immortality, and are anxious to have the question further sifted. So that, when a naturalist of such standing as Professor Shaler, a pupil of Louis Agassiz, too--a warrant that he must have examined sympathetically into the idealizing aspects of the question--produces a book about individual life and death, he may be sure of being read attentively. The book is interesting throughout. Many of its multitude of suggestive ideas light up this thing or that by the side of the road enchantingly. The reader will rise from his perusal feeling that he knows more of human nature than he did when he sat down, and that, too, of an encouraging and warming kind. Vileness will not seem to him so rife as it had seemed. But if one expects to find the question handled with the grasp of a master, he will be disappointed. At critical points it is almost timid, as if the author's scientific reputation had to be considered, or, may be, some responsibility connected with Harvard University. At any rate, we do not seem to have been brought one inch nearer to an opinion about personal immortality. The style and accuracy of statement can best be judged by a specimen:

"A man does not derive the muscular strength he may use in battle from the fight; he has probably gained it in some kind of profitable labor. His courage, his obedience, his endurance in the trials of a campaign are not bred in it; they are the product of his whole life, and that of his ancestors, who gave him his nature and nurture. Men must have in them all the qualities that go to make the soldier before they approach the business of war. All that discipline does is to give a certain mechanical readiness for duty; it makes practically nothing of the soldierly quality.

"Those who doubt the statements just made should look over the history of European states. They would see that the most soldierly people of that continent are the Swiss, who for a hundred years have hardly felt the touch of war. Yet judges of what makes the fighting man, feel that at any moment they would give an admirable account of themselves. Their martial nature, born of national independence and hard, patient labor, with a simple military training to give it embodiment, is enough to deter the greedy folk about them from disturbing their repose. Just beside Switzerland, that has bred its soldiers in enduring peace, we see the French, a folk of endless warring, where hardly a generation in a thousand years but has known compaigns. We hear from them the martial note in their worship of arms and the glory that arms may win; in their trust to the test of battle for the decision of all-important personal and national matters. Surely, if a people gain in the higher qualities by the uses of war, we should find the profit here; for rarely if ever before in the history of man has there been so admirable a chance for this schooling to do its work. What do we find as the result of this age-long process of developing the higher virtues: courage, high-mindedniess [[sic]], patriotic self-devotion, the things for which we pay with the lives of our best youth?"

If a touch of exaggeration can be detected here and there in this passage, it is no more than a just sampling of the book would yield.

Professor Shaler ought to be thoroughly acquainted with the present state of chemistry, physics, and dynamics, in their general and geological aspects, but he does not seem to aim at particularly precise statements about these matters. Thus, he tells us, p. 11, that "the worst stumbling-block in our endeavor to found our theory of the universe on the supposition that it is throughout an expression of energy," is action at a distance; while, in fact, it was from action at a distance that the conception of energy was derived, and there it is most readily applied. On p. 14 we are told that "from the point of view of our present inquiry, the characteristic feature of the ether is that it is undifferentiated." By this he seems to mean that it does not consist of atoms; but it has been generally supposed to do so, and with some reason. Indeed, the question whether it is not a chemical substance is on the tapis. On p. 9 gravitation is called a mode of energy, and is said to be proportional to the number of atoms there may be in any aggregate, and inversely proportional to the distance. It is rather proportional to the sum of the atomic masses--that is, to the total mass acting upon the particle considered-than to the number of atoms. As the word gravitation is used by astronomers, it is an acceleration, not a mode of energy. The energy of gravitation is greater the greater the distance of the pair of gravitating masses. Would not this be understood to be denied in saying that it is "inversely proportional to the distance"? On p. 2 we are told that atoms are commonly held to be permanent units, which hardly represents the present prevalent opinion of astro-chemists and chemical physicists. On p. 75 the fact that the sum of the energy is constant seems to be given as a reason for not admitting that an atom is a centre of energy. We will not criticise this, because we do not know that we can extract any meaning from it. There are many other passages equally little calculated to impress the physicist with the author's soundness.

But it is in the biological and still more the human parts of this work that the expression becomes clear and the observations interesting. It gives the reflections of a geologist and naturalist approaching old age upon human life and death--not at all, however, those of a typical geologist and naturalist, but one of marked peculiarities; a sharp observer, who has evidently always been much interested in individual men and women, and in special cases, generally. No doubt, the author seems to himself to have put forward a philosophical theory of the individual, or, at least, contributions towards such a theory. But we will not attempt to give a more exact account of a book whose chief interest lies in its details. Anybody will find it agreeable and readable.

71 (6 December 1900) 449-450: **The Progress of Invention in the Nineteenth Century**.

By Edward W. Byrn. New York: Munn & Co. 1900. 8vo, pp. 476.

CSP, identification: Haskell, *Index to* The Nation. See also: Burks, *Bibliography*; Fisch and Haskell, *Additions to Cohen's Bibliography*; MS L 159.171.

It is a primary rule of the ethics of rhetoric that every prose composition should begin by informing the reader what its aim is, with sufficient precision to enable him to decide whether to read it or not. If the title can do this, all the better. 'Under

the Red, Red Rose' tells us what we have to expect better than any description in abstract terms that the writer could furnish. The man who puts pen to paper to produce anything like a treatise should, for his readers' sake, and for his own, begin by defining precisely what his book is intended to tell. If the title of this work does so, then certainly among the characteristics of the nineteenth century must be reckoned the peculiar meaning it has imposed upon the word "invention." Nobody, of course, will expect to find here anything about poetic invention, nor about imaginary quantities and homogeneous coordinates; nor about Trusts, clearing-houses, trade-unions, and postage-stamps--great inventions all; nor yet about Bunsen's ice calorimeter and the Holtz machine. One may search the pages in vain for the second-sight trick, the bunco game, or spiritualistic manifestations. It would seem at first, therefore, that under the term "invention" the author means to include only patented inventions; but that is not so, either, for, among the "leading inventions of the nineteenth century," he reckons the different systems of medical practice, the Voltaic pile (which was neither patented nor produced in the nineteenth century), the discoveries of the different chemical elements, Babbage's calculating machine, the Suez Canal, the Brooklyn Bridge, the Capitol at Washinton [[sic]], etc. We can only conclude that the writer had never defined to himself what it was that he proposed to treat of. If he had done so, he certainly would have made a much better book, for he has shrunk from no amount of labor and research.

In the same way, under the different heads, we are furnished with no means of foreseeing what will be found in the book, and what not. For instance, as long as bridges are included at all, one would expect to find here some mention, at least, of the first Niagara bridge, which involved whatever invention there was in the Brooklyn bridge; and this receives a paragraph and picture. The chief inventional feature of these bridges, the stiffening-truss, is not pointed out. The manner in which all the small bridges of the country are now supplied, almost ready-made, by bridge companies is an inventional characteristic of the century worthy of note; but it is not noticed. Almost every chapter is open to similar criticism. Many of the features and circumstances which are the most fundamental, characteristic, and otherwise worthy of attention are passed by unrecognized. The book is, of course, not addressed to persons of any particular technical or scientific knowledge; and those to whom it is addressed will, in any case, necessarily remain ignorant of much that is highly significant. Yet if the author had made space by cutting out all that does not relate, directly or indirectly, to patented inventions, and had used that space to explain the importance of the matters best repaying the ordinary reader's attention, he would have immensely increased the usefulness of his work. Take, for example, the cut-off of a steam-engine. From the few words that are said about it, no definite idea can be obtained of how much it accomplishes. A single paragraph might have given the ordinary reader an insight into the steam-engine which he would have been very glad to gain.

We should not have taken the trouble to make such criticisms if this work belonged to the ordinary type of picture-books about inventions, of which there are so many. It is a serious attempt to give an account of the "inventions," whatever that may mean, of the century, of their successive improvements and gradual

adoption, such as every intelligent men must desire to read. It has no value as literature; but it goes over the ground with a good approach to thoroughness. The index contains nearly nine hundred entries. It is a work upon which far more labor has been expended than can be paid for by the money it can be expected to bring to its author. It is, therefore, worthy of respectful criticism, and not of being passed by as "highly interesting." It may be hoped that in a future edition the improvements above suggested may be made. If it is thought that they would render the book too dry, insertion concerning the personality of some of the most remarkable inventors--most of them men of marked personality, and of various different types--would enliven the text. But, as far as we remember, this has been done only in the case of Goodyear.

An advertisement slipped into the volume admonishes us that the work makes claims to "human interest." If this means that it has the sort of value that literary power might give to such a book, we cannot allow the claim. But it certainly is curious to see how human life appears as seen from the windows of the Patent Office. There are some three hundred illustrations and figures, all most pertinent and clear. A view of the steamer *Oceanic* as if sailing above the best-known part of Broadway is striking.

71 (21 December 1900) 515-516: The Story of Nineteenth Century Science.

By Henry Smith Williams, M.D. With illustrations. Harper & Bros. 1900. 8vo, pp. 475.

CSP, identification: Haskell, *Index to* The Nation. See also: Burks, *Bibliography*; Fisch and Haskell, *Additions to Cohen's Bibliography*; MS L 159.175.

A brief and extremely popular account of the general progress during the nineteenth century of the physical and natural sciences was wanted at this time, and Dr. Williams has drawn it up quite as successfully as could be expected. He does not cover the whole field of those sciences, but deals with all the most significant parts, and adds, besides, a chapter upon modern psychology. To call what is here dealt with "Nineteenth Century Science," ignoring mathematics, linguistics, archaeology, economics, is to do injustice to the nineteenth century, but it is an injustice that we are accustomed to. The word "Story" in the title may serve as a hint to the prospective purchaser of the volume that he may expect a style which strains a little, and rather uncomfortably, after effect and sensation. Still, while there is some truth in that, especially in the earlier part of the book, it would be a great mistake to suppose that this is one of those publications which assume that science consists in producing startling novelties and thrilling marvels. On the contrary, it is a serious work, written by a man who, if he does not everywhere show himself a master of the particular branch of science of which he is treating, which would be almost inconceivable, yet does show that he knows what science really is better than a good many men who go by the name of scientists. He seizes upon the great and fruitful ideas which have been developed in each of the

branches of which he treats, and shows how it has been evolved, in a way to make his work worthy of being called, not a mere "Story," but a History of the Nineteenth Century in respect to ten, at least, of the eleven sciences which he considers. To do this in such a way as to be readable without fatigue by everybody who ever reads anything but a newspaper or a novel, is a veritable feat. That it is easy enough to be entertaining about science we know, but it is not easy to write lightly and yet picture science as it appears to scientific men.

We must not be understood as meaning that the book wraps up any profound insight into the nature and methods of science, as Whewell's immortal history did. It can teach nothing to scientific men, because it looks upon science precisely as they do; but it will be highly instructive for the great public for which it is intended. Bad proofreading, with which we are becoming sadly familiar of late, must be responsible for Lagrange being named as the author of the 'Mécanique Céleste,' for Agassiz's work on glaciers being dated twenty years too late, and a number of such misleading statements, in spite of which a great deal of pains has evidently been taken to insure accuracy in details. The book is not a mere compilation. The writer has looked into the memoirs, and has otherwise sought information at first hand. The volume also contains upwards of a hundred illustrations, almost all of which are positively interesting. Three-quarters of them are portraits. The writer of this notice was acquainted with the originals of nearly half of them, and can testify that those are characteristically portrayed, while most of the others carry conviction.

Nobody can dream that it is humanly possible to write a work like this which is not open to much criticism. The writer must have a weak side, and Dr. Williams shows his, the more exact is the branch of science with which he deals. This is unfortunate, for that is just where mastery would be most desirable. For example, Dr. Williams plainly shows his aversion to the idea of action at a distance, to which he has no doubt been encouraged by Kelvin and his followers. Now there are just three reasons which render the opinion of those physicists a tenable one. The first is purely logical: it is that as long as we are forced to admit an all-pervading ether, we have no need of any action at a distance, nor, indeed, of any other matter at all, and all our notions of rigid mechanics can be replaced, or be regarded as replaceable, by an amplified hydrodynamics. The second reason appeals to the lessons of the history of science. Because Faraday had no mathematical training, and was consequently unable to think clearly about action at a distance, he was led to develop another way of thinking about the forces of electricity which not only is extremely attractive to a mathematical mind, but also gave rise to Maxwell's theory of electricity, and thence to all the conceptions of Hertz (with the Marconi telegraph to testify to their value), and to the vortical theory of matter, with the applications that J. J. Thompson is making of it. The third reason is more positive, although even this is not conclusive: it is that the properties of elastic solids cannot be accounted for by attractions and repulsions between pairs of particles. But Dr. Williams does not touch upon any of these things as causes of the opinion he seems to espouse. He does not tell us that action at a distance was universally accepted by all whose opinions were of any account through the first half of the century, nor how the contrary belief has gradually become respectable. He leaves the reader to imagine, as the popular reader will be sure to do, that the objection to action at a distance is no more than might have weight with a philosopher of Newton's century--its inconceivability. Now there can be no manner of doubt, in the mind of a psychologist that this "inconceivability" of action at a distance is due to the circumstance that the great mass of every-day experience in regard to the communication of forces is of one solid body pushing another. Yet, whatever theory we may entertain about action at a distance, it is an indisputable fact that in such ordinary experience there is really no contact at all between the atoms of the different solids. For if two pieces of iron or glass are brought into actual contact, they will stick together so that they can only be torn apart. In short, the "inconceivability" of action at a distance is of the same kind as the inconceivability of any occult phenomenon, such as the mixture of two liquids making a solid, two colorless liquids making a black mixture, red and green lights making yellow, etc. An inconceivability which does not prevent an hypothesis from being perfectly exact and consistent is no good reason for rejecting it. Although Dr. Williams is so averse to the admission of action at a distance, he seems almost equally so to the only possible escape from it, that of the ether. He even suggests that empty space may fulfil its functions. He does not point out that if light, during the eight minutes after it leaves the sun before it impinges on the earth, is in empty space, the doctrine of the conservation of energy is false.

Passing from physical conceptions to his statement about the state of opinion among physicists, we also find inaccuracies which, though perhaps of no particular importance in so very popular a history, nevertheless show that exact science is not the author's element. Certainly, there is no more important page of the history of physics than that which relates to the development of ideas concerning heat, energy, and gases from 1824 to 1875. But we do not find that the account here given of this movement of thought is all that might be desired. The author asserts that Sadi-Carnot, in 1824, explicitly stated that a definite quantity of heat could be transformed into a definite quantity of work. But, on the contrary, though it is said that Carnot's posthumous papers show that he subsequently entertained this idea, yet in his celebrated book his doctrine is that heat is a fluid, that its quantity remains unchanged, and that it does work in falling from a higher to a lower temperature just as water does in passing from a higher to a lower level. Nothing is said in this "Story" about the second law of thermodynamics. The reader will get the idea that, Joule's results being admitted, or we may even say Rumford's, there was no further difficulty with the theory of the steam-engine; and no adequate recognition is given to the work of Clausius. In regard to the kinetical theory of gases, the names of Krönig, Boltzmann, Van der Waals do not appear. Regnault, Amagat, Willard Gibbs are never mentioned.

In chemistry, we find such assertions as that hydrogen being univalent while oxygen is bivalent, "makes it plain that we must expect to find no more than three compounds of those elements." It did not make the matter plain to those who held to the strict univalence of chlorine; and Dr. Williams says nothing about variable

valencies, but rather implies their fixity. The history of opinion concerning Mendeléef's law is inexcusably inaccurate after the admirable history of the matter by Venable. The importance of Newland's octaves is much exaggerated, since ideas upon the subject, about as nearly correct as his, were generally rife among advanced chemical thinkers of that day. Such comments might be continued to great length. It must suffice to say briefly that the chapter on psychology, although possessed of some merit, is less good than the others.

Endmatter

Footnotes

[†1] For a history of *The Nation* see: Frank Luther Mott, *A History of American Magazines, Volume III: 1865-1885* (Cambridge: The Belknap Press of Harvard University Press, 1967), 331-356; Gustav Pollak, *Fifty Years of American Idealism: The New York Nation 1865-1915* (Boston: Houghton Mifflin Company, 1915); Allan Nevins, *The Evening Post: A Century of Journalism* (New York: Boni and Liveright, 1922); Alan Pendleton Grimes, *The Political Liberalism of the New York* Nation *1865-1932*, The James Sprunt Studies in History and Political Science, vol. 34 (Chapel Hill: University of North Carolina Press, 1953). On Wendell Phillips Garrison, see *Letters and Memorials of Wendell Phillips Garrison, Literary Editor of "The Nation" 1865-1906* (Boston: Houghton Mifflin Company, 1909).

[†2] This issue is raised at several points in the Garrison-Peirce correspondence (MS L 159). Max H. Fisch and Daniel C. Haskell ["Some Additions to Morris R. Cohen's Bibliography of Peirce's Published Writings," pp. 375-381, in *Studies in the Philosophy of Charles Sanders Peirce*, edited by Philip P. Weiner and Frederic H. Young (Cambridge: Harvard University Press, 1952)] cite additional evidence that Garrison pruned Peirce's reviews (see especially pp. 376-377).

[†*] The use of the word *manifestus* in this sense, which is almost peculiar to modern physicists, and that with out explanation, seems to indicate the existence of a sufficiently large class of physicists early in the thirteenth century to have developed a diction of their own.

[††] Another word betraying the existence of a large class of natural philosophers.

[†*] The 'Century Dictionary' attributes the word "Spagiric" to Paracelsus; but it is certainly older.

[†*] This is the local civil date, N.S. The O.S. date was June 26. Prof. Halsted gives it as July 26, bringing the month to N.S., but not the day. Poggendorff

(Wörterbuch) makes it June 8, bringing the day to N.S., but not the month, A "Boston Almanac" for that year, referred to for the date, gives it as July 7, which can be defended as being the *Boston time* of the beginning of the eclipse. Herschell's 'Outlines' also gives it as July 7, being the Greenwich *astronomical* date of the greater part of the eclipse. Such is the treatment minor dates receive. No other books were referred to. Not one was right.