Frontmatter

Title Page

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Part Three: 1901-1908

Compiled and Annotated by
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CONTENTS

INTRODUCTION 13

72 (10 January 1901) 36-37

WALLACE'S STUDIES 17

72 (24 January 1901) 72-73

WILLIAM HERSCHEL 20

72 (24 January 1901) 76

Webster's International Dictionary of the English Language,

edited by W. T. Harris, 23

72 (31 January 1901) 96-97

SHAFTESBURY 25

72 (28 March 1901) 254

NOTES [on Bowley's 2
Elements of Statistics] 8

72 (28 March 1901) 258-259

By Land and Sea, 2
by John M. Bacon 9

72 (13 June 1901) 479-480

Essays in Illustration of the Action of Astral Gravitation in Natural Phenomena,

by William 3
Leighton 1
Jordan

72 (20 June 1901) 497-498

Le Volcabulaire [[sic]] Philosophique, par Edmond Goblot34

73 (25 July 1901) 70

NOTES [on Turner's *Knowledge, Belief, and Certitude*]35

73 (1 August 1901) 95-96

BERKELEY'S WORKS	3 6

73 (1 August 1901) 99-100

Bibliotics, by Persifor Frazer	3 9

73 (15 August 1901) 139-140

The Philosophy of Religion in England and America,

by Alfred Caldecott	4 1

73 (29 August 1901) 172-173

SOME PHYSICAL BOOKS	4 3

73 (3 October 1901) 267-268

MAHER'S PSYCHOLOGY	4 7

73 (24 October 1901) 325-326

vol. III, by John Beattie Crozier 6 0

74 (27 February 1902) 178-179

Classification, Theoretical and Practical, by Ernest Cushing Richardson61

74 (6 March 1902) 192-194

PAST EUR 6 2

74 (3 April 1902) 273-274

GIDDINGS'S INDUCTIVE SOCIOLOGY 6 8

74 (24 April 1902) 322-324

THE NATIONAL ACADEMY OF SCIENCES 7 1

74 (29 May 1902) 433-434

The Story of the Vine, by Edward R. Emerson 7 5

75 (10 July 1902) 31

NOTES [on Delta's *Charades*] 76

75 (10 July 1902) 36-37

A Study of the Ethics of Spinoza, by Harold H. Joachim76

75 (17 July 1902) 53

NOTES [on Atkinson's *Electrical and Magnetical Calculations*]78

75 (24 July 1902) 71

NOTES [on Thorpe's (*lapsus calami* for Forsyth) *Theory of Differential Equations*, vol. 4] 78

Probably by Peirce

75 (24 July 1902) 71

NOTES [on Drude's *Theory of Optics*] 79

Probably by Peirce

75 (11 September 1902) 209-211

PAULSEN 9
'S KANT 1

75 (18 September 1902) 229-230

The Principles of Logic, 9
by Herbert Austin Aikins 5

75 (2 October 1902) 273

The Theory of 9
Optics, by Paul 8
Drude

75 (23 October 1902) 329-330

AVIA 9
TION 9

75 (13 November 1902) 390

The Origin and Significance of Hegel's Logic, by J. B. Baillie103

75 (27 November 1902) 430

Theory of Differential Equations, part III, vol. IV, by Andrew Russell

Forsyth104

75 (18 December 1902) 485-486

ELLWANGER'S PLEASURES OF THE TABLE	1 0 5

75 (25 December 1902) 506-507

Sundials and Roses of Yesterday, by Alice Morse Earle108

76 (29 January 1903) 99-100

Euclid, by Thomas Smith	1 1 1

76 (5 March 1903) 194

NOTES [on *The Physical Papers of Henry Augustus Rowland*]113

76 (19 March 1903) 226

A CORRECTION

Letter by J. S. Ames, with editorial reply by Peirce	1 1 4

76 (2 April 1903) 269

NOTES [on Fiske's *Cosmic Philosophy*] 116

76 (30 April 1903) 349-351

THE NATIONAL ACADEMY MEETING 117

76 (21 May 1903) 418

NOTES [on Mellor's *Higher Mathematics for Students of Chemistry and Physics*]121

76 (21 May 1903) 419-420

HEGEL'S LOGIC INTERPRETED 122

76 (28 May 1903) 436

NOTES [on Whittaker's *A* 1
Course of Modern Analysis] 2
5

76 (4 June 1903) 462-463

Personal Idealism, 1
edited by Henry Cecil 2
Sturt 5

76 (11 June 1903) 482

Dictionary of Philosophy and Psychology, vol. II, by James Mark
Baldwin127

76 (18 June 1903) 497-498

NOTES [on Carus's edition 1
of Kant's *Prolegomena*] 3
0

77 (16 July 1903) 57-58

Studies in the Cartesian Philosophy, by Norman Smith130

77 (23 July 1903) 81-82

Inorganic Chemistry, by J. I. D. Hinds	132

77 (30 July 1903) 98-99

CLERKE'S ASTROPHYSICS	133

77 (13 August 1903) 141

Light Waves and Their Uses, by A. A. Michelson

Waves and Ripples in Water, Air, and Æther, by J. A. Fleming136

77 (3 September 1903) 189

NOTES [on Cohn's *Tests and Reagents*]	138

77 (10 September 1903) 208

NOTES [on Krauch's *The Testing of Chemical Reagents for Purity*]138

77 (10 September 1903) 208

NOTES [on Perrine's *Conductors for Electrical Distribution*]139

77 (10 September 1903) 219

NOTES [on British universities] 139

By an anonymous author

77 (17 September 1903) 229

NOTES [on British universities] 140

77 (1 October 1903) 263-264

BRITISH AND AMERICAN SCIENCE 140

Editorial reply by Peirce to letter by "H. T."

77 (1 October 1903) 265

THE DECLINE OF MATHEMATICS IN ENGLAND 142

Letter by "H. T."

77 (15 October 1903) 308-309

What is Meaning? by V. Welby

The Principles of Mathematics, vol. I, by Bertrand Russell143

77 (22 October 1903) 320

PRACTICAL APPLICATIONS OF THE THEORY OF FUNCTIONS 145

Letter by James McMahon, with an editorial reply

probably by Peirce

77 (5 November 1903) 360

FRANCIS ELLINGWOOD ABBOT 147

Letter

78 (11 February 1904) 110

NOTES [on Walmsley's *Modern Practical Electricity*, Foster and Porter's *Elementary*

Treatise on Electricity and Magnetism, Barnett's *Elements of Electromagnetic Theory*, and Parr's *Electrical Engineering Measuring Instruments*]149

78 (11 February 1904) 113-115

FAHIE'S GALILEO 149

78 (3 March 1904) 171

NOTES [on Hilton's *Mathematical Crystallography and the Theory*

of Groups of Movements] 154

Probably by Peirce

78 (3 March 1904) 171

NOTES [on Campbell's *Introductory Treatise on Lie's Theory of Finite*

Continuous Transformation Groups] 155

Probably by Peirce

78 (10 March 1904) 191-192

NOTES [on Suplee's *The Mechanical Engineer's Reference Book*]155

Probably by Peirce

78 (17 March 1904) 211

NOTES [on Woodbridge's *The Philosophy of Hobbes in Extracts and Notes collected from his Writings*] 156

78 (17 March 1904) 215-216

THE METRIC FALLACY 156

78 (24 March 1904) 231

NOTES [on van't Hoff's *Physical Chemistry in the Service of the*

Sciences]161

Probably by Peirce

78 (24 March 1904) 237

The Reminiscences of an Astronomer, by Simon Newcomb161

78 (14 April 1904) 298

Lectures on the Logic of Arithmetic, by M. E. Boole

Elements of the Theory of Integers, by Joseph Bowden162

78 (28 April 1904) 328-330

THE NATIONAL	1
ACADEMY	6
MEETING	4

78 (28 April 1904) 335-336

COMTE'S	1
PHILOSOPH	6
Y	9

78 (19 May 1904) 393

NOTES [on Jones' *Notes on Analytical Geometry: An Appendix*]172

78 (26 May 1904) 411

79 (25 August 1904) 162

NOTES [on Buchanan's *The Mathematical Theory of Eclipses*]181

79 (8 September 1904) 203-204

The Collected Mathematical Papers of James Joseph Sylvester, vol. I181

79 (15 September 1904) 219-220

LOGICAL LIGHTS 183

79 (29 September 1904) 264-265

Outlines of Psychology, by Josiah Royce 186

79 (17 November 1904) 396

NOTES [on Mendeléefs *An Attempt towards a Chemical Conception of the Ether*]189

79 (17 November 1904) 396

NOTES [on Murray's *Introduction to Psychology*] 189

79 (17 November 1904) 396

NOTES [on Cajori's *Introduction to the Modern Theory of Equations*]190

79 (17 November 1904) 402-403

Experimental Psychology and its Bearing upon Culture,

by George	1
Malcolm	9
Stratton	0

79 (1 December 1904) 432-434

THE NATIONAL	1
ACADEMY IN NEW	9
YORK	2

79 (8 December 1904) 460

NOTES [on Olsen's *Textbook of Quantitative Chemical Analysis*]196

80 (5 January 1905) 18-19

The Preparation of the Child for Science, by M. E. Boole,197

80 (26 January 1905) 71-72

80 (30 March 1905) 244-256

The Phase Rule and its Applications, by Alex. Findlay209

80 (27 April 1905) 327-328

THE NATIONAL ACADEMY OF SCIENCES 211

80 (4 May 1905) 360-361

Philosophy as Scientia Scientiarum, and a History of Classifications of the Sciences, by Robert Flint

A Syllabus of Certain Topics of Logic, by C. S. Peirce215

80 (11 May 1905) 374

NOTES [on Garcin's *N-Rays*] 218

80 (1 June 1905) 438

NOTES [on Kamensky's translation of Mendeléef's *Principles of Chemistry*]219

80 (1 June 1905) 444-445

Scientific Fact and Metaphysical Reality, by Robert Brandon Arnold220

80 (8 June 1905) 461

NOTES [on Santayana's *The Life of Reason*] 221

80 (22 June 1905) 503

NOTES [on Freund's *The Study of Chemical Composition*]222

80 (29 June 1905) 527-528

James Watt, by Andrew Carnegie 223

81 (13 July 1905) 33

NOTES [on Bacon's *Balloons, Airships, and Flying Machines*]226

81 (13 July 1905) 33-34

NOTES [on Hampson's *Radium Explained*] 226

81 (13 July 1905) 42-43

Sociological Papers, published by the Sociological Society

Probably by Peirce

81 (9 November 1905) 382

NOTES [on Wiart's 2
La Cité Ardente] 4
2

Probably by Juliette Peirce

81 (23 November 1905) 417-419

THE NATIONAL ACADEMY OF 2
SCIENCES AT NEW HAVEN 4
3

81 (14 December 1905) 486-488

GOSSE'S SIR 2
THOMAS 4
BROWNE 8

82 (18 January 1906) 61

Radio-Activity, by E. Rutherford	2 5 4

82 (22 February 1906) 160-161

ALFRED RUSSEL WALLACE	2 5 6

82 (22 March 1906) 242-243

HALDANE'S DESCARTES	2 6 1

82 (26 April 1906) 341-342

MEETING OF THE NATIONAL ACADEMY OF SCIENCE	2 6 4

82 (7 June 1906) 475-476

Congress of Arts and Sciences, edited by Howard J. Rogers269

83 (5 July 1906) 17-18

The Dynamics of Living Matter, by Jacques Loeb

Chemistry of the Proteids, by Gustav Mann	271

83 (12 July 1906) 43

*The Life and Experiences of Sir Henry Enfield Roscoe*274

83 (26 July 1906) 78

NOTES [on Clerke's *System of the Stars*]	275

83 (13 September 1906) 226-227

ARISTOTLE'S ETHICS	276

83 (25 October 1906) 353-354

An Introduction to Logic, by Horace William Brindley Joseph279

83 (20 December 1906) 544-545

Side-Lights on Astronomy and Kindred Fields of Popular Science,

by Simon 2
Newcomb 8
2

84 (24 January 1907) 92

The Scientific Papers 2
of J. Willard Gibbs 8
4

84 (21 February 1907) 181-182

A History of Chemistry, 2
by Ernest von Meyer 8
5

84 (28 February 1907) 203-204

Thought and Things, vol. I, 2
by James Mark Baldwin 8
7

85 (12 September 1907) 229

NOTES [on Stickney's *Organized Democracy*] 290

85 (17 October 1907) 355

The Collected Mathematical Works of George William Hill, vol. IV290

85 (31 October 1907) 396

THE WORK OF GEORGE W. HILL 292

Letter by Simon Newcomb, with editorial response by Peirce

85 (19 December 1907) 570-571

LORD KELVIN 293

85 (26 December 1907) 579

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 are issued in three separate parts, this volume being the third. 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 [[sic]] 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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"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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Volume 3

CHARLES SANDERS PEIRCE: CONTRIBUTIONS TO *THE NATION*: **1901**

72 (10 January 1901) 36-37: WALLACE'S STUDIES

Studies, Scientific and Social.

By Alfred Russel Wallace. Macmillan. 1900. 2 vols. 8vo, pp. 541 and 543.

With 114 illustrations.

CSP, identification: Haskell, *Index to* The Nation. See also: Burks, *Bibliography*; *List of Articles*; MSS L 159.171, L 159.173-174.

Fifty-two essays, one for every card in the pack, in the four suits of geology, evolutionary biology, anthropology, and sociology, written in Wallace's clear, flowing style, and with all his argumentative force and ingenuity, full of information upon all sorts of matters of curiosity, afford nothing more interesting among all these than their portraiture of the writer himself. Not quite a typical man of science is Wallace; not a man who observes and studies only because he is eager to learn, because he is conscious that his actual conceptions and theories are inadequate, and he feels a need of being set right; nor yet one of those men who are so dominated by a sense of the tremendous importance of a truth in their possession that they are borne on to propagate it by all means that God and nature have put into their hands--no matter what, so long as it be effective. He is rather a man conscious of superior powers of sound and solid reasoning, which enable him to find paths to great truths that other men could not, and also to put the truth before his fellows with a demonstrative evidence that another man could not bring out; and along with this there is a moral sense, childlike in its candor, manly in its vigor, which will not allow him to approve anything illogical or wrong, though it be upon his own side of a question which stirs the depths of his moral nature. One cannot help entertaining a great esteem for him, even when he is most in earnest and at his *isms*.

A poor reviewer needs to summon all his professional omniscience to comment upon fifty-two discussions with such a range as these; but he can plead the stern exigencies of space as a reason for only noticing a few of them. The seventh essay gives a remarkably luminous and distinct popular account of the different families of monkeys. The reader is disposed to wonder what set Alfred Russel Wallace writing such indisputable matter; but he finds out what it was when, the description being done, in reviewing the order, he pronounces monkeys to be rather low down in the scale of quadrupedal life, both physically and mentally. He still acknowledges that man is the crown of the animal kingdom in both respects. One of these days, perhaps, there will come a writer of opinions less humdrum than those of Dr. Wallace, and less in awe of the learned and official world--for why is not this as supposable as a fourth dimension of space?--who will argue, like a new Bernard Mandeville, that man is but a degenerate monkey, with a paranoiac talent for self-satisfaction, no matter what scrapes he may get himself into, calling them "civilization," and who, in place of the unerring instincts of other races, has an unhappy

faculty for occupying himself with words and abstractions, and for going wrong in a hundred ways before he is driven, willy-nilly, into the right one. Dr. Wallace would condemn such an extravagant paradoxer. If a man must indulge in paradox, let him do so in moderation.

Somewhat like the monkey essay in method is the first one in the book, which sketches, not without artistic skill, the Yellowstone Park, the somewhat differently wonderful Grose valley in New South Wales, and other inaccessible valleys, the text being helped by excellent photographs (all the illustrations in the book, by the way, are choice); but all this is but a prelude to an argument that these wells, as they might be called, with their lofty vertical sides, have been worn out by running water.

The anthropological essays relate mainly to the Australians and to the Polynesians; though there is interesting information about the Malays, the Papuans of New Guinea, the Veddahs of Ceylon, the Ainos of Japan, and the Khmers of Cambodia, ancient and modern. The admirable portraits here are, of themselves, mines of instruction. The Australian physiognomies, with their large, round heads, broad and good foreheads, beetling brows, shapely ears, good muscular development, and full beards, would be remarkably European in the impression they make, were it not for their wide mouths, thick lips, and great gobs of noses. The only Aino face here shown has a still better forehead, an excellent nose, not a bad mouth, and might perfectly well pass for a modern Greek of superior intelligence. The Veddahs are naked and completely savage huntsmen, looked upon by the other inhabitants of Ceylon as little higher than wild beasts; yet their faces betoken tremendous intensity and no little subtlety of intellect, refinement of judgment, humanity of feeling, observation, power of will, along with utter absence of civilized discipline. When Wallace pronounces these three races to belong to the same fundamental division of the human race as ourselves, the feeling their portraits excite assents to it. With the sculptured heads of the ruins in Cambodia, it is different. This civilization is not very ancient. It was in all its grandeur only about six centuries ago; and the most ancient work goes back only to 250 B. C. But the faces recall the theory of M. de la Couperie that Chinese civilization was derived, probably indirectly, from Babylonia, about 2300 B. C., and was brought by a tribe which slowly migrated from Western Asia, perhaps Bactria or Chorasmia. For, along with Mongol eyes, we see high foreheads, strong jaws, somewhat Assyrian mouths, and remarkably fine, large noses, of a peculiar character. The two untrustworthy drawings of modern Khmers look European enough, but do not in any respect resemble the ancient sculptures, except in their general intelligence.

In regard to the Polynesians, whom Wallace also believes belong to the Caucasian stock (for he takes it for granted that there is such a stock), it can be only a piece of self-complacency for us to deem them like ourselves, since they are far superior physically, as well as in the sentiments which their portraits bespeak; nor do they strike us as intellectually much below us. Their inferiority, if they have any, shows itself here only in possibly defective energy. Wallace combats the theory, founded on their traditions and language, that they came from Malaysia, and certainly shows that, physically and morally, they are the very antipodes of the Malays, while the Malay words in their languages belong to too modern a dialect of Malay to prove anything. But he quite fails to notice that there are other resemblances between the languages of a deeper character, such as the prevalence of disyllabic roots in both, the use of intensive reduplications (*bertanistanisan* is "wept greatly" in Sumatran, *kaukauwa* is "strong" in Fijian), the running of words together into a peculiar kind of compounds (like *vakayanokaukauwataka*, "to cause the body to be strong," in Fijian; *ikinapapaghampas*, "a reason for submitting to severe beating," in the intermediate Tagala language; and in the Malay languages, though the compounds are not so extraordinary, they are formed in the same way, as *mendupa*, "to fumigate with incense," in Sumatran, *itel*, "seen by him" in Dyak), and the use of a particle to introduce statements of fact. It is surprising, too, that Wallace, with his eye for spying out arguments, should not have seen that the late introduction of words from Malaysia, but not from further north in Asia, goes towards showing that the original migration most likely took the same course.

The general reader will be glad to learn from these volumes what an old Darwinian, a Darwinian before Darwin's hypothesis was known, thinks now of that question, and of Neo-Darwinianism, and of the last utterances of Romanes. He will learn, to begin with, what, of course, is common knowledge with the biologists, that variation in reproduction is far commoner and far greater than it was supposed to be when Darwin wrote--so much so that adaptations might be effected, if need be, like lightning (geological lightning, we mean), or, say, in a few centuries; and that the real reason why it is the insensible, and not the large, variations that are efficient in natural selection is, that the changes in the environment are so slow that, a species having been already adapted to one state of its environment, any variation not quite minute would render it less fitted for continuance than none at all. He will also observe that the author draws a strong line between the acceptability of natural selection as the only cause of the differentiation between allied species, which he holds to be as good as proved, and the acceptability of it as the cause of the differentiation between families and higher classes, which he thinks extremely doubtful. He is decidedly disposed to accept the doctrines (or some of the doctrines) of Weismann, although he sometimes slips back into modes of thought which we venture to think inconsistent with those doctrines. Thus he says:

"We may, I think, say that variation is an ultimate fact of nature, and needs no other explanation than a reference to general principles which indicate that it cannot fail to exist. Does any one ask for a reason why no two gravel-stones, or beach-pebbles, or even grains of sand, are absolutely identical in size, shape, surface, color, and composition? When we trace back the complex series of causes and forces that have led to the production of these objects, do we not see that their absolute identity would be more remarkable than their diversity? So, when we consider how infinitely more complex have been the forces that have produced each individual animal or plant, and when we know that no two animals can possibly have been subject to identical conditions throughout the entire course of their development, we see that the perfect identity in the result would be opposed to everything we know of natural agencies."

But if he refers to vicissitudes in the life of the individual animal in question, they have no bearing on variation at birth; while if he refers to vicissitudes of his parents' lives, Weismann often speaks as if such circumstances could have no effect upon the germ-plasm, and often makes the offspring a mathematically exact resultant of the germ-plasms of its parents, in so far as they enter into it, and quite independent of aught else. Wallace, however, does not go so far as positively to deny the transmission of acquired characters; he only maintains that there is no real evidence of such a thing. If there should ultimately turn out to be such evidence, the theory of germ-plasm would, apparently, collapse at once; and Wallace seems to admit that the Darwinian theory must stand or fall with germ-plasm.

We do not mean to discuss Mr. Wallace's socialistic doctrines. We only note that he holds, at once, strongly to the freedom of the individual and to socialistic arrangements, such as the state owning all the land, issuing paper money, etc.

72 (24 January 1901) 72-73: WILLIAM HERSCHEL

William Herschel and His Work.

By James Sime. Charles Scribner's Sons. 8vo, pp. 265.

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

James Sime (1843-1895) was a British author and journalist. He was graduated M.A. from the University of Edinburgh in 1867. In addition to the work on Herschel, here reviewed by Peirce, Sime also wrote biographies of Lessing (1877), Schiller (1882), and Goethe (1888).

Before Herschel the Great, astronomy had consisted almost exclusively in measuring the positions of the stars, at different times, relative to one another and to the earth, and in searching out and testing hypotheses to explain their motions. The most important exception to this statement is that, as early as Ptolemy, at least, some astronomer--we may, for several reasons, guess it was a Babylonian--divided the visible fixed stars into six magnitudes, which were then further subdivided by affixing "greater" and "less" to their ordinal numbers. This is one of the most important facts in the history of science, for the reason that when the light of the stars came to be measured, several millennia later, it was found that there was a mathematical relation between the ordinal numbers of the magnitudes and the intensity of the light sufficiently accurate to form the basis of a mathematical mode of reasoning--a discovery which came almost too late to be of service in astronomy, but which may serve to assure us that direct estimates of differences of sensation of any kind will, under proper precautions, have sufficient objectivity to form the foundation for an exact science. The only other exceptions worth mentioning were Galileo's telescopic discoveries, and a small body of observations relating to the surfaces of the sun and moon. The old astronomy probably never can cease to be the crown of science, as having been the most efficient factor in the development of man's scientific intelligence; but its living importance is already beginning to cede to that of the new astronomy, which applies

photometry, photography, spectroscopy, bolometry, polariscopy, and physics generally, to the study of the stellar universe.

The new generation justly regard Sir William Herschel as the father of the New Astronomy. It was he who first compared the spectra of the different stars; it was he who first made careful and definite observations upon the relative lustre of a great number of stars; it was he who first made a great telescope and showed, in the most masterly manner, all the uses to which it could be put; it was he who first proved the motion of the solar system as a whole; it was he who first showed that observation could discover the structure of the galactic cluster; it was he who first produced evidence of the variation of our climate with that of the frequency of sun-spots; it was the survey begun by him and completed by his son which discovered, and furnished us with tolerably accurate descriptions of by far the greater part of all the nebulæ known to-day; it was he who showed how to apply those observations to speculation upon the evolution of the universe; it was he who first undertook to survey the entire heavens systematically and with practicable thoroughness; it was he who first taught man to contemplate the heavens with an eye of discernment.

The greatness of the man and his enormous originality are proved by the fact that it is only within the last third of the last century that astronomers generally have recognized the importance of his work; and we have to thank many of his contemporaries in the astronomical world for demonstrating how far he shot above them by kindly recording, for the benefit of posterity, their own assified [[sic]] opinions of his results. The man who to-day has spent a lifetime in elaborating an exacter or wiser method in some branch of science still in the hands of fogies (as such branches there still are) will find a warming cordial in those contemporary criticisms of William Herschel.

One may be sure that a man of such achievement was a wonderfully real personality, with more life in him than other men, who seem mere shadows by his side. It may be doubted whether, among all his contemporaries, there was any except Napoleon Bonaparte to be compared with him in force of personality. He would have made the most splendid subject for a biographer of the first order, were it not that the greater part of his waking hours during his active years were spent with his eye glued to the telescope, or with his hands to the grinding-tool, so that his sister had to put the food into his mouth. What was his origin? Mr. Sime, who is one of that great tribe of biographers who are a little timid about the truth, tells us that his family was a "sturdy Protestant stock"; but it is easy to recognize in the name, from the Old High German *hêr*, "superior," one of those magnificent appellatives which are affected by our Semitic cousins; and when we find that grandfather Abraham Herschel begot Isaac, who begot Jacob, Sir William's eldest brother, we hardly need to be told more. The family, however, became Protestant, and emigrated from Moravia to Saxony, in consequence of this change of religion. Sir William's father, Isaac, was nothing but a poor and prolific Capellmeister of the regiment of Royal Hanoverian Guards; and his wife was the simplest of Hanoverian peasants, unable to write a letter to her husband, and energetically opposed to all learning, down to the very multiplication-table, as a most dangerous and unpractical thing, subversive of orderly life. The father, however, was a man of ability and breadth, who would often sit up to the small hours in conversation with Friedrich Wilhelm, the little sister in bed in the room, catching from time to time the names of Leibniz and Euler. This father was also a born musician, as were all (except possibly one) of his children, and most of their posterity since. William was a very clever composer, and lacked nothing of having an artistic nature, except those defects of character and of temper which are understood to belong to the genius. Literary capacity distinctly appears in all his writings. He learned languages with great facility, being perfect in his French and English, without any regular schooling, and good in Italian and Latin. That he should have expressed a dislike for poetry on account of its untruthfulness, only goes to place him in the category of scientific minds of poetical tendencies, all of whom, from Plato down, have been more keenly sensible than other men of the dangers of poetry. Poets always liked him; and the very notes of observations which he would call out in dictation to his sister within, as he would stand at his telescope outside, with the thermometer perhaps toward zero, and his person carefully brought as near as possible to the temperature of the atmosphere, have, as Professor Holden has shown, their poetical breaks.

Capellmeister Isaac of course found Wilhelm a place in his band, and at the age of seventeen, about the beginning of 1756, they first went to England with the regiment. At the gloomy opening of the campaign of 1757, they were ordered into the field. The father's health had been pretty well broken in the former war with France (he had been at Fontenoy), and now that same Duke of Cumberland, again their commander, was defeated anew at Hastembeck. The mother, at this, insisted upon her "practical" view of the proper thing to be done, which was, that William should desert. This he accordingly did, and went to England so hurriedly that he was not even able to carry anything with him; and when his mother dispatched his things after him, she did not see fit to include his precious Locke's 'Essay concerning Human Understanding.' So there was the boy on a foreign shore, without acquaintances, almost without a penny, but with such a "go" in him that within ten years he occupied the lucrative post of organist of the Octagon Chapel in the fashionable city of Bath, conducted all the concerts at the rooms and the theatre, besides having a separate orchestra of his own of a hundred pieces, was overwhelmed with pupils, and, after seventeen hours of work daily, would go to bed to unbend his mind with Smith's 'Harmonics' or Maclaurin's 'Fluxions.'

Smith's 'Harmonics' led him to Smith's 'Optics,' and, finding his time hang heavy on his hands after the day's work would be done, he took to making himself telescopes in order that he might see what no mortal had seen before. Meantime, he was doing everything for the family in Hanover, and had found time, himself, to make the grand tour. At any rate, he had been as far as Italy. His elder brother, Jacob, was an eminent musician; but the other four were looked after by William. Sophia's five sons were provided with places as musicians at the English court. Another brother, Alexander, though an energetic man and good musician, lived much in William's house, and was pensioned by him. The remaining brother,

Dietrich, also a musician, seems to have been almost supported by William, and received £2,000 by his will. The other sister, Carolina became William's astronomical assistant and a distinguished astronomer herself. He himself received from the astronomer-king, George III., for whom a major planet had been named, the first to be discovered in historic times, the munificent pension of £200, along with his pardon; and he bequeathed to his son, Sir John, two estates and about thirty thousand pounds. His lady was residuary legatee. This was pretty well done, for a man who had hardly given himself time to look nearer earth than the orbit of the Georgium Sidus. One of the most powerful intellects that the history of human science can show, with a musical and artistic temperament sufficient to give him a place in the history of that art, an administrator of such ability as to raise him from something like twenty shillings, with which he landed in England, to positive wealth by his own unaided efforts, he was certainly as well-rounded and well-vitalized a reality as any man need be, even in the age of the French Revolution.

Mr. Sime's biography is rather confused. It does not compare for an instant with another life of the same hero, published by the same house twenty years ago, which was a perfect gem of literary judgment and skill, and remarkable for the justice and knowledge of its scientific appreciations, being in fact the work of a leading astronomer, the Chevalier Holden--to call him by a title which, it is needless to say, he does not use. Mr. Sime has omitted some things which the earlier writer did not fail to make clear, has inserted a few items which his predecessor seems to have rejected as distracting details, has corrected one error, that Herschel's degree of LL.D. came from Oxford, though really from Edinburgh, and has made a nice padding of irrelevant matter, which is not bad reading when it does not too much embroil the essential facts. In this way, he has contrived to double the "reading-matter," very much at the expense of the literary quality of his book. We miss the charming portraits of the earlier volume.

72 (24 January 1901) 76: Webster's International Dictionary of the English Language, . . .

under the Supervision of Noah Porter, D.D., LL.D. To which is now added A Supplement of Twenty-five Thousand Words and Phrases.

W. T. Harris, Ph.D., LL.D., editor-in-chief. Springfield, Mass.: G. & C. Merriam Co. 1900.

CSP, identification: Haskell, *Index to* The Nation. See also: Burks, *Bibliography*; Fisch and Haskell, *Additions to Cohen's Bibliography*; MS L 159.166.

In addition to his duties as editor of this dictionary supplement, William Torrey Harris (1835-1909) was also a sociologist, educator, philosopher, and author. He had begun his advanced education at Yale, but left after two and one-half years. In 1858, Harris became an assistant teacher in the St. Louis public schools system, where he taught for twenty-three years. He served as superintendent of schools there from 1867 until 1880. Today, Harris is remembered primarily as the founder of the *Journal of Speculative Philosophy* (1867), although he was successful in other endeavors as well.

— 24 —

Wide currency has been given to the fact that Dr. Murray has said that perhaps Webster's International Dictionary is the best of one-volume English dictionaries. Surely, no living man's opinion upon such a question can carry so much weight as Dr. Murray's, especially to one who has read the whole lecture in which he said this, and has noted the spirit of giving each work its full due which pervades that most interesting delivery. Considering how long it is since the quarto Worcester was last revised, that the Standard (which, at any rate, contains some peculiar information that will always give it an historical interest) was probably only known to Dr. Murray as published in two volumes, that Stormonth is a comparatively small book, and that all the other one-volume English dictionaries are abridgments, the "perhaps" of Dr. Murray's utterance is the only thing about it that could surprise anybody. The International now appears with a supplement of twenty-five thousand words, an addition of one-seventh part to the bulk of the former vocabulary. These are words which have come into importance during the last ten years. Their multitude measures our progress during that period. A full half of them belong to the physical sciences, and of these nine out of ten to biology, including medicine. There must be five or six thousand botanical terms, alone, in the supplement. About a tenth or twelfth of the additions are words of local use. Probably not five per cent of them are good literary words. There are near a thousand colloquial and slang expressions. Spanish and French words, terms of art, terms of theology, philosophy, and bibliography make up the chief of the remainder.

We have taken the trouble to test the work by making a number of lists of twenty-five words (each list containing words in a different branch) that have acquired importance during the last ten years. We have then looked these out, both in the body of the work and in the supplement. This examination did not extend to literary English (because we presume that a person who wants information about such words would go to a much larger dictionary), but was chiefly confined to scientific terms. The result was to show us that the biology, and especially the general evolutionary biology, and the botany, had been extraordinarily well done, both in the main body of the work and in the supplement, and most of the other branches were found very well done in the main work, and, for the most part, fairly well kept up in the supplement. Certainly, the more philosophical sciences, mathematics, logic, metaphysics, and psychology, are the least satisfactory. Both the old editor-in-chief and the new one, the Hon. W. T. Harris, Ph.D., LL.D., were philosophers mainly occupied with obsolescent systems of thought.

Of course, such a work will inevitably be open to just criticism in thousands of places. Many faults might have been set right at moderate expense, by small alterations in the plates. Why, for example, should a list of chemical elements be put before the reader which not only omits all the new elements, the interest of most of which is uncommonly great--and some of these are not even given in the supplement--but also includes sundry pretended elements that never had very strong claims to the title, and are now known not to be such? Such names ought to be in the vocabulary, but not in a list of elements.

It is popularly supposed, we believe, that the great effort of a dictionary-maker is to get together as many words as possible. That probably was true at an early stage of the art, but to-day his great struggle--especially if it be a one-volume dictionary that he is making--is to keep his work within its prescribed bounds. A dictionary, however, is a work which must continue to be sufficient for many years; and, for that reason, the head of each department should be a man thoroughly imbued with the spirit of the coming developments in that department. If space is to be made, it should be done by striking out matters of detail; but there should be a disposition to welcome all words which signify new ideas that are seriously pressing for recognition in each branch. To that end, the editor-in-chief should be a man of the most modern and progressive spirit, always impressing the specialists with this view of what is wanted. It would be easy to show that this has not, in all cases, been done in the present instance, and that several departments of the dictionary have suffered in consequence of its not having been done. Nevertheless, the whole is a magnificent work of the greatest every-day utility.

72 (31 January 1901) 96-97: SHAFTESBURY

The Life, Unpublished Letters, and Philosophical Regimen of Anthony Earl of Shaftesbury, Author of the 'Characteristicks.'

Edited by Benjamin Rand. London: Swan Sonnenschein & Co.; New York: Macmillan. 1900. 8vo, pp. xxxi, 535.

Characteristics of Men, Manners, Opinions, Times, etc.

By the Rt. Hon. Anthony Earl of Shaftesbury. Edited, with an Introduction and Notes, by John M. Robertson. New York: E. P. Dutton & Co. 1900. 2 vols. 8vo, Pp. xlix + 338, viii + 275.

CSP, identification: Haskell, *Index to* The Nation. See also: Burks, *Bibliography*; *List of Articles*; MSL 159.169; MS 1436 (draft).

Shaftesbury, the third of that title and moralist, not only was educated and reared upon philosophical principles, but was veritably bred philosophically. For, his sire being about sixteen years old, the grandfather, that extraordinary man, Ashley Cooper, then Lord Ashley, dispatched the philosopher Locke into the northern counties to select a mother for the future head of his house. A great fortune was not required, but she was to be of "good blood, good person and consitution [[sic]], and, above all, good education and a character as remote as possible from that of a court or town-bred lady." She was found in the person of a daughter of the Earl (afterwards Duke) of Rutland. The marriage took place September 22, 1669, and the moralist was born February 26, 1671, his father being then nineteen years old. He was immediately taken in charge by his grandfather, by whom his education was intrusted to the absolute direction of Locke. The result of this curious experiment was that the scion turned out to be a man of weak constitution, but strong character, a perfect Whig, a highly accomplished gentleman, a forcible writer. But his master passion was a love of virtue, as taught by Epictetus and Marcus Aurelius.

Dr. Rand has discovered, among the Shaftesbury papers in the Record Office, two note-books filled with meditations by the moralist for his own edification, and gives them here apparently complete. They fill 272 pages of the volume. Dr. Rand has entitled this matter "The Philosophical Regimen," but Shaftesbury himself headed it {Askemata} *i. e.*, Training Exercises. Words cognate with this were commonly used in Greek by those who adhered to the Socratic opinion that virtue is a thing to be learned. But Shaftesbury, broadly dividing all ancient philosophy into that of stoical and that of Epicurean color, no doubt wrote this word at the head of his meditations as a sort of general stoical confession; and Dr. Rand pronounces that the Phrygian "slave, the Roman emperor, and the English nobleman must abide the three great exponents of stoical philosophy." This dictum ought certainly to be modified; for the first two are not exponents of stoical philosophy at all; they are simply stoical moralists. They rarely touch upon the philosophy which made so large a part of stoicism; and where they do so, they are as often as not heretical. As for Shaftesbury, he accepts, of course, to begin the account, all those stoical ideas which have become assimilated into the common sense of the modern European. His usual method of reasoning by means of divisions and reductions to the absurd is also in the stoical style. But to the rest of the elaborate stoical logic he makes little allusion, and that little is rather depreciatory. Of that epistemological pragmatism which is near the root of stoicism, he shows, neither in the 'Characteristicks' nor in these new Ascemata, the smallest comprehension; but that is not surprising to us when we have seen the best historians of ancient philosophy puzzled and led astray by the doctrine.

To the germinal conviction from which stoicism springs, that the only end of man lies in action, and that knowledge, as such is an idle accomplishment, Shaftesbury may be said to be more faithful than Zeno and Chrysippus ever were. Questions that excited the deepest and liveliest interest under the Poecile, even that of a future life, are dismissed by him with a cold shoulder and the question, How does this concern me? The same spirit manifests itself in his characteristically stoical taste for allegory and emblems; and, notwithstanding his long studies of art in Italy, the now published letters show him attaching great importance to the vignettes that ornament the 'Characteristicks,' which, though pretty, are the flattest of farfetched symbols, betraying a heart that, even after long familiarity with art, can value it only as a means of calling attention to homilies. It is no wonder that we find in such a writer few traces of the materialism of the stoics or of their other purely metaphysical opinions, beyond those that have passed into the common traditional ways of thinking. Among those may be reckoned his conception of God, and the particular shade of optimism which is symbolized by the cobweb in the vignette to the second volume of the 'Characteristicks,' showing how flies have been created for the benefit of spiders. In regard to the freedom of the will, his position appears to be that of orthodox stoicism.

Just as his too sincere acceptance of the vital principle of stoicism makes him a bit of a heretic among stoical metaphysicians, so his thorough acceptance of the

great maxim of stoic morals--Follow Nature--causes him to diverge considerably from stoical apathy. The newly discovered Ascemata are stuffed full of quotations from the ancients, the great majority of them from three men whom Shaftesbury seems to have regarded as the great depositaries of the art of virtue. Two of them are Epictetus and Marcus Aurelius: the third is a Latin author. The reader, we will wager, is jumping to the conclusion that it is Seneca. Not exactly, it is that stern moralist Qunitus Flaccus. On the sole occasion when Shaftesbury's disciplined intellect plays truant and goes chasing the butterfly knowledge for knowledge's sake, it is in seeking to prove to the Lockian logician Le Clerc that Horace was first and last a consistent stoic, and that it was only for a brief intermediate period that he was led away by Mæcenas towards Epicureanism. One sadly fears that the poet's gentlemanly grace has quite blinded and well-nigh seduced the innocent Shaftesbury. At any rate, what Shaftesbury understands by following nature is not at all the suppression of all emotion, but just such a degree of emotional lukewarmness and good-humored composure as the hedonistic Horace loved to parade. For all that, human nature is not capable of greater earnestness than Shaftesbury's in his quest after the highest virtue. We feel it in the style of the 'Characteristicks,' and it is proved to demonstration by the newly published book. Thus, we find that Shaftesbury had developed a kind of stoicism of his own. It was not a particularly profound kind, philosophically considered; yet the new publication is likely to prompt fresh studies of stoicism, to a better comprehension of what it was in the school at Athens, and also of what development it is capable in a modern intellectual climate.

The letters of Shaftesbury which Dr. Rand has printed fill as many pages as the Ascemata. Many of them are of interest in reference to his own philosophy, especially in relation to that of Leibniz. His regard for his master, Locke, led him to conceal his pretty thorough dissent from Locke's great masterpiece; and it was only toward the close of his life, in 1709, that he confided the true state of things to one person in a letter here given. A number of letters addressed to Locke show Shaftesbury's personal veneration and affection for the philosopher. There are also many political letters which show his inherited Whiggism, coming as near to a passion as stoicism would permit, and the effective blows that he was able to strike for true liberty. Other more personal letters are by no means the least significant in studying his philosophy, and go to increase our esteem for his conception of morals. The same is true of his letters to his proteges. But those to Thomas Micklethwaite are of interest for other reasons.

Dr. Rand has done his work very diligently. To the innumerable Greek and Latin passages he has appended translations, and has usually stated where they are found. It is to be regretted that he has not supplied the volume with an index. The portrait is a reproduction of a worn impression of the frontispiece to the 'Characteristicks.'

Messrs. E. P. Dutton & Co. present us with a very beautiful reprint of the 'Characteristicks,' with a readable and, on the whole, instructive introduction by Mr. J. M. Robertson. It is the first edition that has appeared for over a century. The reason is obvious. The number of persons who care to study an exploded theory of

morals is small; and those few will prefer Hutcheson's exposition. Or, if they must have Shaftesbury, there were four old editions, superbly printed, and often magnificently bound, extremely accurate, and with the vignettes and other minutiæ which were so important in Shaftesbury's own estimation; and these are still easily procurable at low prices. It is true that all the editions before the fifth omit the "Letter concerning Design," which Shaftesbury intended should be included, and which (all the more because of its philistinism) throws an important light upon his ways of thinking. But then, this is omitted in the new edition as well. Mr. Robertson's introduction contains a number of statements and expressions which we may believe he would have modified had he been acquainted with the contents of Mr. Rand's volume; such, for example, as the notion that the least metaphysical of moralists was chiefly influenced by the most metaphysical, Spinoza. But, that corrective being now at hand, the value of Mr. Robertson's introduction depends upon its positive merits, not on its errors. Its discussion of Shaftesbury from a literary point of view is fairly good, not fully doing justice to his extraordinary earnestness, emphasizing a little too much his fashionableness, his earldom-ness (though this is somewhat obliterated by the modernization of Shaftesbury's peculiar spellings and other little idiosyncrasies which disappear in the reprint), but, nevertheless, bringing to light a number of points which the student of Shaftesbury will be glad to have so clearly put before him; and, on the whole, well characterizing the celebrated characterist upon his literary side.

72 (28 March 1901) 254: NOTES

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

'Elements of Statistics,' by Arthur L. Bowley (P. S. King & Son), is clearly intended for the advanced student, or for one intending to prepare for an examination for entrance into the British public service. The work of Bertillon, however many its merits, was written with the French administrative system in view, and could be of little service outside of France as an introduction to statistics. Mr. Bowley's work suffers from the same limitations, and the Labor Department of the Board of Trade is the main objective of the leading chapters. The schedules of that Department are analyzed, the resulting tables criticised, and most of the examples are taken from reports on wages. This is unfortunate, for wage statistics are notoriously elusive, and even the best are open to grave objections. More than the inherent difficulties in the way of obtaining good results stands the serious neglect shown to this branch of inquiry in the United States, where no two States pursue the same methods. It is to be feared, therefore, that Mr. Bowley's studies will not receive the attention they merit. Upon the subject of averages, index-numbers, and the theory of probability applied to statistics the book is full, and shows that the author is familiar with the most recent work of Pearson, Edgeworth, and others in these lines of investigation. The numerous diagrams included are suggestive and valuable. Yet it is not a book for the beginner, however necessary to the student.

72 (28 March 1901) 258-259: By Land and Sea.

By the Rev. John M. Bacon, A.M., F.R.A.S. London: Isbister & Co.; Philadelphia: J. B. Lippincott Company. 1901. 8vo, pp. 275. Four photographic illustrations.

CSP, identification: Haskell, *Index to* The Nation. See also: Burks, *Bibliography*; *List of Articles*; MS 1437 (draft).

John Mackenzie Bacon (1846-1904) was a scientific lecturer and aeronaut. He graduated from Trinity College, Cambridge, in 1870 and was ordained the same year. Bacon made his first balloon ascent in 1888 and was noted for his successful experimentation from balloons in acoustics and wireless telegraphy. By far the most daring event of Bacon's career was his crossing of the Irish Channel by balloon in November 1902.

Reviewers who are addicted to the practice of reading books through are not often sorry when they come to *l'envoi*. Yet it does sometimes happen; it did to one reviewer in the case of Mr. Bacon's volume. Somebody was wondering the other day why ballooning had not become a more generally favorite sport with Englishmen; for surely, said he, nothing could be more dangerous or more expensive. It has, however, several disadvantages; there is no din; there is not the least sense of motion, nor even a zephyr; and there is very little to see, especially in England. For when the balloonist looks down upon anything but clouds, the landscape is on most days thickly veiled with haze.

Mr. Bacon has usually had a scientific object in making his ascensions. Sometimes it was to count meteors, or make some of the other astronomical observations for which a solid support is not necessary; sometimes it was for wireless telegraphy, but predominantly it was for acoustical research. This is the natural way in which science advances; a man first acquires some peculiar facilities for making a certain class of observations, and then he applies those facilities as best he can. Mr. Bacon gives us many curious and novel observations about sounds, and connects them in a way which even the professional physicist will find instructive. Still, one can hardly say that he has manifested a very distinguished genius for this class of investigations, the great difficultness of which is well recognized. His chief result is that, so far as he can find, no echoes are given back from air, contrary to the conclusions of Tyndall. But he mistakes the nature of the problem. Theory leaves no doubt that acoustical disturbances must be reflected from air; the question is, What are the limits of the circumstances under which they become audible, and how far can reflection from air, with the consequent interferences, account for the areas of inaudibility of fog-horns, etc.? Sounding horns from a balloon is, no doubt, a great lark, but one could not expect the perpendicular reflection of a sound from a stratum of air to be audible, especially of denser air. Nature reveals her secrets only to her most earnest worshippers, and although Mr. Bacon has been sufficiently interested in his acoustical operations to spend several days, "marooned," as he calls it, out in the German Ocean, in the Maplin Lighthouse, which stands on an iron scaffold, and has passed one night in making experiments at the top of St. Paul's dome, and another on the churchtower of Thatcham, yet it is easy to see that the animating motive of his aeronautics, as it will be that of his reader, has been the pure fun of the thing.

God defend us for blaming him, or depreciating his book. If he has not the scientific genius of a Faraday, he has another gift that is rare and serviceable to his fellowmen, for every ascent is described so simply and unsensationally, and yet so gayly and so vividly in all its details, that one might wager a pretty penny that any given reader of it, supposing the book remains at his elbow and that leisure is granted, will not leave it long without reading it again. Sober discussions of curious phenomena alternate with descriptions of ascents in each of which there cannot fail to be something more or less exciting. For example, chapter iii. describes a balloon race.

"Our opponent began by forcing the running, and from the first with obvious success.... To set, however, against the gain in speed acquired by flying high, there was a material loss entailed by expenditure of ballast.... The next move was played simply by lapse of time, without manœuvring, and resulted in a slow and steady rise and consequent gain on our part, while the opponent, reaching the culminating point due to her leap into space, suffered now from the loss of gas incurred, and was presently sinking below us.... From an early stage of the race I am now describing it was evident that our balloon was the more heavily, in fact too heavily, loaded, by the dead weight of passengers. We had the smaller quantity of sand at disposal, and, in consequence, our defeat was only averted by artifice.... Two hours had passed and we were flying at 3,000 feet.... At this point we saw our rival still distancing us.... We began settling down.... To say truth, there was little more manœuvring left us, for we had but one bag of ballast remaining, and this we were already paying out by driblets to save collapse. Down we swooped, fast and faster, in spite of the constant slender flow of sand, till at last we stood ready to meet a seemingly inevitable plunge into a long level pasture. Quickly we discharged the whole remaining ballast, and so abode the issue. It was touch and go. Diving to within three feet of the ground, our balloon hesitated, halted, and then kept level, while we coasted the whole length of the paddock with cows in stolid wonderment lying right and left of us. Then we righted, the impetus of rapid descent was spent, and we lifted, shooting up once more in right good earnest. Down plunged fields and woodland into such a fast-deepening gulf that the aneroids were promptly consulted, and showed the index-hand running backwards without stop or stay till near 6,000 feet was noted and we were still rising. The pace had been great. We were now more than a mile high, and we looked down and around. There, already behind us, lay our opponent sprawling on a grass field, a crowd of people round her, giving us an easy win after all. They had evidently thought us beat and so came down."

This was badly managed on both sides. The opponent had cast out ballast during the first rise, a most senseless proceeding. He might better never have taken it. The result was that, soon after that, Mr. Bacon's balloon was gaining. His party had, therefore, only to stay up in order to win; and they ought to have stayed as high as possible, because the wind is swifter higher up. Yet, when they were descending and, of course, "fast and faster," they payed out ballast only in a slender flow of sand. They ought, as soon as they began to descend, to have thrown out

enough to make them lighter than the air. They waited until it was only by the merest luck that they did not strike anything. Still, they went up 6,000 feet. They would have gone 3,000 feet higher, at least, and have got the advantage of a stronger wind, had they done so earlier. As it was, the opponent might have stayed up long enough to beat them; but he never could have done so after his first folly, had Mr. Bacon's party discharged their ballast at the right moment.

Although Mr. Bacon records in this volume seventeen balloon-voyages made by him, and we may suppose he has made others, he still labors under the idea that the entrance of a balloon into colder air at once causes it to descend; and to account for the rising of a thistledown through cold air he thinks it necessary to suppose upward "rivulets" of warm air. At one time, he thinks, "we must have entered a cold current unknown to ourselves," because the balloon began to sink. Another time, on emerging to the upper side of an evaporating cloud before sunrise, it is the "chill" which he feels, and not the facts that the aqueous vapor is much lighter than air and that the free radiation would probably cause dew to be deposited on the balloon, that retards its ascent. When the sun sets, it is, according to him, the cooling of the air, not the cooling of the balloon, that makes it come down, although Glaisher had demonstrated that sunset does not particularly cool the higher air. And many other passages are to like effect. Physicists will know what to think of them; and they do not make the book less entertaining.

72 (13 June 1901) 479-480: Essays in Illustration of the Action of Astral Gravitation in Natural Phenomena.

By William Leighton Jordan, F.R.G.S., etc. Longmans, Green & Co. 1900. 8vo, pp. 192, with two folding tables and a plate.

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

Mr. Jordan's book is charmingly printed on excellent paper, and is written in good taste. He admits that, "from a theoretical point of view, the argument is unassailable" that a body will not alter its velocity unless acted upon by an extraneous force; but he declares that the opinion which prevailed before Galileo is "equally reasonable as pure theory," and is in better accord with physical phenomena. He is mistaken, however, as to what the old opinion was. It really was that a body left to itself would instantly come to rest, so that what made a slung stone move on, after it left the hand, was the motion of the air; and what kept the air in motion was the motion of the stone. Now, as a matter of "pure theory," this was not all that could be desired. Mr. Jordan, on the other hand, thinks that a body left to itself would gradually lose velocity till it came to rest. Furthermore, "it is, I say, matter of fact, and not abstract argument, that must be appealed to for a decision." Being in that state of opinion, one might expect that he would make experiments to ascertain according to what law the velocity is diminished, and, that law having been made out, to determine how long it takes a body to come to a state of absolute rest. Since the solar system seems to be moving through space

many times faster than the fastest locomotive, a body at absolute rest would fly off at a relatively tremendous rate; so that, if there be such a tendency "from a practical point of view," one would think there ought to be some way of utilizing it. But Mr. Jordan in one place confesses that he has not mathematics enough to solve a certain problem which, in fact, merely involves a simple application of the calculus; and he seems not to have a sufficiently mathematical mind to perceive that his theory calls for the formulation of a law and for the measurement of a constant. This does not, however, in the least prevent his discussing problems in dynamics of considerable difficulty.

If a common physicist is asked why he accepts the first law of motion, he will reply that the pendulum of an ordinary astronomical clock, detached from the escapement, will, in air, swing for several hours; that, under such a partial vacuum as an ordinary air-pump will furnish without much labor, it will swing many times longer; and that observation shows that the departure from the usual theory is too small to be detected in that way. No experiments have ever been made in high vacua; but the earth continues to rotate without any retardation or acceleration that any clocks hitherto made can discover, and thus the retardation or acceleration, if there be any, of the motion of an isolated body, must be so small that it is best assumed to be zero, until we meet with some facts which point to a different value. If, now, we turn to Mr. Jordan, and demand his explanation of the earth's rotation, he inquires of us whether we do not accept the proposition that every particle of matter exerts a gravitation upon every other inversely proportional to the square of the distance; and on our granting this, he adds, "Very well; it is the gravitation of the fixed stars--astral gravitation--which keeps the earth rotating." Were we rashly to produce a dynamical demonstration, which would be easy enough, that astral gravitation cannot produce any such effect, he would not pretend to understand it, but would inquire of us whether it did not assume the truth of the first law of motion, in which case it would beg the question; and we should be obliged to admit the entire justice of this. Mr. Jordan has us at an advantage by making no pretension to being a mathematician; for it is plain that if the first law of motion were to be modified, that would involve the complete demolition of natural philosophy down to the ground, so that it would have to be rebuilt in some radically new way. Now if Mr. Jordan were a mathematician, we could ask him how he proposed to do that rebuilding. But, as it is, he would simply tell us that it was our business, not his, to find that out. Poincaré, the Lorraine mathematician, says that since the unknown exceeds the known, there are more unknown quantities in physics than equations to be satisfied; so that we can assign to any one of the former--as, for instance, the rate of variation of the velocity of a body that is left to itself--any value we choose. If this were intended to show that we must in natural philosophy use some different method of reasoning from mathematical deduction, it would do very well.

If Mr. Jordan had known what eminent thinkers are virtually with him in ascribing the earth's continued rotation to the influence of the fixed stars, he probably would have been better pleased than they would be by the alliance. The facts deserve to be set forth. Although the first law of motion was discovered by Galileo, yet the accurate conception of it seems to have been reserved for Newton. The law is, that if the motion of a body is quite independent of other bodies, the velocity remains unchanged in amount and direction. But this is mere verbiage unless the motion of a body can, or at least could, without violation of any general principle of nature, really be independent of other bodies. The law assumes, then, that motion belongs to a body though no other exists, and continues to have the same definite amount and direction; a notion that drove Newton to assert that Space is a real entity. But German metaphysicians refuse to admit that Space is a real entity, no matter what the facts of observation may be. Some of them maintain that there is a certain body, called the Alpha, wherever it may be, and that the motion of a body left to itself simply continues to be the same relatively to that Alpha. They forget that no abstract definition of a straight path is possible; so that, in order to define how the ordinary body moves, it would be necessary to make the Alpha pervade all space, when the difference between it and Newton's entitative space would be reduced to its name being "Alpha," instead of "Real Space." But Ernest Mach, who is distinguished among Germans as an extra clear head, substitutes for the Alpha the aggregate of all the masses in the universe; and since the mass of bodies near us is perfectly insignificant in comparison with the fixed stars, his view amounts substantially to saying that the first law of motion is, that a body left to itself continues to move in the same way relatively to the fixed stars.

Now, it is an easy corollary from the first law of motion, that if a swinging pendulum is free to change its plane of rotation, it will, nevertheless, continue to swing in the same plane; and if its point of support is displaced (with infinite gentleness), it will continue to swing in a parallel plane. Hence, when Foucault hung up a pendulum, and found that, left to swing freely, its plane of rotation turned relatively to terrestrial objects at a certain rate (which would at the pole give one return every twelve hours, or a complete revolution in twenty-four hours), the Newtonians exclaimed that this proved the absolute motion of rotation of the earth. But no, says Mach, absolute rotation is simple nonsense (that is to say, his metaphysical system has no room for it), and what you call absolute motion, in your first law of motion, is really motion relatively to the fixed stars, so that the Foucault phenomenon is due to the relation of the earth to the fixed stars. Now that, we maintain, is the same thing as to say, with Mr. Jordan, that it is the fixed stars that keep the earth in rotation. If it be true that the relation of a body to the most distant objects is so much more important than its relations to near ones, one might be inclined to question whether, on the whole, spatial relations were as significant as we have been accustomed to think them. It ought, at any rate, to encourage the telepathists.

We will not recommend anybody to purchase this book; but if any young student of dynamics has a copy, he can find an excuse for giving it house-room in the pretty problems, all well known but conveniently collected in the fifth essay, where the author professes to explain them by astral gravitation.

72 (20 June 1901) 497-498: **Le Vocabulaire Philosophique.**

Par Edmond Goblot, Chargé de Cours à l'Université de Caen. Paris: Armand Colin & Cie. 1901. 12mo, pp. 489.

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

Many dictionaries of philosophy have appeared since that of Goclenius was published in 1613. None have been thoroughly good, and none that we have seen have been by any means worthless. The best, hitherto, has been that of Franck, in six volumes, executed upon the cooperative plan, the first publication of which began in 1844. There have, again, been some philosophical dictionaries of limited scope, which perform all that they promise quite to perfection. Such are the 'Index Aristotelicus' of Bonitz, and the index to the old Rome edition of Thomas Aquinas. For a very small book, Schmid's Kantian 'Wörterbuch' can hardly be improved. Another class of philosophical dictionaries is marked chiefly by ignorance and bad judgment, and yet, though most of the labor expended in preparing them has been misapplied, still there has been so much of it that they often prove extremely serviceable. Among dictionaries which are extremely useful, provided one is continually on his guard against their treacherous errors, may be mentioned that of Eisler, published two years ago, which has a text of half a million words. This book, which consists almost wholly of quotations, assumes that the reader reads currently Greek, Latin, Italian, and French, as well as German, but not English.

Every student of philosophy has probably, on some occasion or other, found even so slender a work as that of Fleming convenient. The French volume before us for review is somewhat on the same scale, though not upon the same plan. Since accurate and systematic thinking constitutes pretty much the whole substance of philosophy, it follows that, to read philosophy, it is necessary to have the most precise knowledge of the meanings of its technical terms; and no matter what the student's natural aptitude may be, extensive reading is more indispensable to any degree of competence to consider any question of ultimate good, of right reasoning, or of the general character of the universe, which are the subjects of philosophy, than it is in any other branch of science--unless, perhaps, we should except history and politics. Now the terminology of French philosophy is quite peculiar. Very few French writers distinguish sharply between the essence of a philosophical term and other ideas that are closely associated with it; and those associations are often quite different from our own. For this reason, an English-speaking reader has to make a separate study of the language of French philosophers. The French have now reached that stage of philosophical development in which the ideas of Kant occupy them somewhat largely, though at the time when philosophy flourished in France under Louis Philippe, the name of Kant did not appear in French books, unless now and then for decorative purposes. This makes a new vocabulary adapted to the present state of ideas very welcome to us. It should tell us much about the present state of philosophy in France. Besides the Kantian terms, we find here terms of physiological psychology cutting a great figure. That that science is no part of philosophy is rightly insisted upon by the psychologists themselves. Indeed, at the time James's classic appeared--only a decade back, but it seems an age--the disposition among them was to cut the acquaintance of the metaphysicians altogether. It was just as well to include these terms, for philosophers like to allude to psychology; but of the present volume those words are quite the characteristic feature, and no French writer later than Maine de Biran is so often referred to here as *Pierre* Janet. Political and social philosophy is almost entirely neglected. Ethics and modern metaphysics are equally so. Renouvier is but very slightly referred to in two articles. Modern logic is hardly at all represented. But then we must remember that the volume is small, and anything like a good all-round representation of French philosophy could not be expected. Many of the statements are monstrously inaccurate, but whether or not the work is on that account less typical of the French student of philosophy of to-day, which is the point of view from which an American would value it, we are not prepared positively to say. In short, the book is one of which some use may be made, but it is hardly worthy of so eminently respectable a publishing-house as that which issues it.

73 (25 July 1901) 70: NOTES

CSP, identification: Haskell, *Index to* The Nation. See also: Burks, *Bibliography*; *List of Articles*; MS 1438 (draft).

There is a class of persons whom 'Knowledge, Belief, and Certitude,' by

Frederick Storrs Turner (London: Swan Sonnenshein & Co.; New York: Macmillan), will precisely suit. Let a man, with no intention of seriously studying philosophy or of forming any decided opinions about it, wish principally to kill time, and incidentally to gain from some candid and fair informant so much acquaintance with the doctrines of Riehl, Hegel, Lotze, Wundt, Bradley, Hodgson, and other philosophers whom he sometimes hears praised as can be gained without the inconvenience of his being called upon to do any thinking, or of being importuned with paradoxes, and we do not know what author we could recommend to him so confidently as Mr. Turner. For a young student of the subject, however, no teaching could possibly be worse. As for the trained metaphysician, he may have his reasons for looking into the book; but he will not do so in any hopes of improving his comprehension of epistemology, after the first five minutes. Let nobody attempt this book whom either twaddle irritates, or who attaches the slightest value to his time.

73 (1 August 1901) 95-96: BERKELEY'S WORKS

The Works of George Berkeley, D.D., Formerly Bishop of Cloyne; including his Posthumous Works. With Prefaces, Annotations, Appendices, and an Account of his Life.

By Alexander Campbell Fraser. Oxford: Clarendon Press; New York: H. Frowde. 1901. 4 vols., 8vo, pp. xc. + 527; vi. + 415; vi. + 412; viii. + 611.

CSP, identification: Haskell, *Index to* The Nation. See also: Burks, *Bibliography*; *List of Articles*; MS L 159.179; MS 1439 (draft).

It was a rare event, and truly astonishing, that a man without anything like a transcendent intellectual power should make a decided impression upon the philosophical thought of every country in Europe, such as Mr. Fraser did make by his former edition of Berkeley's works, which appeared in 1871. Berkeley was, there is no need to say, already very celebrated the world over; and in English-speaking countries no young metaphysician failed to read his 'Principles of Human Knowledge' or to talk about his theory of vision. His 'Theory of Vision, Vindicated and Explained,' had reached its second edition in 1860, only one hundred and twenty-seven years after its first publication; but this second edition, a very pretty one, too, had been little read. In Germany, identically the same theory--dressed in modern conceptions, as no intelligent modern reader would fail to dress it for himself--was attributed to Helmholtz, whose real services in the matter were analogous to those of Messrs. Harper & Bros. in 'Harper's Latin Dictionary.' The compartment of the brain in which men stored what little they fancied they knew of Berkeley was their cabinet of bibelots. Fraser's publication, which was not merely an edition, but an exposition by a student burning with the conviction of the present appositeness of Berkeley's method, was a veritable event in the history of European thought. The present edition is not a revision of that other, but quite a new one, and, considered simply as an edition of Berkeley's works, distinctly a better one. Dr. Fraser is now in the eighties, and so in condition to expound the 'Siris,' which breathes all the wisdom of a philosophical and learned old age, with greater insight than he could possess thirty years ago. It ought now to be a happiness to him to find that the generation which has derived from him an impulse into Berkeleyan studies has at last quite gone beyond him in the understanding of Berkeley, in perception of his errors, and in recognition of his effective eminence in philosophy.

Berkeley is, in truth, far better entitled to be considered the father of all modern philosophy than is Kant. It was he, not Kant, who first produced an *Erkenntnisstheorie*, or "principles of human knowledge," which was for the most part correct in its positive assertions. It was he, more than any other single philosopher, who should be regarded as the author of that method of modern "pragmatism"--*i. e.*, the definition, or interpretation, of conceptions by their issues--which equally distinguished the thought of Kant, but which neither philosopher grasped clearly enough to formulate it in general terms.

With two exceptions, we can think of no great factor of Kant's method of attacking a question which is not more or less

— 37 —

emphasized in Berkeley's. One of these two is the doctrine that existence is not a form to be conceived, but a compulsive force to be experienced (which is prominent in Kant's refutations of Berkeley and of the ontological proof that there is a God; and indeed everywhere). This was of British origin: it is the doctrine of Scotus. Indeed, in Kant's thought, generally, there is hardly anything but his architectonic method that is not more in the line of English tradition and development than it is in the German line. Even where he appears least English, he is following Cudworth. There was, undoubtedly, the Leibnizian influence; but, apart from its dogmaticalness and its unclearness, that is not very German, either. One of the greatest weaknesses of Berkeley is shared by Kant in a lesser degree. We mean his Ockhamism, or refusal to acknowledge any being *in futuro*, or any mode of being whatever except that of individual existence. Even the Ockhamist Stuart Mill defines matter as a "permanent possibility" of sensation; but, for the more consistent Ockhamist, Berkeley, possibility is absolute nonentity: material objects must, when men have them not in view, be all along actually present to the Divine mind, or they would collapse into utter nothingness.

Berkeley's importance in psychology is best exhibited by setting down a few dates. It must be borne in mind that the association of ideas had never been lost sight of by students of Aristotle. Thus, the younger Scaliger reports that his father used often to say that the thing he most ardently wished to understand better was the causes of "reminiscence." Now, reminiscence was nothing but the Aristotelian name for the action of the association of ideas. Here is a little chronological table which exhibits in a nutshell more than we could find space otherwise to set down:

1661. Glanville in his 'Vanity of Dogmatizing,' sketched in a word or two what subsequently became Berkeley's theory of vision.

1687. Locke's 'Essay concerning Humane Understanding.'

1688. The 'Entretiens sur la Métaphysique et sur la Religion' of Malebranche, which somewhat develops Glanville's idea.

1709. Berkeley's 'Essay towards a New Theory of Vision.'

1710. Berkeley's 'Principles of Human Knowledge.'

The 'Théodicée' of Leibniz.

1713. Arthur Collier's 'Clavis Universalis,' which was a quite independent development of the same ideas as those of Berkeley's Principles.

1731. Gay's 'Dissertation on the Fundamental Principles of Virtue.' This first put forward the principle of association as the one great law of all mental action, and is, therefore, one of the most epoch-making of works. Yet Gay does not appear in the 'Dictionary of National Biography' nor in Allibone nor in the supplement. His first name is unknown to us. Hartley (who calls him the Rev. Mr. Gay, and tells us that he wrote this anonymous 'Dissertation'), confesses that he had been put upon his line of thought by him. He published another little book on the subject in 1747, two years before Hartley's 'Observations on Man,' but probably after Hartley's 'Conjectural Quædam,' the date of which we do not know.

1732. Wolff's 'Psychologia Empirica.'

1739. Hume's 'Treatise on Human Nature' (first two parts). Hume, who was directly influenced by Berkeley, first clearly distinguishes between association by resemblance and by contiguity.

1749. 'Hartley's Observations on Man,' fully developing the action of association.

1782. Kant's 'Critic of the Pure Reason,' which is psychologically, in some important respects, behind Berkeley.

This table is enough to show that Locke, Berkeley, and Gay ought to be regarded as the three original precursors of modern psychology.

Berkeley must, by all accounts, have been a man of extraordinary eloquence. His inducing Parliament unanimously to grant £20,000 for his Bermuda project is an example of this. His ardor was of the purest; and what he believed, he believed with his whole soul. We cannot, in this, at least, agree with what Fraser says of 'The Theory of Visual Language Vindicated,' that "its blot is a tone of polemical bitterness directed against Shaftesbury." On the contrary, it seems to us that that remark is a striking illustration of the decadence of Christian belief in our days. The courtesy and self-restraint of Berkeley's severe strictures upon the mischief done by Shaftesbury's writings could not easily be paralleled by any utterance of the present generation coming from a man who was deeply in earnest about the evil he attacked. Every stoic, such as Shaftesbury was, was a thorough materialist; and, as such, an atheist to Berkeley's apprehension, whatever he might fancy himself to be. As for the majority of the free-thinkers, Berkeley, who had heard their private conversation, did not think them to be under any such illusion concerning their own position. But a man may easily think that he believes what he does not believe. For example, Berkeley himself, and Fraser for him, cannot admit that an opponent of Berkeley treats him fairly unless he begins by admitting that Berkeley believes in the existence of matter in the sense in which the world at large believes in it. But for an opponent to grant that would obviously be to surrender his whole position. The true question is whether Berkeley has not overlooked certain of the constituents of the ordinary, instinctive notion of matter.

Fraser's own contribution to the development of the Berkeleyan doctrine is sufficiently indicated by the following sentence from his preface: "His Philosophical Works, taken collectively, may encourage those who see in a reasonable *via media* between Omniscience and Nescience the true path of progress under man's inevitable venture of reasonable Faith." To find the development of this idea, one must turn to the author's 'Philosophy of Theism.' Then if one desires Berkeley's works as completely as possible, one will further procure his 'Life and Letters' by Prof. A. C. Fraser. A thorough student of Berkeley will want all that.

Whether for an ordinary reader of philosophy--putting aside the question of price--this edition or that in Bohn's "Philosophical Library," published three years ago, is to be preferred is a delicate question. The text of either is excellent, although neither, we are sorry to say, respects Berkeley's punctuation, which is a part of his style. Probably the Bohn edition is most scrupulously accurate. That it omits such things as the diary in Italy is really no objection. Its most serious omission is the common-place-book of notes for the preparation of the 'Principles.' This is rhetorically interesting; but it throws less light on the development of Berkeley's views than would be expected. The Bohn edition gives the Latin works (of which one, 'De Motu,' is not altogether devoid of importance), only in translation; the

Fraser edition only in the original. Berkeley's Latin has a certain academical elegance; but it is a garb which does not set so comfortably on his thought as a homelier English. In regard to additional matter, each edition has something one regrets to miss in the other. Much more is attempted in this way in the Fraser edition. Nothing is really indispensable but Berkeley's own forcible and persuasive language; and the Fraser notes form sometimes an officious, one had almost said an impertinent, interruption to a philosopher who is quite able to manage the English language for himself. The new life prefixed to the Fraser edition is much fuller and somewhat more accurate than Mr. Arthur Balfour's capital biographical introduction to the other. Neither biographer has suggested that the good bishop's very sudden and very quiet death may--in view of the oceans of tar-water that he was accustomed to swill--have been due to an overdose of carbolic acid. It is quite certain that the Bohn volumes are prettier and lighter and more agreeable to read; but their editor, we feel sure, would concur in the judgment that the new Fraser edition is the most valuable that has yet appeared or is likely to appear, for as long as one can foresee.

73 (1 August 1901) 99-100: Bibliotics;

or the Study of Documents, Determination of the Individual Character of Handwriting, and Detection of Fraud and Forgery: New Methods of Research.

By Persifor Frazer. Third ed. Philadelphia: J. B. Lippincott Co. 1901. 8vo, pp. 266.

CSP, identification: Haskell, *Index to* The Nation. See also: Burks, *Bibliography*; MS L 159.176; MS 1439a(s)(draft).

Persifor Frazer (1844-1909) served as aide in the United States Coast Survey after his graduation from the University of Pennsylvania in 1862. He entered the Union army, and fought at Gettysburg with the Philadelphia City Cavalry. After the war, Frazer was appointed instructor in the department of natural philosophy and chemistry at his alma mater, and was appointed chairman of the same in 1872. Among his discoveries are the cause of the difference of color of the moon by night and by day, and a method of detecting forgeries by use of composite photographs of genuine signatures.

Here we find ourselves plunged into a corner of the fray that has so long been raging all over the field of historical criticism between instinct and systematic logic. "Your instinctive judgments are subjective," cry the logical party, "and your pretensions to detecting what common men cannot detect, or to applying what you call the principles of common sense to history, are proved to be charlatanry by your all coming to different conclusions, without any accord but that of fashion." "Your wooden rules never penetrate the facts," the lauders of instinct retort, "and your notion that one can form any just judgment concerning human conduct, in any department, without the exercise of tact and of subtle feeling, is worthy of the savans of Laputa." Dr. Frazer adds his weight to the logicians' side in the discussion of that branch of the science of diplomatics which seeks to identify individual handwritings.

The old-fashioned experts in chirography, the Chabots and the like, set out from the fact that we all of us learn to recognize at a glance, we scarce know how,

yet with entire confidence, almost as many handwritings as faces. The real certainty of our identifications, even of faces, falls short of our confidence in them. In the case of writing, the expert often meets with doubt, either his own or on the part of others whom he wishes to convince; and in such a case, the old practice is to observe closely what appear to be salient features, such as the ways of forming single letters. Juries might sometimes be convinced in that way, but it is a somewhat dubious proceeding. There is all the difference in the world between the trustworthiness of an emphatic instinctive judgment, made unreflectively, and an alloy of instinct with semi-scientific, semi-instinctive testing. When it comes to applying tests at all, the only security lies in making the testing process thoroughly scientific. Now the first rule of scientific induction is that it ought to be planned and performed under the guidance of mathematical considerations, and to that end it ought to be rendered quantitative wherever it can be so rendered. Now, as Dr. Frazer shows, quantitative tests can be applied to handwriting in such profusion that it ought to be possible to develop a method which should hopelessly distance every attempt at imitation.

But to develop such a method will require an arduous scientific investigation. It is by no means a thing which can be got up for any case that may occur; though cases may happen to arise in which, owing to peculiar circumstances, mathematical tests may readily be devised. Even after the scientific method shall have been developed, another serious study must be made of the art of presenting the proofs so that a jury can fairly weigh them. Dr. Frazer has broken ground for the road which ought to be built. He himself cannot regard, and we think does not pretend, that what he has accomplished is anything more than a promising beginning. But even the comparatively modest claims which he does make somewhat outrun what can be granted. For example, taking Twistleton's book on the comparison of the handwriting of Junius with that of Sir Philip Francis, he measures the ratio of height to length of the word "more" as written thirty-six times by each penman, and also certain parts of the word "Woodfall," as written once by each; and, having made the comparisons, he heedlessly remarks, "The conclusion from the results of the last as well as from the preceding is that the same penman wrote the letters of Sir Philip Francis and of Junius." But even granting the methods of comparison to be unexceptionable, this is a very roomy sort of conclusion. All that is proved is, that the general shape of the word "more" was the same as either wrote it, and that, as each of the two once wrote "Woodfall," four different proportions were considerably alike. We should want to know what percentage of other contemporary handwritings had the same characters before pushing our inference further.

But, looking in detail at the comparisons, we find other hasty statements. Thus, in regard to the word "more," Dr. Frazer says, "It appears that in Francis's own writing nineteen, in Junius's sixteen, of these ratios differ by less than eight per cent. from the standard." But Dr. Frazer's measures do not prove in the least how uniformly either writer wrote. It can be shown mathematically that the discrepancies between the ratios of the height to the length of the word "more," in the single instances measured, are almost entirely due to the fact that the measures

of height were only made to half divisions of a scale of fortieths of an inch, and the average height measured was in one case only 4, in the other 2.6, divisions of the scale. Of the remainder of the discrepancy, how much is attributable to error of measurement and how much to irregularities of writing, there is not sufficient evidence to show. We may add that no modern theory of probabilities has been employed, and that there are a number of small errors of computation.

We have referred to but a single part of the book, a part of extreme importance, not for what it accomplishes, but for what it shows reason to believe may ultimately be accomplished. One of the earliest applications of mathematical methods to a question of handwriting was made by Benjamin Peirce. In a certain trial a paper was put in as evidence of which the signature appeared to be a tracing of another signature in the case. Enlarged photographs were made of all the signatures of the same person that were in evidence, some thirty. There were twenty down strokes in the signature. A comparison of all possible pairs showed the probability that any signature would, by mere chance, be superposable in all its down strokes upon another, as the two signatures in question were. The case was decided as Professor Peirce's probability would infer that it should be decided, but upon other grounds. The argument seemed at that time a strange one; and courts ought to be reluctant to deal with modes of argumentation which the judges and the lawyers are not in condition intelligently to criticise.

Several other methods of great interest are described by Dr. Frazer. In the present state of the subject, it seems to the writer of this notice to be too hazardous to convict a man of crime on the evidence of handwriting mainly unless the identification be such as every bank teller would be perfectly confident of, or unless there were other exceptional features in the writing. Circumstantial evidence may, no doubt, be stronger than any direct testimony can be; but to trust to the judgment of an average jury on circumstantial evidence is quite another thing.

73 (15 August 1901) 139-140: The Philosophy of Religion in England and America.

By Alfred Caldecott. The Macmillan Co. 1901. 8vo, pp. xvi + 434.

CSP, identification: Haskell, *Index to* The Nation. See also: Burks, *Bibliography*; *List of Articles*; MS 1440 (draft).

Since much of what appears about the theory of religion is put forth without sufficient acquaintance with what else there is to be said, it seemed desirable to give a conspectus of what has been done all over the field. This might have been accomplished in different ways; or, rather, the chief emphasis and effort might have been put upon different parts of the task. That which Dr. Caldecott has chiefly, though not exclusively, aimed to do has been to take up each writer of any importance--perhaps a hundred and twenty or thirty of them--and, without entering into the merits of the controversy, to state intelligibly what that writer's position, method, and style are, to exhibit sufficient of his argument to show his place in the discussion, to give a critical estimate of his thought, and to inform the reader as to his reception and following; in short, to produce a sort of book-notice of the works of each writer such as a thoughtful evening newspaper might like to give. In this aspect of the work it is simply admirable. The author has a remarkable power of finding out just what is in a book, and what is not in it, and what its idiosyncrasy is. He is accurate, careful, calm, appreciative, many-sided. His power of reasoning is good enough to make any reader of philosophy glad to learn his opinion of a book, while it does not penetrate so deep beneath the surface that the aptness of his judgments can miss recognition for their extreme profundity. His style is always savory; and where occasion is, he can write with finesse or with impressiveness. In one word, it is safe to say that there exists no directory to any branch of modern philosophy that is half or quarter as useful as this book is destined to be found.

Dr. Caldecott distributes the philosophies with which he deals into types; and it will dispose any reader to confess his need of the information that this publication furnishes, to learn that those types are in number no fewer than thirteen. This leads us to notice the second purpose of the book, which is to classify and consider the various types of thought which have been pursued, with a view of extracting therefrom lessons as to what should be tried next. It was, we dare say, beyond human powers to classify in a satisfactory way all the writers that had to be dealt with. But any well-trained logician would have avoided the worst faults of the classification of Dr. Caldecott, who, although Professor of Logic, is weak in that direction. At any rate, competent logicians will easily convince themselves that Caldecott is not of their number by turning to what he says of Dr. Samuel Clarke's so-called demonstration of the existence of a God, in which our author sees a "singular mixture of assertion and ratiocination" which has so puzzled him that he has "sorted out" Clarke's pretended demonstrations in two different ways before satisfying himself as to what the nature of the argument really was. Now, in an ordinary reader nothing could be more pardonable than a perplexity about Clarke's meaning. Indeed, it would rather be a sign that the reader's ways of reasoning were sound and healthy than the reverse. For, as John Caird pretty accurately says, "it is a piece of meaningless jargon." But to a reader well versed in logic there is nothing singular about the argument nor anything to hinder its being understood at a first reading. Clarke's notions of demonstration were false enough, but they were shared by almost all his contemporaries, particularly by Spinoza. The difference between those two writers was that with Spinoza the living thought did not pursue that erroneous method, which, in his case, was merely the garb in which it was clad after it was full-grown--and even then only imperfectly, since it does not accurately conform to the logical rules which it acknowledges. Clarke's reasoning, on the other hand, satisfies those requirements to the full, for it was constructed to do so, and never aimed at anything truer. Its sole merit is that of conforming to futile rules.

The division of thinkers into types would no doubt have been a good idea if it had been restricted to the separation on logical grounds of the histories of widely disparate lines of thinking, leaving smaller subdivisions to be drawn by the historical associations and dissociations themselves. What, unless it was the mysterious fatality of the number thirteen, should have possessed the author to make so many divisions on purely rational grounds that it becomes a nice question in what compartment

almost any given author may be most appropriately pigeon-holed--thus calling for parallel histories in equal number, of movements not historically distinct--one is at a loss to imagine. The consequence is that there is little genuine history in the book, whose parts are brought into relation only by the cement of rather fictitious reflections. It will appear to many that an account of the philosophy of religion in England and America which includes Emerson, treating the 'Essay on the Over-Soul' as natural theology, yet excludes the 'Substance and Shadow' of Henry James, the father, does not thoroughly comprehend its own purpose. The author is not sufficiently acquainted with American thought.

The great utility of this work as a compendium remains untouched. Even if the author is not strong enough to stem the tide of an ephemeral public judgment, as in the importance he allows to Balfour's stuff, this does not in the least matter; or perhaps is a positive convenience. We may add that it is a very agreeable book to read. Its natural style never tires. Its excellent index compensates for all faults of classification. It is printed, not on that beautiful paper so much in vogue which everybody likes who detests reading, and everybody detests who likes reading, but on a laid paper on which the ink takes black, and which gives a book of 450 pages weighing only a pound and a half, cover and all. That, in itself, ought to double the sale of it.

73 (29 August 1901) 172-173: SOME PHYSICAL BOOKS

Contributions to Photographic Optics.

By Otto Lummer. Translated and Augmented by Silvanus P. Thompson. Macmillan. 1900. 8vo, pp. 135.

Experimental Physics.

By Eugene Lommel. Translated by G. W. Myers. J. B. Lippincott Co. 1900. 8vo, pp. 664.

An Introduction to Modern Scientific Chemistry.

By Dr. Lassar-Cohn. Translated by M. M. Pattison Muir. D. Van Nostrand Co. 1901. 12mo, pp. 348.

Practical Electro-Chemistry.

By Bertram Blount. Macmillan Co. 1901. 8vo, pp. 374.

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

Silvanus Phillips Thompson (1851-1916) was a British physicist. He was principal and professor of applied physics and electrical engineering at the City and Guilds Technical College, Finsbury, from 1885 until 1916. Thompson was the author of several works on electricity and the lives of famous men, including Kelvin, Faraday, Lodge, Gilbert, and Petrus Peregrinus.

Dr. Silvanus P. Thompson is known as an excellent physical investigator, and the author of several particularly admirable expository works. In translating Professor Lummer's articles on Photographic Optics he has used great freedom in improving upon the German author, quite rewriting many passages, and adding two

chapters, together with an appendix and an interesting preface. In this preface, Dr. Thompson laments the badness of British text-books of optics, which he attributes to the fact that optical books will not sell in England unless they are cram-books for university examinations set by non-optical examiners. English text-books "serve admirably to get up the subject for the tripos; but they are far too academic and too remote from the actual modern applications. In fact, the science of the best optical instrument-makers is far ahead of the science of the text-books." It is certainly very unfortunate that the possibility of publishing a scientific treatise in England, and far more in America, should depend on whether or not it will be a means of making money for the publisher; but it would be hardly better if it depended on its serving to make money for an optician. Even in Germany, publication is not so facile as might be supposed. Dr. Moritz Cantor's great 'History of Mathematics' hung fire for years before Teubner would proceed with the second volume; and other works which have notably advancd [[sic]] human thought have cost their authors the savings of their lifetime. Meantime, the master-key to the theory of lenses was given by a British mathematician, Rowan Hamilton, as long ago as 1833; and it has remained substantially unused, while German scientists have pursued the fearfully tortuous and labyrinthine analysis which was more directly suggested by the exigencies of the instrument-makers.

A photographer is not throughly [[sic]] accomplished in his profession until he knows at least as much about lenses as is taught in this volume, and there is no other in any language in which the outline of the theory is made equally clear. It will prove a tough nut to crack for most readers; but there is no help for that until the mathematicians have furnished new developments. The two authors seem to be of opinion that photographic lenses have now reached their highest pitch of perfection--at least, until new kinds of glass are produced. But this may be doubted; for hitherto nothing has been aimed at except to bring all the waves of light from any one point of the object to focus on a given point of the plate, without inquiring whether they arrive at that point in the same phase of motion, so as to reinforce one another fourfold, or in opposite phases, so as to extinguish one another. A photographic lens may easily be too perfect, in an optical sense, to produce an artistic effect for any eyes except those which see exceptionally well. We desire that a picture should present nature as it looks to us when we are in a sympathetic mood. If the lens is much more perfect than the human eye, the view reminds us of how things look when our nerves are strung for stern and disagreeable duties--only more so. But from this it must not be argued that the photographer can afford to neglect the seven kinds of aberration which Lummer and Thompson expound after Seidel. On the contrary, the proper conclusion is, that, to the study of this book, the photographer should add that of physiological optics, in order to know what particular kind of defects to value in his lens, and in what different degrees.

Lommel's 'Experimental Physics' is written in a lucid and agreeable style. The author shows a decided faculty for making the subject clearly intelligible with surprisingly little mathematics. The book is not too large for use in a high school. The English edition has a really tasteful appearance, in striking contrast to most of our schoolbooks, however handsome they may be; and this ought to recommend it to teachers and school boards. For when a boy has thoroughly studied a book of natural philosophy, he ought to keep that volume within reach for the rest of his days. It is, therefore, particularly important to inquire what kind of a book this is. We will accordingly examine a single section, occupying about a page--a section neither among the best nor among the worst in the book, but chosen as embodying several qualities that are very characteristic of the whole. It relates to the heat of chemical combination. The author begins by treating the heat of crystallization, and here we remark that the translator says that the color of a certain salt is "bright grey." The setting of plaster of Paris is considered. It is a well-chosen illustration, for it is a process that every boy wants to understand. Unfortunately, the explanation is pretty thoroughly wrong. The statement made is that plaster is calcium sulphate, or gypsum, which has lost its water of crystallization. The truth is, that the essential constituent of plaster is a salt which contains one-fourth as much water as gypsum. That is the reason why the burning of gypsum is such a delicate operation, for if it is heated only a little too much, it loses all its water, and becomes substantially anhydrite, an insoluble salt which absorbs water only very slowly, and will not make a cast in any length of time because it won't hold together. The true plaster, on the other hand, dissolves in the water, and then pretty soon combines with the water in which it is dissolved to form insoluble crystals of microscopic size. In doing this it contracts slightly and evolves heat; but, owing to the crystals lying "every which way," they are not packed accurately together, and microscopic interstices are left, so that the whole mass has a tendency to swell enough to fill out every fine line of the mould. Thus, notwithstanding the pores, the crystals are pressed against one another so closely at certain points as to come within the range of powerful cohesive attraction, which gives the cast a certain degree of strength.

The author next considers the slaking of lime, of which we have the following account: "Burnt calcium (calcium oxide, CaO), generated by heating native calcium (calcium carbonate, $CaCO_3$) in a calcium oven, thus driving off the water, combined with water to produce calcium hydroxide

(CaH_2O_2), or *slaked calcium*, which is a solid." A lime-kiln may properly enough be called a furnace, in English, but hardly an *oven*; nor is lime called "calcium," or lime-stone "native calcium." A little below there is a small table of heats of combustion of ten substances commonly burned for fuel or light. It would have been appropriate to include some foodstuffs. Of these ten values, six are grossly in error, five having the decimal point put one place too far to the left, and in the sixth, alcohol, 91.90 being printed, instead of 71.90.

The translator has studied in Germany, and he evidently thinks himself qualified to improve upon the English language. This sometimes has disastrous effects; as where the rhombic system of crystallization (which we identify by his giving the synonym "quadratic," and also by his not otherwise enumerating this system), being called by the translator "rhomboidal," is confounded with the rhombohedral system; and the pupil is told that it is a hemihedral variation of the hexagonal system, which, by the way, is not true even of the rhombohedral crystals. Water cooled below 32 degrees F., but still liquid, is said by the translator to be "undercooled."

Other writers call it "overcooled." A hydrometer the readings of which are inversely proportional to the density is christened a "volumometer." Truly, with three such lovely words as "volumenometer" and "volumeter," already in the dictionary, and now the new-born "volumometer"--all meaning entirely different things--the English language ought to be supremely happy. The volume is crowded with contributions to the dictionary.

We know very well that Mr. Pattison Muir is a translator acquainted with the

English language and with the science of chemistry--is, indeed, thoroughly skilled in both. Under these circumstances, we cannot understand his choice of Lassar-Cohn's book, or how he could call it an "introduction to modern scientific chemistry," when there is not a word in it about the dominant kind of chemistry of to-day. The name of Ostwald does not occur from cover to cover. The doctrine of valency is much insisted upon; but we hope that *that* is not regarded as particularly modern or as particularly scientific. There are two pages about the "unsymmetric carbon atom," which dates from 1869, as does Mendeleef's table of elements, from which the Helium-Neon-Argon-Crypton-Xenon series is omitted. We notice, by the way, that in two of the three places where xenon is mentioned it is called "xeon." Selenium, too, is called "selenion," throughout. Yet helium is not called "helion." On page 215, the following dictum is printed in authoritative italics: "It is impossible to think of life without the presence of nitrogenous substances." That is not a proposition in chemistry; and to slip it into a chemistry for children, where nobody would suspect such proselytizing, may accord with North German notions; but, in this country, some people will not deem it dealing honestly with parents. Since Mr. Pattison Muir has failed to see the impropriety of it, the publishers would do well to cut it out of the plate. It is a doctrine of metaphysics, and uncommonly metaphysical metaphysics. There are other eccentricities. Thus, the translator adds a note of his own to say that "in our preposterous English system of weights and measures, there is no simple relation between the units of weight and volume." The British unit of weight is the imperial pound; the unit of volume is the imperial gallon. The imperial gallon is defined as the volume of ten pounds of water under standard conditions. No relation could be simpler. This system was the result of the most careful and deliberate consideration on the part of the most competent metrologists that British science has ever produced. It is a little bit pre--something--let us say *premature*--for Mr. Pattison Muir to call it "preposterous" in that particular feature.

Mr. Bertram Blount discusses the economic aspects of every branch of electrochemistry. In eight sections he considers (1) general principles, (2) the electrolytic mining and refining of metals from aqueous solution, (3) the electrolysis of igneous liquids, (4) the electric furnace, (5) electro-deposition, (6) the alkali and chlorine processes, (7) the electrolysis of organic compounds, and (8) power. There are some electrical processes the details of which are kept secret. Others have never been put into practical operation under economic conditions. In such cases, there is, naturally, not very much to be said; but what there is to be said is here said, and said well. Under the sixth and seventh sections there are chemical reactions that some students will think Mr. Blount has not got to the bottom of. But where the

questions relate simply to the economics of electricity, the discussion is masterly. The bitter-beeriness that pervades the British arts is occasionally illustrated in these pages. Thus, we read of an American silver-refining company that has to remelt its silver with a little copper before sending it to England, because the English dealers cannot admit on any evidence that silver can be more that 998 fine. In the last chapter, the following question is put as if it were a poser: Into one of two vessels, both filled with a solution of sulphate of potassa and connected with a siphon, is placed a bar of carbon, and into the other a bar of zinc, these bars having been connected with the terminals of a galvanometer. There is a momentary current, which promptly ceases. Now into which vessel shall we pour a little sulphuric acid in order to make a steady current, and why? The great name of Ostwald is invoked for a principle on which to decide this question--a Titan imported to crack a hazelnut, as any American amateur electrician would find it.

73 (3 October 1901) 267-268: MAHER'S PSYCHOLOGY

Psychology: Empirical and Rational.

By Michael Maher, S. J. Fourth edition, rewritten and enlarged. Longmans, Green & Co. 8vo, pp. 602.

CSP, identification: Haskell, *Index to* The Nation. See also: Burks, *Bibliography*; *List of Articles*; MS 1441 (draft).

Father Maher's purpose is so to present psychology as to illustrate the advantages of the Thomistic Aristotelian metaphysics, as well as the positive contributions to psychology by St. Thomas Aquinas and by Aristotle. This is done with a remarkably complete acquaintance with modern psychology, and a sufficient acquaintance with scholastic writings for this purpose. Here and there we detect a vague conventionality in the accounts of mediæval scholasticism which leads us to surmise that, as it was in its full bloom, it is not very well understood by the author. But that is of small consequence. The great scholastic psychologist was Aquinas; and Aquinas has been thoroughly mastered.

The volume verges upon corpulence, and when one finds that much the larger part of it is in one or other of three grades of small print, one gets the impression that it is a pretty full treatise. But when one comes to find not only that it covers everything usually called psychology, including both *Erkenntnisslehre* and such branches as Animal Psychology and Hypnotism--pausing, too, to make practical applications--but that, besides, it discusses Free Will, the nature of the Soul and its connection with the body, and, furthermore, that the postion [[sic]] taken involves considerable controversy, it is evident that, although concision has been studied, the work can really do little more than outline what is known upon each topic. The historical matter, the summaries of different opinions, and the select references to places in other books where each matter is treated more at large, would be sufficient to give this treatise a real utility for the student, but its main interest is as a defence of Thomism as a grounding for modern pyschology [[sic]].

"My aim," says the author, "has been, not to construct a new original system of my own, but to resuscitate and make better known to English readers a psychology that has already survived four and twenty centuries, that has had more influence on human thought and human language than all other psychologies together, and that still commands a far larger number of adherents than any rival doctrine."

As to the last pretension, if noses are to be counted simply, regardless of what lies behind each nose, and if notions about mind and body, however vague, are to be dignified as psychological "doctrines," no doubt the author is right. If we are to exclude all but readers of psychological treatises in English, French, German, and Italian, taken collectively, the estimate is probably much exaggerated; and if we confine ourselves to scientific psychologists, the majority is great on the other side. Perhaps, however, in one sense, it might not be so, if scientific men were accustomed to draw the distinction that ought to be drawn between the hypothesis which is preferable in a given state of general scientific research, and the hypothesis which is preferable for instant action. It is true that, after induction has done its work, or has substantially done it, no such distinction is to be drawn; for then the hypothesis has ceased to be a mere presumptive hypothesis. But as long as experimentation to test the hypothesis is in its early stages, which is the case in regard to the deeper questions of the science of the soul, the economies of a research which may probably be protracted through several generations, or even centuries, render indispensable a system of procedure which will have little relation to what seems likely at the moment. There is nobody who is experienced in difficult inquiries--say, for example, the detection of the author of a crime--but is well aware that nothing is more fatal than to attach much weight to what merely seems likely towards the beginning of an investigation.

Probably, during such an inquiry, several theories will have to be tried and rejected; and in what order they shall be tried is a question of economy. But if one be forced, without completing the study, to act upon one theory or another, quite a different series of considerations ought to be decisive. In particular, good scientific economy will usually prescribe that simple hypotheses shall be thoroughly tested before resorting to complicated ones. This is the truth in Ockham's razor. But it is very far from being true, in questions concerning any science of life--psychology, physiology, and the like--that the true hypothesis is likely to be simple. On the contrary, the history of discovery in those departments shows many more examples of the old theory being found to be too simple than of its being found to be too complex.

Now the main, and almost the only general difference between the psychology which Father Maher defends, and that which is current among modern scientific psychologists, is that the former admits an element, that of the efficient agency of reason, which the latter excludes. Certainly, the proper scientific method is to try first whether all the phenomena may not be explained without that agency, and to resort to it only after it shall come to be overwhelmingly proved to be indispensable. But as long as it is very far from having been proved that the phenomena of the universe and of mental action, so far as we know them, can be entirely explained without the efficient agency of reason, not merely upon mind (and *that* the modern psychologist practically refuses to admit), but even upon matter, there is nothing

illogical in entertaining, as a small party among the warm advocates of the existing method of study do entertain, the opinion that science will ultimately be driven to have resort to that theory.

We fear that the perusal of Father Maher's treatise may rather weaken than strengthen any previous bias toward his views. There is a charm about Aquinas. His reader breathes, for the time being, a mediæval atmosphere; and in the dim cathedral light of that interesting age that built the Sainte-Chapelle, and Amiens, and much of Notre Dame, theories look very attractive which, when they are set down on a modern page, and are examined in the hard daylight of the twentieth century, strike him as cramped and grotesque, not to say crude. It may be doubted whether Father Maher has, after all, done the best for the essential theory. It may be doubted whether a man of his profession could do that, although his thought is, very likely, quite as free as that of an average North German university professor. But in the one case there is an external rule which draws a sharper line than exists in the other.

However, it was in no way incumbent upon him that he should fall into the very fallacy which he justly condemns in many of the works that build on modern ideas. Although this book has been almost entirely rewritten, yet it is based upon an original edition of 1890, so that much of it was written before James's great 'Principles' appeared. Now, in the fundamental conceptions of the science there has been a great advance since then. They are not by any means thoroughly clear, even yet; but probably nobody would now propose, as James then did, to write a psychology altogether uninfluenced by any metaphysics. As Ladd well names it, the "clandestine" metaphysics which such an attempt inevitably brings with it, is all the more dangerous from its lying in ambush. But Father Maher does substantially this very same thing. It is true that he avows his metaphysics at the outset, but he makes no formal defence of it until he reaches page 459. "In fact," he says, "our chief contention is that a complete and accurate separation of the two branches of Psychology [positive psychology and metaphysical psychology] is impossible." There is no need of considering absolute cleanness of cut; that is not the question. The point is that it has been made manifest that positive psychology cannot escape taking for granted a metaphysics of one kind or another in no inconsiderable measure. But what never has been proved, nor can any good reason for believing it be found, is that metaphysical psychology stands in need, in any degree worth consideration, of the scientific results of positive psychology. We must distinguish between results which depend upon the validity of the scientific method of psychology--scientific discoveries--and those rough facts about the mind which are open to everybody's observation, and which no sane man dreams of calling into question. As a matter of fact, it is upon these latter facts, and upon a series of similar facts about the outer world, that every man actually and really bases, first, his general metaphysics, and then his metaphysics of the soul. Even modern conceptions of the nature of intelligence, although facts of physiology have aided their development, can be more logically defended without resort to anything but those general facts about which nobody any longer ever simulates a doubt, and never did do more than simulate one. But as for the general Aristotelian metaphysics upon

which Father Maher builds, it would be ridiculous to say that it cannot find all the support that is to be found for it anywhere, in the common facts upon which Aristotle himself rested it; nor is anything more needed for Father Maher's pneumatology.

It is remarkable how very little his Book II., on metaphysical psychology, would have needed to be modified had he chosen to transpose this with Book I., on positive psychology. The result of his doing so would have been that a good many discussions in Book I. could have been dispensed with; and the whole work would have been at once more sincere--we mean, more true to the author's real thought--and vastly more logical. As it is, we should decidedly recommend this transposition in reading the book. There will, however, still remain the fault that the general metaphysics, upon which the decision of the dispute really must turn, is not made the subject of an explicit and separate examination. That ought to have come first of all. Logic required it; good rhetoric, too. For a way of thinking so different from that of our day that it would have come upon the reader as a complete surprise, has everything to gain by an overt attack. It is only assumptions that the reader already makes that can to any purpose be slipped in surreptitiously.

To conclude, the book will be found well worth consideration by students. It has much to recommend it, also, for those who never expect to read another on this subject, although its consicion [[sic]] renders it just a little dry. Let this be followed by the delightful perusal of James's smaller book, after which Baldwin's little 'Story of the Mind' will be an *entremets*, and the reader will have a very decent knowledge of what psychology is.

73 (24 October 1901) 325-326: Ethics: Descriptive and Explanatory.

By Sidney Edward Mezes. The Macmillan Co. 1901. 8 vo, pp. 435.

CSP, identification: Haskell, *Index to* The Nation. See also: Burks, *Bibliography*; *List of Articles*; MS 1442 (draft).

Professor Mezes of the University of Texas has been known to the general public as a scholar of Howison, and as one of the four authors of the sympotic book, 'The Conception of God.' He there produced upon us a mixed impression, for his intellect seemed not to have quite so keen an edge as is called for in philosophy; and yet here and there conceptions appeared so simple and obvious, and yet so novel, that one ransacked one's memory in the endeavor to recall any anticipation of the remark. Much the same impression is renewed by the present book. Hard work and solid has been put into it; and, of course, the harvest must have proportionate value. Parts of the treatise are admirably worked out, and are, at any rate, instructive, even if their conclusions are rejected. But hard work is not all that is required in dealing with such a subject.

In aim and method the present work is fully as original as it ought to be. The author belongs to that school of ethics which is probably nearest right--

that is to say, to the school which makes tribal tradition a main factor of morality, and which

is thus enabled to frame an evolutionary theory of it. But although the author is thus in the van of ethical exploration, a certain old-fashioned and conservative color--attributable, perhaps, to temperament and Texan environment--strongly tinges his theory. Now, conservatism in morals is most needful in practice, and, of course, is theoretically defensible. But that defence itself is not conservative: on the contrary, it is rationalistic; and in pure theory, especially in a theory of aims, conservatism is irrational and out of place. The writer effects a reconciliation of his conservatism (which is very likely unconscious) with his advanced views by exaggerating more than usual a prevalent tendency which we venture to think that the majority of philosophers of our day carry too far--we mean the tendency to base everything in philosophy upon the psychical sciences. The immense success of scientific psychology during the last forty years has very naturally given it a weight in men's minds that ought not in philosophy to be accorded to any merely special science, which is precisely what psychology has all along been striving and struggling to be. On the contrary, it is now generally admited [[sic]] that psychology, like general physics, necessarily takes for granted a *Weltanschauung* or outline system of metaphysics. Now, metaphysics can have no satisfactory grounding except upon a scientific logic; and logic rests on ethics to a degree that few are aware of. So if there be no other basis for ethics than psychology, which is a third story above it, the whole erection floats on air. Ethics as a positive science must rest on observed facts. But it is quite a different thing to make it rest on special scientific observation, and still more so to base it upon scientific conclusions. The only solid foundation for ethics lies in those facts of every-day life which no skeptical philosopher ever yet really called in question.

Now, Mr. Mezes is so far from taking this view that he maintains that the whole business of the moralist consists in saying what men mean by morality, in describing what they hold to be moral, and in explaining how they come to do so. This is a most interesting and valuable study, but it is ethical anthropology, not pure ethics; and to limit ethics in this way is to be faithless to the first duty of a moralist, as such. "Ethical writers do not in any proper sense," he says meaning that they overstep the bounds of their province when they do, "judge conduct or issue pronouncements as to what is right or wrong. Their more modest task is to discover and record men's genuine judgments as to what is right or wrong." Let us see how this view of ethics works. A judge, let us suppose, has brought before him a case in which a man has suffered injury for which he claims damages of another. Whether damages ought to be paid in such a case is often, we know, a delicate and puzzling question. We will follow Professor Mezes in using a much too simple illustration, which ought to puzzle nobody. "Take," he says, "the case where A's cattle break out of their enclosure, in spite of A's having used all the care he reasonably could have used, or could learn to use, and destroy B's valuable crop in an adjoining field." This case (or rather another far more difficult) puzzles the judge, and he takes it under advisement. He naturally looks into works on ethics, and, finding nothing pertinent in modern books, is driven to the scholastic treatises. Now, there is nothing in the whole scholastic logic more justly an object of derision for any modern thinker than its weak confusion of thought in its doctrine of causes; nor in that whole doctrine is there any more manifest absurdity than the distinction between a *proximate* and a *remote* cause. When we meet with an application of it in the scholastic commentary on the Sentences, it stands out as so much more nonsensical than the rest as to be comical; but that anybody should be made to suffer because of any consequence of such metaphysical jargon is outrageous flippancy. Yet it is just this outrage that the judge is driven to commit, or to pretend to commit, because the ethical writers have not expounded right and wrong in a sufficiently luminous and reasonable form.

Professor Mezes follows them. He maintains that A, the owner of the cattle, ought to reimburse B for the injury done by them to his crop, because A is the *proximate cause* of B's suffering. If he would not follow the decisions of Texas courts as the ultimate evidence concerning right and wrong, he could not fail to see that the real reason why the judge awards damages to B is that to allow a private person to undertake a business humanly sure in the long run to injure his neighbors (and all the more so if he "cannot learn to use" suitable preventive measures), and then to allow him to pocket all the profits, and make his neighbors pay for incidental losses, would be to bring himself and his court into public contempt and into no little danger. That was the judge's real reason. But in days gone by (perhaps not yet in Texas) if a judge could decide a case justly, and yet by a process of metaphysical reasoning the less intelligible the better, he was regarded with awe by the vulgar; and that was one motive for his seizing upon that argument when he could get no modern light.

One of the distinctive features of Professor Mezes's book is a seventy-page chapter on Justice, in which legal decisions are followed, often in a way which will be repugnant to right-minded readers, and yet not so exclusively that the chapter can be said to constitute an exposition of the traditional legal conception of justice. Professor Mezes defines ultimate good as "the welfare of all sentient beings," but he is doubtful whether it is worth while to have any regard for the welfare either of *bacilli* (are these sentient beings in Texas?) or of criminals of all classes. The last exclusion is characteristic, we are sorry to say. But when we ask what he means by "welfare," in place of a *definition*, nothing is vouchsafed but a *division* of "welfare," in which there are two or three dozen items, such as "easy activity," "sense of personal attractiveness," "sense of solvency," "satisfaction from social standing," "sense of divine favor," "national pride," "self-control," "a body of well-poised spontaneous activities," "systematic ideas of rights and duties," "sagacity." There are those who will think that all this is on a pretty low plane, and we do not see much in the list about the welfare of earth-worms, etc., notwithstanding the insistence upon "all sentient beings."

The best thing in the book is the psychological analysis of conscience, which is decidedly noticeable. We could hardly have expected the terminology to be reformed. The scholastic writers mark two things which they distinguish by the terms *synderesis* and *conscience* (the latter nearly in the sense in which it is a household word). The interest of progress in ethical discussion calls upon us to come to agreement about the use of technical terms. But each of us is attached to his own habit, and will not surrender it unless it can be shown clearly to violate a law to which

he has given in his allegiance. A code of rules is needed, in framing which we cannot do better than to be guided by the taxonomists, who have had, of all men, most experience in dealing with similar difficulties. If we do that, our first rule, subject, perhaps, to a few general but well-defined classes of exceptions (the fewer the better), will certainly be that every technical term of philosophy ought to be used in that sense in which it first became a technical term of philosophy. This will, generally speaking, result in the greatest accord between the language of philosophy and the vernacular, of which the word *conscience* will be an example. As for that other thing which a good many moralists call conscience, some other name ought to be given to it, preferably a new word. At any rate, not *synderesis*, of which the original meaning, we are convinced, is not that which Siebeck assigns to it. Professor Mezes, whose definitions are mostly of doubtful accuracy, distinguishes between conscience about others' acts and conscience about one's own. But a stay-at-home conscience does the most to render earth habitable.

As we rise from the reading of the whole book, we find ourselves saying, If *this* is what morality is, we are disposed to sympathize with Henry James, the elder, in his very limited respect for morality.

73 (21 November 1901) 393-395: THE NATIONAL ACADEMY AT PHILADELPHIA

CSP, identification: Haskell, *Index to* The Nation. See also: Burks, *Bibliography*; *List of Articles*; MS 1443 (draft).

PHILADELPHIA, November 15, 1901

The National Academy of Sciences has just closed here one of the most successful of its autumn meetings, and one of the most agreeable and interesting as to its reception. It met on Tuesday forenoon, November 12, in Houston Hall, which is the general students' club of the University of Pennsylvania. The first paper was a biographical notice of the late Dr. Genth, who was eminent as a chemist. On Wednesday Gen. Comstock read a notice of the life of Gen. John Newton. The list of papers promised that chemistry would occupy the attention of the Academy more than any other science. But the chemists seem to think--mistakenly, I fancy--that scientific men outside their own fraternity cannot be expected to care for questions of the chemical constitution of this or that class of bodies; and the consequence was that some papers in this department went unread, as did two by Dr. Barus, one of which, on Nuclear Condensation, we were particularly sorry to miss. One of the most important contributions was made on Thursday, by Prof. James M. Crafts, on the "pseudo-catalytic action of concentrated acids."It related to that branch of chemistry, to-day almost the dominating one, which considers the rapidity--or, as the chemists phrase it, the "velocity"--of chemical action. This, we know, in a general way, depends upon the concentration of the different reagents. But how should "concentration" be defined? Hitherto, only extremely dilute solutions have been employed in such researches; and as long as study was confined to them, it was sufficient to define concentration as the number of molecules, or of ions, per

unit of volume. It has been well known that this definition would not lend itself to any simple expression of the action of concentrated solutions; but in what manner it ought to be modified has not hitherto been satisfactorily made out. Professor Crafts has found a definition which answers to perfection for certain concentrated solutions, at least. He has selected for study the action of "strong" acids--namely, those which are unable to resist the ionizing action of water--in decomposing many substances, without themselves entering into any chemical combination. This fomenting of discord is what is called, in a general way, catalysis, or pseudo-catalysis when it is suspected that the fomenters are not really quite so disinterested as they pretend to be. Professor Crafts has had the happy idea of adopting for subjects of decomposition a class of bodies called sulphonic acids, which, for certain reasons, are particularly available for the purpose. In these cases, the proper definition of concentration was found to be that it is the ratio of the number of active ions present to the total number of molecules. Experiments were also made with muriatic acid as the catalyzing agent, with the result that its effect is an exponential function of the amount. This is a fact perhaps as important as the definition of concentration. Although the mass of observations already obtained by Professor Crafts is large, it is likely to be greatly increased.

Another very interesting chemical paper was presented on Tuesday morning by Prof. Edgar F. Smith. Everybody who has read a treatise on chemistry within the last twenty years is aware that the interest in the precise value of the atomic weights of the elements has been immensely heightened by the discovery of the periodic law. All the elements, from atomic weight 39 up, are arranged, according to their chemical properties, in eighteen vertical columns and five horizontal rows; and the atomic weights increase downwards and to the left, the left-hand element in each row (having the lowest atomic weight in that row) being of higher atomic weight than the right-hand (or highest) atomic weight of the row above it. There are two exceptions only. The atomic weight of Nickel, 58.7, ought to be higher than that of Cobalt, which 59.0; and the atomic weight of Iodine, which is 126.8, ought to be higher than that of Tellurium, which is 127.5. But while the atomic weights, with those exceptions, always increase, and although upon this table have been based successful predictions of the existence and chief properties of four elements, at least--Gallium, Germanium, Scadium, and Neon, while Crypton and Xenon were virtually predicted--yet there seems to be no exactitude or strict regularity in the amount of difference between two successive atomic weights. This may very well be because many, if not most, of the elements are so impure as to falsify their atomic weights, and in that way completely to mask the law of progression, which is probably itself periodic.

This hypothesis is somewhat confirmed by the fact that there is a gap in the table, which is supposed to represent sixteen successive elements hitherto undiscovered or unlocated; and nearly all the elements which appear in the table just above these undiscovered elements have their atomic weights heavier than we should expect from the periodic law; while all those just below any of the undiscovered elements have their atomic weights lighter than we should expect. This would be just the effect that would be produced if these elements were contaminated with the undiscovered elements. Among the elements whose observed atomic weights are lighter than we should expect them to be, none is quite so remarkably so as Tungsten, which is set down as 184, although we should expect it to be between 186 and 187. Professor Smith has undertaken a new determination of this atomic weight, and has ascertained that there is no known process which will free Tungsten from Vanadium, the atomic weight of which, being only 51.4, must have lowered the apparent atomic weight of Tungsten by a considerable amount. The patience of an inorganic chemist needs to be inexhaustible, and thus far Professor Smith has not himself succeeded in effecting the necessary purification of Tungsten. Meantime, since he has proved that all the hitherto supposed pure Tungsten contained Vanadium--say, perhaps, about 1 per cent. of it--it is probable that the true atomic weight is about what the periodic law would lead us to expect it to be.

Another paper approaching a chemical subject, if Mr. Brush's announced etherion of several years ago can be called chemical, was read by Prof. Edward W. Morley, on the transmission of heat through the vapor of water at low pressures; the name of Mr. Charles F. Brush being associated with his own as collaborator. Mr. Morley proved conclusively that, at certain very low pressures, the transmission of heat through aqueous vapor is very considerably more rapid than through air, though it is always much less than through hydrogen. This paper was read on Wednesday.

There were very few physicists at the meeting, and one may say no astronomers, who were doubtless kept at home to welcome the Leonids. But a geologist, Dr. George Ferdinand Becker, brought out, on Thursday, a physical phenomenon which one can hardly believe to be absolutely new, although it would probably be difficult to find any record of it. Namely, it is not uncommon to find laminæ of slate separated by crystals; but it has hitherto been supposed that the laminæ were first separated by faulting or otherwise, and that subsequently the crystals were deposited. An instance in which a crinkle affecting several successive laminæ lay in a line perpendicular to all of them led Dr. Becker to cause the following experiment to be made. Horizontal plates of glass, kept from one another at a fifth of a millimetre, were immersed in a strong solution of alum, which was permitted to crystallize. The result was that the crystals forming between the plates forced the latter apart nearly to the distance of a millimetre. Now, since there seems to have been nothing but friction to oppose lateral expansion, it seems that the growing crystal is capable of stresses like those of the solid.

Another note by Dr. Becker was geological in its aim; but its reasoning was purely dynamical. This was a refutation of the orogenetic theory of tilted blocks. According to this theory, the crust of the earth, floating upon a magma, becomes broken up into blocks which are then tilted, so as to lean in oblique positions one against another, thus forming mountain ridges with intervening valleys. But Dr. Becker showed that masses so great as would be required thus to account for mountain formations would, even if each was a block of flawless granite, be broken by its own weight into pieces, some of which would be so thin as to turn quite over upon their sides, so that a sort of discontinuity would result, very different from anything seen in geology.

There were two interesting and brilliant exhibitions by Prof. George F. Barker: one, of the five new gases and their light--helion, neon, argon, crypton, and xenon--neon showing a most extraordinary scarlet light, and argon, with a condenser, a magnificent deep blue; the other, of two of the new incandescent electric lights, (1) that one in which the filament is composed of that same mixture of 99 per cent. thoria and 1 per cent. ceria which shines in the mantle of the Welsbach burner; and (2) that one in which the filament is composed of metallic osmium. Presumably, the hindrance to the extensive use of the latter would be the impossibility of obtaining osmium in large quantities. The method of making the filament of this excessively refractory and hard metal is a secret in possession of the Welsbach Company. Otherwise, since it requires but half the voltage of the carbon incandescent light, there would be very great economy in it. The ordinary voltage would presumably destroy the filament.

On the biological side there were half-a-dozen papers of a high average order of merit; especially two in physiology. The best was on "Snake Venom in Relation to Hemolysis, Bacteriolysis, and Toxicity," by Dr. S. Weir Mitchell and Dr. Simon Flexner, the latter not yet a member of the Academy. It is generally known that Dr. Weir Mitchell was the first to study snake-venom scientifically but it was not until long after his first researches had been laid aside that, by some subconscious process, well illustrating Whewell's theory of scientific ideas, he was brought to the hypothesis that venom consists of a mixture of two distinct poisons. When Dr. Weir Mitchell had conceived the hypothesis, he called upon a friend to collaborate with him in putting it to the test of experiment; and it was not until many months had been devoted to patient work, and discouragement was setting in, that the investigators found that the two poisons could be separated by a dialyser, the one acting intensely upon the higher nervous centres, the other disintegrating the blood. Circumstances then again long arrested the further prosecution of the inquiry, during which time it had been somewhat advanced by European physiologists, until last year Dr. Weir Mitchell proposed to his student, Dr. Simon Flexner, to take up the investigation anew upon certain general lines which he formulated. Dr. Flexner seems to have performed his task with distinguished ability; and he certainly presented the somewhat complicated matter with unusual lucidity and fluency.

Another interesting investigation, by a young physiologist, Dr. Horatio C. Wood, jr [[sic]]., introduced by Professor Barker, had been read on Tuesday and was illustrated by photographs and sphygmographic traces. The problem could not be more important from an iatric point of view, being that of the peculiarities in the supply of the lungs with vaso-motor apparatus of nerves, and the consequent specific effects of various drugs in incipient pneumonia, etc. The experiments were performed upon dogs, whose chests were opened by median incisions. It was evident that the physiologists of the Academy thought this the weakest link of the work; for when the lungs were so exposed to the air, carefully as the temperature and humidity might be attended to, they were under most abnormal conditions.

One of the stronger of the young biologists, Prof. Henry F. Osborn of Columbia University, put forward an hypothesis of what he called Latent, or Potential, Homology,

which he illustrated by a peculiarity in the growth of teeth. When we study the design of the most developed grinding teeth and compare with it the teeth of "recent" horses on the one side and of primates on the other, we find commencing in both these widely separated groups a development of certain folds which are evidently destined to take a certain future development, alike in the two cases. But horses and primates have had no common ancestor for upwards of a million years back, if we accept a chronology whose possible errors can be of no consequence for the present purpose. We appear, therefore, to be confronted with a feature of development which has been lying *in posse* for a million years. What could have been its mode of being during that time? Professor Osborn had two other papers full of interesting ideas. In one of them he proposed to use the features of dolichocephaly and brachcephaly [[sic]] (long and broadheadedness), which have had perhaps an exaggerated esteem among anthropologists, and have been confined to their science, as important factors in the taxonomy of different families of vertebrates.

A paper by Dr. Caswell Gravé, introduced by Prof. W. K. Brooks, was read by the presiding officer. It related to an improved method of rearing marine larvæ. Finally to be mentioned is a paper by Mr. C. S. Peirce upon the logic of the process of drawing historical conclusions from ancient testimonies. This was an elaborate memoir, in which the method of balancing probabilities was combated as being, in most cases, illogical; a different method being developed and defended, with full details of the different conditions to be fulfilled. Three examples were given to illustrate the new method and contrast it with that in vogue among the higher critics. The first related to the Scepsis story concerning Aristotle's manuscripts; the second to the chronology of the Dialogues of Plato (where the data accumulated by Lutoslawski were employed); and the third to the life and character of Pythagoras, this being selected as showing how to deal with the least trustworthy testimonies.

The Academy received the most charming attentions from the Philadelphians. Provost and Mrs. Harrison gave a reception on Wednesday evening in the Museum of Science and Art. A New Yorker would do well to devote a day to going to Philadelphia to visit that museum, one of the most deeply interesting that the whole world contains, if only for these three departments--the Babylonian remains, including those from Nippur; the Japanese Buddhistic temple; and the Matthew Stewart collection of gems, which is unrivalled in interest by anything of the sort your correspondent has ever seen. On Thursday Dr. Weir Mitchell entertained the Academy at dinner, where were gathered many of the most interesting men in Philadelphia.

M. D.

73 (28 November 1901) 415: NOTES

CSP, identification: Haskell, *Index to* The Nation. See also: Burks, *Bibliography*; Fisch and Haskell, *Additions to Cohen's Bibliography*; MS 1444 (draft).

--Persons who are in the way of writing French will find the 'Concise French Grammar' of Mr. Arthur H. Wall (Oxford: Clarendon Press; New York: Henry Frowde) convenient, owing to its embodying the effect of the *Arrêté* of July 31, 1900, so that, for example, they not only must, as formerly, write, "Cette dame est *toute* gracieuse," but *may* write, "Cette dame est *toute* aimable." We do not think, by the way, that the permission to write "Ces dames sont *toutes* aimables" for "Ces dames sont *tout* aimables" will be felt to be an advantage by anybody; but some of the old rules were certainly intolerably senseless. We observe that Mr. Wall says that "neither rules nor explanations are of much practical value" in guiding us to the preposition that ought to accompany an infinitive following a verb. That, perhaps, depends upon how much "much" means. We are certainly dominated in the matter by psychological principles of association which to a certain extent follow formulas. No rule can be given which will dispense with the necessity of learning the construction of each verb along with its signification. To trust to any rule would be much like trusting to the English or Latin for the precise import of the word. But as nobody would deny that the recollection of the Latin meaning does help us to remember the meaning of a corresponding French or English word, so rules can be given which will aid very much in retaining the French construction, even when it is exceptional. Without doubt, subconscious rules there are in the minds of people who talk French, sometimes without their being able to say two minutes later what preposition they had employed. The little volume of two hundred and fifty pages well deserves recommendation, and is wholly free from the nonsense that fills the pages of Girault-Duvivier and even Bescherelles--nonsense not without its utility, however.

73 (12 December 1901) 462: Practical X-Ray Work.

By Frank J. Addyman. London: Scott, Greenwood & Co.; New York: The Van Nostrand Co. 1901. 8vo, pp. 207. With twelve plates and 52 cuts.

CSP, identification: Haskell, *Index to* The Nation. See also: Burks, *Bibliography*; Fisch and Haskell, *Additions to Cohen's Bibliography*; MS L 159.186; MS 1445 (draft).

Being really well written and put together with good judgment, this book better deserves mention than the common run of technical publications. It treats amply of every part of its art without being swamped by inutilities. The historical division of the work is naturally brief. More than half of it is taken up with discussing the advantages of different forms of apparatus, sources of electricity, coils, tubes, airpumps, screens, localizers, etc. Then there are chapters on Installations, Radioscopy, Radiography, and the applications of the new art to dentistry, to chemistry, and to war. Much depends upon the fact that, other things being equal, opacity to X-rays is apparently increased by the presence of an element of high atomic weight, although nothing that can be called a law has been formulated. But lead glass is far more opaque than soda glass; Iceland spar than silica or alumina; imitations than genuine precious stones (exclusive of the pearl). The application of X-rays to examinations of the roots of teeth and their surroundings must be a

matter of no little difficulty; which explains a chapter being devoted to it. The localization of what is seen is a branch of the art which time will, no doubt, greatly improve. At present, it is done on the principle of a plane-table triangulation, but with various sorts of instruments. Apparatus depending upon our natural stereoscopic judgments has hitherto been found too troublesome.

1902

74 (23 January 1902) 78-79: History of Intellectual Development on the Lines of Modern Evolution.

By John Beattie Crozier. Vol. III. Longmans, Green & Co. 1901.

CSP, identification: Haskell, *Index to* The Nation. See also: Burks, *Bibliography*; *List of Articles*; MSS L 159.181-182.

When we bade a hopeful *revoir* to Mr. Crozier's History at the end of its first volume, Christianity was just thoroughly established; but, the second-projected volume having been skipped, we are now surprised with a third, devoted, half to the nineteenth century and half to the twentieth. We are very sorry that failing eyesight was the cause of this change of plan as we suppose it also was of there being no history, but only disquisition, in the third volume. But we are heartily glad to meet Mr. Crozier again on any terms, for he always has something to say which were well worth reading, even if it were not set forth in a style which would make almost any matter pleasurable--a style which this iron age is not all accustomed to. What he now gives is the application of what we should have learned in the second volume to the politics of England, France, and America of to-day. He lays down four "rules of practical statesmanship" as the lessons which we should have learned from the unwritten volume, and then proceeds to apply them modestly, as mere exemplifications of their meanings, to the condition of the three countries named. The rules are as follows: First, consider the genius of your nation. Don't try to make a lap-dog of a horse; but limit yourself to such excellences as your stock can vitally assimilate. Second, make for the ideal in a steady march; but do not leap to it. Third, break down any barriers which may prevent one caste from being recruited from another. Fourth, restrict your aim to improving the material and social condition of your country; and do not attempt to change the character of your people.

When he comes to deal with the United States, Mr. Crozier finds the government so perfect that no room is left for other than minor suggestions:

"For," says he, "if we consider it, there is no one of the great objects for which government exists that has not for the last hundred years been abundantly provided for and safeguarded by the Federal Constitution--life, liberty, the pursuit of happiness, security of person and property, freedom of religious opinion and worship, and, above all, an open arena, with equal rights, equal opportunities, and equal access to positions of honor and trust for all--and that, too, in a degree unknown elsewhere in the world, with the exception, perhaps, of certain colonies still attached to the British crown."

Of course, it is our duty to be amused at every remark that a foreigner may make upon our politics; and we are a little amused at Mr. Crozier's imagining that the Federal Senate is much freer from evil influences than the House. The best of his hints is, that a great part of the business of House committees, being of the nature of inquiry into facts, ought to be conducted somewhat according to the rules of a court, evidence being heard in public. He also suggests that private bills and other exceptional legislation ought to be discussed in a large and public committee before going to the special committee.

Mr. Crozier is very much opposed to abstractions in politics (such as equality and the like) being considered as ends. He is willing to grant that they may be commendable means. But an end, in his view, must be something strictly concrete and free from all generalization--at least in politics. Some will think that that opinion affords an accurate guage [[sic]] of Mr. Crozier's philosophical calibre. Anybody whom his former books reminded of 'Typical Developments' will be capable of smiling at parts of this; but it must be acknowledged that it abounds in suggestions, as we have said, and the ornate and almost poetical style would render far less solid matter agreeable reading.

74 (27 February 1902) 178-179: Classification, Theoretical and Practical. I. The Order of the Sciences; II. The Classification of Books.

By Ernest Cushing Richardson. Charles Scribner's Sons. 1901. 12mo, pp. 248.

CSP, identification: Haskell, *Index to* The Nation. See also: Burks, *Bibliography*; *List of Articles*; MS 1447a(s) (draft).

Ernest Cushing Richardson (1860-1939) was a well-known American librarian. He was graduated from Amherst in 1880 and from Hartford Theological Seminary in 1883. He served as librarian and instructor in the Seminary from 1883 until 1885, at which time he added to his duties those of assistant professor of bibliology. Richardson was the author of several translations of ancient works and a monograph on Faust.

We should hesitate to recommend this book to a beginner in the subject; but one who knows what is usually said about it will find Mr. Richardson's absolute independence of all that, refreshing. He does not argue much, but his dicta are dynamitic [[sic]]. For instance, he holds that, in order to be a subject of classification, a thing must have separate existence. This is sufficiently surprising; but upon this statement immediately follows another even more radical, to wit: "Ideas are therefore the subjects of classification just as much as anything else not only because they have a material as well as a spiritual substance but because they are individual separate things existing in a certain place at a certain time in a certain definable nature." (The non-punctuation is not ours.) Whether the man who writes so has read, may be doubted, but he has certainly thought. The same fact is evinced in his definitions. "*Likeness*, as the ground of the putting together of things in classification is, in brief terms, interchangeability." If this writer would only subject himself to some Socratic dialogues with a logical sharp, we do not doubt that his analytic powers would ultimately become very useful to his profession of librarian. But it cannot be that he should at present produce a good classification of subjects of books, because he would be governed by conceptions that not only are unfamiliar to those who would consult his catalogue, but are also untenable in themselves. As for his classification of sciences, we may possibly have seen worse ones.

It is, however, the Appendix to the book, which fills more than half again as many pages as the body of the text, that is the really useful part of it. This consists of lists of references to systems of classification of sciences and of books. Of course, it cannot be complete. There is, for instance, no reference to the Century Dictionary under *Science*. The tables do not evince great learning, but they must prove highly useful.

74 (6 March 1902) 192-194: PASTEUR

The Life of Pasteur.

By René Vallery-Radot. Translated by Mrs. R. L. Devonshire. McClure, Phillips & Co. 1902. Two volumes. 8vo, pp. 301, 304.

CSP, identification: Haskell, *Index to* The Nation. See also: Burks, *Bibliography*; *List of Articles*; MSS L 159.185, L 159.187; MS 1448 (draft).

Pasteur was heartily a Frenchman, but not after the fashionable light-hearted pattern. A heart more completely unsullied by all there is in its world to sully it--egotism, chiefly in *his* world--is not to be found in what land or age you will. It happened that his discoveries shone brilliant to those who regarded them from the side of utility--to wit, his improvements of wine and beer, his cures of the silkworm trouble, the chicken cholera, the swine fever, anthrax, diphtheria, hydrophobia, etc. Nor was it accidental that his discoveries had that character. Still, it was not that which made him the great man he was, but his strength in research, which cracked every nut, however redoubtable, with such surprising promptitude that one really cannot think that it was ever fully tested. When we wish to remind ourselves what scientific logic really is, we may think of Pasteur, his investigations, and his life, and ponder them well on every side, without overlooking the intimate vital link between the intellectual and moral parts of him.

He may be said to have commenced business on a certain day early in 1847, when Auguste Laurent, another chemist of clairvoyant eye--a poet, too--at that time his co-assistant in Balard's laboratory, showed him, under a microscope, some tungstate of soda, and made him remark that it was a mixture of three distinct kinds of crystals. That was enough. Pasteur forthwith set to work crystallizing tartrates and racemates. It is a pity that the biographer has not seen fit to set forth the motives which determined this momentous selection of a subject for such experiments, for it not only would have explained just what Pasteur's feat was, which is often misstated, but would also have afforded an instructive illustration of how science gets gradually built up. Tartaric acid had been one of the numberless discoveries of that muse-visited apothecary Scheele, made apparently in 1769 or 1770. In 1809 Weiss had first set forth the crystallographic systems, but it seems to have been some ten years before his work attracted much attention. In 1811 Arago had discovered some of the phenomena of circular polarization, which Biot, beginning in 1813, had explained as a rotation of the plane of polarization. Biot had shown, too, that not merely certain crystals, but also certain solutions

and even certain vapors, exhibited this phenomenon. He had found that some slates of quartz cut perpendicular to the axis turn the ray to the right, while other specimens turn it to the left. In 1821 John Herschel had remarked that, of the three kinds of quartz crystals that Haüy had noticed (namely, first, those which can be held vertically so that, of two prismatic faces to the right and left, the former has two small facets impinging upon it below to the left, the latter two similar facets above at the right; second, those which are exactly like the first except that right and left are interchanged; and third, those which are tolerably regular dipyramids), the first invariably rotate the plane of polarization to the left, the second to the right, and the third neither way. This was strong evidence, though far short of proof, that the rotation of the ray was somehow due to some molecular structure of the general nature of a right-handed or left-handed screw; and, what fired scientific imaginations, dynamics was utterly baffled to suggest what sort of force it could be that should regularly produce such an arrangement.

The year (1822) of the publication of Herschel's discovery had been that of another discovery, made two or three years previously, which no single mortal then dreamed had the remotest connection with Herschel's. The manufacture of tartaric acid is a somewhat delicate business, not unlike sugar-refining. Among other places where it was then carried on was the pretty town of Thann in Alsace, which no tourist will have forgotten, if he has been there, on account of its church. A Mr. Kestner was conducting the manufacture there by a regular routine when he one day found, to his surprise, that a certain batch of his acid, made by the regular process, had hardly any tartaric acid in it, but consisted of a new acid, less soluble than the tartaric, though closely similar. He at first mistook this for oxalic acid, but, after having been set right on this point by the Hollander John, he named it after the little town that so well merited fame, *thannic* acid. But when he tried to make more of it he failed, and so did other manufacturers, although some of them found that, when tartaric acid was wanted, this thannic acid was very apt to put in a distinctly uninvited appearance. Before 1826, Gay-Lussac had compared the new acid, which he (we think first) called by its now usual name of *racemic*, with the tartaric, and had found that their chemical behavior was identical, except toward certain bodies with which one or the other seemed to have some underhand relations, where strong contrasts appeared. By 1830 Gay-Lussac and Berzelius had severally proved that the composition of the two acids was one and the same; in Berzelius's phrase, they were *metamers*--the third or fourth instance of this phenomenon to become known. In 1835 Biot contributed the information (destined to prove so important) that tartaric acid and the less complex tartrates rotate the ray to the right.

In 1842, Eilhard Mitscherlich had published the fact that sodium-ammonium racemate is seen under the microscope to crystallize in the same shapes precisely as sodium-ammonium tartrate, although the latter rotates the ray, while the former does not. The following year, his amanuensis, Werther, took for the subject of his dissertation for the doctorate the crystal forms of the tartrates, and this dissertation was frequently used by contempoaray [[sic]] French chemists. Although it appears that Mitscherlich had overlooked the unsymmetrical facets of the sodium

ammonium tartrate, which are certainly small, we can hardly believe that they escaped the careful study of Werther. At any rate, the crystals are geometrically just like those of ordinary Rochelle salts, which must have come under the eye of Pasteur himself, so few things escaped it. Thus, in one way or another, it is altogether probable that Pasteur had been for some time already aware of the unsymmetrical forms of the tartrate. Indeed, we find it recorded that in a conversation held in 1844 with his bosom friend Chappuis, Pasteur repeated *verbatim* a paragraph of Mitscherlich's [[sic]] 1843 paper, to the effect that, although the two salts in question formed crystals geometrically indistinguishable, yet the tartrate rotated the ray, while the racemate did not. Now there would hardly be anything in this so impressive as to cause Pasteur to carry the very words in his mind, if the tartrate crystals were symmetrical. If, however, he was aware that they presented a screw-like configuration, there would be a hope of resuscitating the theory of Herschel to which Mitscherlish's [[sic]] observation would otherwise deal the deathblow. It is evident that he must have cherished the wish to examine the racemates--salts not easily obtainable--to see whether they were not destitute of the unsymmetrical facets. This idea, then, had been in his mind since 1844; and, accordingly, when Auguste Laurent showed him that soda tungstate under the microscope, and he saw that it consisted of three kinds of crystals mingled, we can now understand why he at once set to work making crystals of tartrates and racemates. It had evidently struck him as one of the possibilities that the racemates likewise were mixtures.

Upon how many men's backs was Pasteur mounted when he attained to the making of this memorable experiment which marks the opening of a scientific era to the close of which our great-grandchildren may not be able to look forward! The novelty of the discovery may, for aught one can clearly discern, be attributable to Pasteur's good fortune rather than to his merit. Yet it certainly illustrates his mysterious faculty of rightly guessing at Nature's ways (a privilege upon which he never presumed); and what at any rate was a property of the man alone, and is truly surprising, was his prompt recognition of all the importance of the thing, even to its remoter consequences. We are told that the moment he first got the fact solidly in his grasp, he sprang up quite wild, and ran out, embracing the janitor in the corridor, and, though no man ever more economized minutes, betook himself to the garden of the Luxembourg, where he occupied the rest of that day in setting his disordered ideas to rights. Before long, we find him writing to his beloved Chappuis that this discovery of right-handed and left-handed tartaric acid is destined to lead to the conquest of the most dreadful diseases! What a marvel of clairvoyance! For we now know that, without being aware of it, he had set foot upon the territory of that Unsymmetrical Carbon Atom which sets the pretensions to supremacy of attractions and repulsions at defiance, and carries one of the chemical keys of life.

When Pasteur first exhibited the experiment to Biot, who had been appointed by the Academy of Sciences to report upon the matter--the aged Biot, who, though reared in and sternly attached to the materialistic school of Lavoisier and Lagrange, of Laplace, Carnot, Monge, and Berthollet, had yet never ceased to be Catholic, and in his old age was become even *dévot*--at the critical moment, the old man exclaimed: "Hold a moment, my boy. I have so loved science that this makes me feel faint." Pasteur likewise was Catholic; and if he had not been possessed of that accurate logic which caused him to be as exact as Biot himself in scientific matters, but yet to make room for spiritualistic views of life, it is safe to say that not one of his great discoveries could have fallen to him. Somebody at the Academy of Medicine was one day urging the importance of a certain hypothesis. "Oh," exclaimed Pasteur, "as for hypotheses, we fetch them into our laboratories by the armful." Mind, he did not absurdly pretend to keep them out, as some do; nor want them only in homæopathic quantities, like many more. No, he would order large supplies of them; he would consume them in armfuls. And what would he do with his armful of hypotheses, when it was fetched? He would begin by sorting them over, with unwearied industry, as he had done his right-handed and left-handed crystals. First, those that could be shown upon consideration to involve something in conflict with known facts were weeded out. Then, by a skilful alternations of excogitation and experimentation, the mass of the others would be successively bisected until all but one were discredited, while that one was made gradually luminous with apparent truth. To a less cautious man it would have seemed demonstrated. But now began the main work of the investigation. This one hypothesis which had been winning favor until it was become prime favorite, had now to be treated as an enemy, and ingenuity had to be racked in trying to find something which the hypothesis would require to be true, but which experiment should refute. This fight was always an obstinate one--it sometimes lasted for years; and it was not until every device that could be thought of for bringing the theory to grief had been tried and had been found to avail only to put its truth into stronger light, that at length Pasteur would bring it before the world. Then his scientific friends always urged upon him the view that here the business of the man of science ended, and that he should now pass to another problem; and Pasteur himself might assent to this, in his head, but his heart always overruled such counsels. He *must* see the thing accepted and acted upon, so that here would begin those struggles with all the agencies of conservatism and of night that brought Pasteur to his comparatively early death.

As an illustration of Pasteur's style of dealing with such controversialists, a little episode that concerned Colin may be recounted. Colin wished to impress people with the belief that he knew much more about anthrax than did Pasteur. So, one day when, in a paper before the Academy of Medicine, Pasteur incidentally mentioned that fowls do not take anthrax, Colin jumped up and said that nothing was easier than to give a hen this disease. Thereupon Pasteur said that in two days he should be sending Professor Colin some specimens that he had demanded of him, and, since it was so very easy to give a hen anthrax (which develops almost immediately), he would be very much obliged to him if, in exchange, he would send him a hen dying of anthrax. "You shall have it," said Colin. At the next meeting Pasteur remarked that he had not received that hen, and Colin made some excuse, promising to send it in a few days. But at every meeting, Pasteur recurred to the hen, which was never forthcoming, though

always promised. At length, after this had been going on for months, Colin rose and said that he had found that he had fallen into an error, and that it was impossible to inoculate a hen with anthrax. Upon that, Pasteur remarked that his colleague was now going much further than he had ever done, for he had only said that hens *do not take* anthrax; but that it was possible to give it to them, he would demonstrate by experiment. So the following week he brought to the Academy three hens--one dead of anthrax, a second dying of it, a third recovering from it. Colin was silenced for the moment, but at a subsequent meeting he said, with an air of insolence: "I wish we could have seen the bacteridia of that dead hen, which M. Pasteur took away with him without showing us the necropsy and microscopical examination." Thereupon, Pasteur demanded a committee of investigation, of whom Colin should be a member, so that the skeptic was thus obliged to sign his own refutation. The secret of the matter was that Pasteur had ascertained by experiment what temperature was the highest at which the bacillus of anthrax could live; and, since this had been found to be two or three degrees below the temperature of the blood of fowls, they could not under ordinary circumstances contract the fever, and he was confident that Colin, looking at the matter from quite another point of view, would never light upon the proper way of giving chickens anthrax, which was simply to immerse them in a cold bath until the disease was developed.

This book will serve to correct many a misapprehension. It shows us clearly, for example, that Pasteur was not at all, as he has so often been represented, the exponent of a certain philosophy. The extreme simplicity of his heart, the childlike singlemindedness of his gaze at truth, at one with a childlike insight into things, seem to have prevented the intense concentration of his observational and intellectual energies from becoming a source of fallacy to him. His simple peasant's religion probably did aid him to keep clear of metaphysical entanglements which might very probably have prevented his researches into spontaneous generation; but, except in this alexipharmic way, they did not influence his scientific work in the least degree. He considered the logic of science to be one thing, and the logic of life to be another, and quite independent, matter. Another mistaken notion which has been current among those who busy themselves with Boriogolah-Gha, and which was perhaps fostered by Pasteur's severity of countenance, has been that he was a monster of cruelty, whose treatment of animals and children ought to have been restrained by law. The fact we find to have been that his susceptibility and tenderness of heart were quite excessive. Again, those histories of chemistry that are written with the German determination to concatenate events, albeit at the expense of spinning threads from the substance of the writer's being, tell us that "it was his work upon optically active compounds which led him on to the treatment of biological questions." This is certainly a most rational view, and is open to no criticism whatever, except that it happens to run counter to the facts. Those facts are to the effect that, with a single partial exception, every one of his incursions into biology resulted, not from any inward leading, but against the passionate longings of his heart, from external propulsion and the compulsion of that sense of duty to which his too emotional nature was all his life enslaved.

The whole of this noble life is laid open in these pages. It is a relatively small book, and its greatest fault of which the reader will be sensible is that there is not more of it. In such limits it could not be a Boswellian mirror; nor would the life of Pasteur, who was no show-specimen of a man, lend itself to such treatment. But we rise from Boswell's volumes knowing indeed perfectly what Johnson was, or, at least, how he appeared in society and in intimate conversation, but still perplexed to imagine how he came to be the man he was; whereas, here, Pasteur is not only exhibited but explained. The whole evolution of him from his seventeenth-century ancestors, the nurture and formation of mind and heart, are made comprehensible. The father, especially, stands out distinct as an old acquaintance, no character in novel sharper lined--a veritable contribution to our knowledge of men.

The scientific world in which Pasteur lived is veraciously and vividly portrayed, and this is one of the most valuable features of the work. Many portraits are hit off with as much truth as *chic*. One of them is Biot's. Happy would be the reader who was not familiar with the 'Causeries du Lundi,' since he would have one of the joys of life still to taste; but probably the reader does know that second of two articles in the second volume of the 'Nouveaux Lundis' which describe Biot. Of course, Sainte-Beuve, as he himself confesses, could not estimate Biot. The writer was too sophisticated and fine-drawn for his subject. All that he says is true; but what rating is to be placed upon the different features as elements in the make-up of a scientific man is quite another question. The portrait, on the whole, is not agreeable; in this book, on the other hand, Biot appears in the most charming light, and, since the whole man is viewed, in a much truer light. There are points of interest that both avoid. Gay-Lussac, too, Thénard, Balard, J. B. Dumas, E. Mitscherlich, Liebig, Henri Ste.-Claire Deville, Bertrand, Lister, Virchow, Vulpian, all come upon the scene, with many a younger man. We know not where to point to so truthful and useful a picture of the world of science. If you want to understand the typical man of science, you will find the creature here veraciously expounded. Behind the chief figures there is a life-like and animated background, where Napoleon III., the Emperor Friedrich, Dom Pedro, Littré, Sainte-Beuve, Renan, Alexander Dumas *fils* (very charmingly), Henri Regnault, and other painters will be noticed. Verily, the French still maintain their clear supremacy in the art of making a book, especially a characterism.

The translation is in excellent English. Only rarely have we come across a sentence which, we are persuaded, cannot have been quite rightly rendered, or a word not just the usual scientific expression; and on the whole the translator's work has been done with so much care, and has to so high a degree the rare virtue in a translation of making agreeable reading, that we cannot help feeling particularly grateful to her. The index is excellent, notwithstanding a few misprints. The photographic portrait is a likeness. The get-up of the book is extremely beautiful, with black type and uncalendered paper; the volumes are light to hold; the linen covers, simple and in good taste. In short, the book is an unalloyed delight--the clothing to the senses, the contents to the heart and spirit.

74 (3 April 1902) 273-274: GIDDINGS'S INDUCTIVE SOCIOLOGY

Inductive Sociology: A Syllabus of Methods, Analyses, and Classifications, and Provisionally Formulated Laws.

By Franklin Henry Giddings. The Macmillan Co. 8vo, pp. xviii, 302.

CSP, identification: Haskell, *Index to* the Nation. See also: Burks, *Bibliography*; *List of Articles*; MSS L 159.185-186; MS 1449(draft).

Franklin Henry Giddings (1855-1931) was an innovator in the field of sociology. He introduced into that study the exact methods of other sciences, with statistical profiles of conditions among various classes of people and intensive research into problems of heredity and environment. Giddings graduated A.B. from Union College in 1887 and took his A.M. there in 1889. From 1888 until 1894, he was professor of political economy at Bryn Mawr, succeeding Woodrow Wilson.

Perhaps the best way to convey to our readers an idea of what they may expect to find in this volume will be to give a brief account of the contents of one of its more elaborate chapters, that upon Concerted Volition. We find this chapter to be divided into four parts: (1) The Rise of Concerted Volition; (2) On Cooperation; (3) On the Modes of Concerted Volition; (4) The Laws of Concerted Volition. Under the head of the Rise of Concerted Volition are considered, first, its subjective conditions, and, secondly, its objective conditions. The latter are said to be principally, (1) Developed Communication (the press, etc.), and (2) Association in meetings. Since it is our business to criticise and doubt, we ask ourselves whether this enumeration of the objective conditions evinces a thorough historical study.

Volition is not desire nor passion; it is action. Now does history show that a passionate state of public temper is transmuted into volition by those two influences alone; or does it show that, upon the top of them, some startling act of apparent violence is usually requisite besides? Was our civil war brought from smoke to flame by the attack on Fort Sumter, or the Franco-German war by the supposed insult to the French Ambassador, or our difficulty with Spain by the destruction of the *Maine*?

Under the head of Cooperation, we note that Professor Giddings is not one of those who would make this the essential fact of society. But, on the other hand, neither is he one of those who conceive that it, like responses to stimuli, is divisible into the two grand divisions of cooperative volition and competitive volition, and who opine that these two are pendants to one another; roughly speaking, of equal importance in sociology. So far is he from accepting this view that no chapter of his book is devoted to competition. In all his pages there are but a dozen lines in which the word occurs, and there only as one of two topics. Is this factor of human society, then, not worth any special study? The nature of cooperation is analyzed and found to involve four factors--common interest, perception of what others are doing, intercommunication, and mutual confidence. Its causes are inquired into with the result that men first find that they are virtually cooperating, and then do deliberately what they began to do instinctively or accidentally. Other influences, however, are recognized as secondary. Cooperation is found to have four forms. Two are simple--that in which all do the same thing, and that in which there is division of labor. Two are complex; but the definitions are not very clear, and the

division, though possibly useful, is somewhat artificial. Perhaps we should say that the line of demarcation is artificial; for two classes may be as really separate as two branches of a tree, and yet it may be equally impossible to find any real defining line between them. Distinctions in the extent of cooperation are noticed, as well as the distinction between public and private cooperation.

There follows a somewhat elaborate analysis of the work of cooperation. The practical activities of a social population are said to have four modes, Appreciation, Utilization, Characterization, Socialization. This had been said in an earlier chapter. Appreciation is liking or disliking a thing. By Characterization is meant adapting ourselves to things; by Socialization, adapting ourselves to our social environment. Now, granting that this is a good division of the results of our activity, can it be truly said, or not, that these four results of activity give rise to four forms of cooperative work? To make this out, the author first defines Culture as the cooperative development of appreciation. We must all admit there is such a thing. Perhaps it is what is commonly called fashion or the prevailing taste of an historic period. Professor Giddings says there are three orders of ideas belonging to it--the linguistic, the animistic, and the scientific. Apparently, this is where Science is pigeon-holed in his scheme of society. But if there be anything more distinctly foreign to science than another, it is appreciation, in the sense of liking or disliking. The scientific interest of things disgusting is just as great as that of the most sugared sweets.

We now come to the third division of the chapter, on Modes of Concerted Volition. The like-mindedness is either Instinctive, Sympathetic, Dogmatic, or Deliberative. The instinctive is seen or read of in the squatter. Of the sympathetic, the subjective factors are said to be (1) impulsive like-response; (2) suggestibility; (3) reciprocal consciousness of kind; (4) the suggestions of shibboleth, etc.; (5) imitativeness; (6) contagious emotion. The objective factors are communication with physical conditions, and the crowd. Sympathetic likemindedness especially enriches cultural activities. It is manifested especially in panics, revivals, sympathetic strikes, riots, and similar phenomena. Dogmatic likemindedness is traditional, customary, and conservative. Its subjective factors are belief and deductive reasoning. Its objective factors are the communication of common beliefs and authoritative instruction. Its contribution to cooperation lies chiefly in its conversion of common ideas into differentiated traditions and of common activities into customs. It is marked by partisanship, reliance on legislation in regulate private conduct, deference to tradition, and some other phenomena. Deliberative likemindedness is characterized by critical thought and well-coordinated action. Its chief subjective factors are public opinion and inductive research. Its objective conditions are freedom of speech and of meeting. Without giving all Professor Giddings's items, we may sum them up by saying that he conceives that this kind of likemindedness brings rationality into every department of cooperative work. It is found in the most advanced modern nations. Now follow thirteen pages of tables of questions to be investigated as to the state of concerted volition in any society.

Finally, we reach the fourth division of the chapter, that on the Laws of Concerted Volition. The formulation of these betrays imitation of a bad model, that of

Tarde, who is one of the well-known type of Frenchmen who copy the phraseology of mathematics, as if that possessed, in itself, a secret virtue of rendering vague ideas precise. Thus, we are told that "impulsive social action tends to extend and to intensify in a geometrical progression." Mathematicians, we are aware, speak of one variable increasing geometrically, while another increases arithmetically; but what (if anything) may be meant by saying that a quantity varies in geometrical progression without reference to any second quantity, we must confess transcends our powers to divine. Nor do we see that imitation can be so measured that it is worth while to attempt to say what the mathematical nature of the function is that connects it with another quantity. For the present, such ideas seem irrelevant. At any rate, the meaningless expression must excuse our suspecting that there is nothing more valuable beneath it than the simple remark that where there is a tendency to imitate, the imitation of imitations will multiply imitations. The second law is, that "Impulsive social action varies inversely with the habit of attaining ends by indirect and complex means." Here again, the language is to a mathematician repellent. Is anything more meant than the truism that when men are in the habit of acting reflectively, they are less under the dominion of impulse? If so, why should it not be expressed non-mathematically? So, likewise, why cannot a sociologist content himself with saying that, other things being the same, the older a tradition is, the more it overawes men, instead of laying it down that "tradition is authoritative and coercive in proportion to its antiquity"? The use of mathematical phrases in the other laws leads the reader to suppose a mathematical proportion is intended here. There are several other laws which the reader will find in the book. If this class of writers would study the mathematical theory of measurement-- say, Clifford's 'Analytical Metric'--sufficiently to perceive how such talk as theirs must appear to those who understand what quantity is, we believe that all such phrases would disappear from their pages. Prof. Giddings's work would only be strengthened if this were done.

Perhaps there may be persons who do not reckon the syllabus as a distinct form of literary composition, but there is none which calls for a severer classicality, meaning by that the effect resulting from an abhorrence of the too much, and from long pondering every sentence, as if papyrus were costly and the use of the stylus laborious. It has been the mathematicians who have most excelled in it. A syllabus needs to be a feat of intellectual strength, under pain of sinking to an exhibition of a wooden model, less like science than a painter's mannikin is like a man, by far; for a mannikin may take an expressive pose. The specimen we have given will have convinced the reader that Prof. Giddings is a man of no mean analytic power. Such a scheme as he has drawn up will have its utility in the incipient state of sociological science, even if it is not what a syllabus of sociology ought to be. The index is like the book itself. It is not perfect; but it is much more than pretty good.

74 (24 April 1902) 322-324: THE NATIONAL ACADEMY OF SCIENCES

CSP, identification: Haskell, *Index to* the Nation. See also: Burks, *Bibliography*; *List of Articles*; MSS L 159.182, L 159.218; MS 1450.

WASHINGTON, April 19, 1902.

The April meetings of the National Academy of Sciences, which are held in Washington, are usually more important, scientifically, than the autumn meetings, which are held elsewhere. The relations of the Academy to the Government would naturally render them so; and other circumstances tend to the same result. At the meeting which has just adjourned, twenty-six papers were presented, of which four were biographies of deceased members. Of the remaining twenty-two, five related to Astronomy, as many in a broad sense to chemistry, three each to biology and geology, two each to psychology and logic, and one to metrics; while one was a description of Mosso's station on Monte Rosa.

Of the five papers upon astronomy, one, a review of the present state of our knowledge of the constant of aberration, by Dr. S. C. Chandler, was read by title only, and one upon the coefficients of precession and nutation, by Mr. Ira Ibsen Sterner, was an affair of computation. In a paper upon the planet Eros, Prof. E. C. Pickering expressed the belief that the photographic observations of that planet thirty years hence would yield the best value for the solar parallax; but Prof. Asaph Hall held that the best value would quite possibly be derived from a certain inequality of the moon's motion. Professor Pickering discussed in a very interesting way the irregular variations of light of the same little asteroid, Eros. The period of time during which the planet goes through all its changes of brightness seems to be constant at about two and a half hours, but the amount of the change appears to depend upon the direction from which the planet is viewed. The inference is that the planet rotates about an axis which must be invariable in direction, or nearly so. Yet the planet does not appear to be displaced in position during the period of its rotation; from which it may be inferred that it is about equally bright in all parts. Hence, its variations of brightness would be owing to the angular area exposed to the eye, as the object is viewed from different sides. Thus, the amount of variation, the mean brightness, and the law of variation, as the planet is viewed from different sides, afford means for a study of its form, and of the causes of variation, in a way hitherto unparalleled in photometry.

Professor Pickering also gave an account of the present state of research into the distribution of stars of different magnitudes over the heavens, a work begun by the immortal William Herschel, from which--with such assistance as proper motions and possibly some minor sources of information may afford--must be deduced whatever knowledge of the form and constitution of the star cluster in which the solar system is as a grain of sand upon a seabeach, the denizens of earth may be destined ever to attain.

Professor Hall, who preserves a manifest attachment to analytical devices for getting at facts where observation is more or less in default, discussed the possibility of a comet's being disrupted by gravitation alone, and wrote down the general differential equations of the problem--equations which a skilled mathematician might study for many months without finding any really good way of handling. Professor Pickering remarked that a series of photographs had demonstrated the existence of a repulsive force between parts of a certain comet, at a certain epoch, which was many times as strong as the disrupting force of gravity.

Of the five papers I have classed as chemical, one by Professor Nichols, on the optical properties of asphalt, and one by Professor Morley, on the tension of mercury vapor below the boiling-point of water, were of technical interest. Professor Morley finds that the tension, as experimentally determined, is greatly less than that which had been deduced by extrapolation. Professor Crafts gave a brief statement of the progress he has made since November in the study of the catalysis of comparatively concentrated solutions, using as a catalyser what will be understood by all students of chemistry as $C_6H_5SO_3H$.

Professor Richards, who at present looks after the atomic weights more than others do, has determined that of the very rare potassium-metal cæsium. The number he obtains is about 132.879; with a range of from 132.873 to 132.882. Hitherto, 132.8 has been the number given. He also offered a largely speculative, yet highly useful, contribution to the question of what hypotheses may reasonably be tried in order to account for changes in atomic volume. It seems very extraordinary that, notwithstanding the stupendous mass of chemical facts that have been collected and the very considerable researches that have been made into physical chemistry, we are still almost entirely ignorant of what a chemical compound is, or how its constituents are held together. We are not even sure that they are held together by mutual attractions; for although heat is generally evolved when bodies combine, showing that mutual forces are satisfied simultaneously with the act of combination, yet in some cases, on the contrary, heat is absorbed during combinations--a fact which naturally leads us to inquire whether there may not be other agencies than mutual forces whose action indirectly results in the formation of chemical bodies; and whether, if so, it is not probable that such agencies, whatever they may be, are a factor even of those combinations in which they are aided by direct forces. When hydrogen and chlorine come together to form muriatic gas there is no condensation--or none of which account is commonly taken--although there is enormous evolution of heat. But then, the extreme chemical activity of the resulting gas seems to prove that it is not a fully complete chemical compound. Notwithstanding the tremendous energy with which the ions have approached one another, they are still so active that the case must be very different from what it is when a mixture of hydrogen and oxygen loses one-third of its volume in combining. A body may undergo contraction without combining with a different body; as when aqueous vapor is cooled. In all such cases heat is plenteously evolved, and molecule unites with molecule. It is a question how far the heat of chemical reaction is anything more than heat of contraction. Contraction does not necessarily consist solely in the approach of atoms towards one another. If an atom is a vortex, it must be in itself elastic and capable of deformation; and it may be so even if it is not a vortex. If atoms are compressible, the mutual attraction between two atoms would naturally tend to deform

them. Another question, therefore, to be considered is the relation between such deformations and their valency. It seems to be a fact that highly compressible elements have low valency, while carbon and other elements of high valency are little compressible. All these are interesting and valuable considerations to be borne in mind in the construction of new hypotheses upon which experimental investigations are to be based.

The geological papers were of remarkable interest. Two, by the new President of the Academy, Mr. Alexander Agassiz, related to the mode of formation of coral reefs and to the somewhat peculiar coral reefs of the Maldive Islands, which Mr. Agassiz has lately visited. Instead of being in the trade-winds, these islands are in the monsoons; and, instead of being exposed to the tremendous surf of the Pacific, they are in the gentler Indian Ocean. Darwin's theory of the origin of the coral reefs, which, as is generally known, was that the work of the coral animals began when the Pacific was a shallow sea, and that, as its floor has gradually sunk, the corals have built higher and higher, until they now rise in some cases from the deepest ocean, seems, at any rate, to be definitively exploded. In the first place, it does not seem to be generally true that the sea-floor is everywhere sinking where there are coral-reefs. In the next place, borings show that the coral-rock extends only to a moderate depth. In the third place, an admirable survey was made of the Maldives, about 1830 to 1836, from comparison of which with the existing islands it is found for certain that older and younger atolls exist side by side in the same sea, which is hardly compatible with Darwin's theory. There are several other arguments to the same effect, not so easily stated in a few words.

On the other hand, the differences between the Maldives and the Pacific coral islands, which are very remarkable, are easily explicable on the theory of their formation which is supported by Agassiz. The Maldive atolls are excessively composite. What may be called an atoll, and upon a small-scale map has every appearance of such a formation, may be found to be a composite of tens or of hundreds of partially independent atolls; and the lagoons will contain rich growths of corals, in striking contrast to the atolls of the Pacific. This is attributable to the wide and deep passages existing in the reefs. In the Maldives, as in the Pacific and near Yucatan, manganese nodules were found upon the floor of the ocean, and these are held to be necessarily of eruptive origin. Not limestone, but eruptive rocks form the foundation structures upon which the coral islands are built. Next above these rocks are limestones that are not coralline, but are composed of fragments of shells of globigerinæ. These deposits are raised to such a height as to be moved by the action of the sea and to be further thrown up in places to within 30 or 40 fathoms of the surface, at which depth the work of the coral animals can begin. After that, everything seems to depend upon the action of the sea, and so upon the prevailing winds. The nature of the changes which individual islands have undergone since 1830 are extremely interesting and significant. A coral reef is not necessarily circular. That type occurs but seldom. It is more apt to be shaped like a pear or a gourd. According to circumstances, in the Maldives, in course of time, a faro, or island, or several, may be formed upon the reef; quite commonly two, where the seas of the two monsoons strike tangentially. These islands put out spits in one direction and the other as the wind changes, and thus the two islands at length join together and the one island takes the form of a broken ring.

Professor Osborn summarized the evidence that North America and Eurasia were, during the Mesozoic and Cenozoic, joined in such wise as to constitute a single zoological realm. In regard to the latent homology of which he told us last October, he has since found that this had been long ago remarked by Owen, and named homoplasy. Professor Osborn presented part third [[sic]] of a monograph on the bombycine moths of America. This part relates to the Sphingicampidæ.

Professor Cattell read a paper on psychophysical fatigue, in which he showed that Mosso's mathod [[sic]] of experimenting upon lifting a dead weight from the ground, the amount of work being measured by the product of the mass into the height through which it was lifted, is open to the objection that there is much effort before the weight is stirred. By experimenting upon pulls against a spring, Professor Cattell has entirely avoided this objection; and the consequence is, that the strange anomalies of Mosso's results now completely disappear. Three papers by Mr. C. L. Peirce, on Color Sensation, on the Postulates of Geometry, and on the Classification of the Sciences, were read by title.

Mr. William Sellers read a paper on the compulsory introduction of the metric system into the United States. This referred to a bill which the doctrinaires of the metric system, with their usual utter neglect to ascertain the state of facts, have introduced into Congress requiring every bureau of the Government (including the Bureau of Weights and Measures, the Mint, the Bureau of Construction of the Navy, etc.), from and after a given date, to use no other than the units of the metrical system for any purpose whatsoever. That this would render every plan in the Navy Department worthless, that it would be impossible to repair the engines of any ship, are among the smallest inconveniences which would result from carrying out the purposes of this fantastic measure, which is, however, urgently pushed by Gen. Comstock. At present the American screw system is in use generally upon the Continent of Europe. There has been, of late years, some attempt to revolt against it; but if America only maintains her position, those countries must ultimately come to the inch for mechanical purposes, because it and its modes of subdivision are more convenient and advantageous for those purposes. America is now, said Mr. Sellers, fifty years in advance of the rest of the world in mechanics. Really, to discard the inch would be to surrender our preeminence, which could not, under those circumstances, continue, such advantage should we be at once putting into the hands of England.

The newly elected President of the Academy, Mr. Agassiz, gave a brilliant reception on Wednesday at the Arlington. President Roosevelt received the Academy on Wednesday morning with the utmost grace.

74 (29 May 1902) 433-434: The Story of the Vine.

By Edward R. Emerson. G. P. Putnam's Sons. 1902. 8vo, pp. 252.

CSP, identification: Haskell, *Index to* The Nation. See also: Burks, *Bibliography*; *List of Articles*; MS L 159.194.

Edward Randolph Emerson was a widely recognized authority on wines and viniculture. The work here reviewed became a standard in the field, charting the history of winemaking from ancient times to modern methods. In 1902, Emerson published his *Lay Thesis on Bible Wines* which caused a great deal of controversy. In that book, he refuted the view held by temperance advocates that there were two different kinds of wine in biblical days, the fermented and the unfermented. Emerson labored diligently to organize American winemakers, and he served as president of the American Wine Growers Association and as treasurer of the American Champagne Makers Association.

For a diner with Duke Humphrey and a pint of red wine this volume may serve to enliven his repast, and can be agreeably read through in the sitting. It runs on like a brook in springtime, busy in an idle way, and plentiful of those anecdotes of which nobody with any pretension to wine-wisdom can decently be ignorant. It runs on without cessation or stint in its aqueous stream, unchoked by hard facts, its limpidity unmuddled by any ideas but the simplest. That we should do well to drink, in careful moderation, of pure and good American wines, that American winegrowers should strive for quality and not be too much in haste to make money, are the chief burdens of its song. The author seems to have lent his European trips an interest by occupying himself with the study of œnology, and, without troubling us with technical details, he distils for us the moral of his experience. He is quite confident that there is no better cure for the dram-drinking habit than the use of pure and wholesome wine. He remarks how the population of the Moselle valley believe in the health-giving quality of their wines, which they drink so copiously, and reminds us that the most celebrated of their *crus* (and we may add the least obtainable) owes its name, the Berncastler Doctor, to that faith. Beside that fact he might have noted the cautiousness with which the neighboring Burgundians taste of their richer vintages. It is enough to walk along the streets of Macon or Dijon to be persuaded that the most delicious wines are not always the most innocuous. They are too high and good for human nature's daily food. As a rule, expensive wines enjoy great renown for their wholesomeness;

but perhaps the two facts that one does not drink them unmeasured, and that after them one does not relish an inferior wine, have had their influence on this dogma.

The principal question about any book is how far it accomplishes its purpose. This one is, like a light wine, intended chiefly to be agreeable and wholesome enough; and these ends it fully attains. It has as much body as Moselblümchen. Nobody ought to require great accuracy of statement or thought in such a work. One can be assured that it contains entertaining matter, and that nothing of that sort has been ruled out without a better reason than that its connection with the vine is slender.

75 (10 July 1902) 31: NOTES

CSP, identification: Haskell, *Index to* The Nation. See also: Burks, *Bibliography*; Fisch and Haskell, *Additions to Cohen's Bibliography*; MS L 159.193.

We have, perhaps, not devoted to Delta's 'Charades' (Cambridge, Mass: Charles W. Sever & Co.) as assiduous a study as so profound a tome requires. The impression it has left is, that Delta deals with his charade-words as popularizers do with the secrets of natural science, and aims "to make them patent to the meanest capacity." But this may be a mere effect of superficial reviewing; for not half the book has been worked through. So, for aught we know, there may be tests here fit to measure every grade of charade sagacity. A certain neatness is observable in these problems, hardly rising to elegance. No mortal could for one moment mistake them for poetry. Yet experiment has shown that they will beguile an idle hour very well.

75 (10 July 1902) 36-37: A Study of the Ethics of Spinoza.

By Harold H. Joachim. Oxford: Clarendon Press; New York: H. Frowde. 1901. 8vo, pp. 316.

CSP, identification: Haskell, *Index to* The Nation. See also: Burks, *Bibliography*; *List of Articles*; MS L 159.191; MSS 1451, 1451(s) (draft).

Harold Henry Joachim (1868-1938) was a British Hegelian of some note. He was educated at Harrow and Balliol College, Oxford. He was fellow and tutor in philosophy at Merton from 1897 until 1919, and he served as Wykeham professor of logic there from 1919 until 1935.

For the Cartesians and the intellect of Continental Europe at the time Spinoza wrote, geometry, as presented in the first book of Euclid's Elements, was the very exemplar of what science should aim at being. From our modern point of view, geometry is a triumph of instinctive good sense; but it is not entitled to be called a positive science, since it makes no critical examination of the truth of its assumptions. Its abridged style of exposition will answer for a subject where an almost unerring instinct guides us; but in itself that style is utterly vicious as not half setting forth the thought. In all this, Spinoza felt himself obliged to imitate Euclid in order to maintain his pretensions to science. His philosophy was deep, out of the common ways of thinking, and intelligible only from peculiar points of view so that it would have been difficult enough to understand had it been ever so lucidly presented. Clothed, as it is, in the garb of Euclid, the 'Ethic' is one of the most enigmatical books that ever were written. The most curious circumstance about it is that a logical writing which Spinoza left unfinished at his death, shows that the Euclidean form of the 'Ethic' was utterly untrue to the author's own way of thinking. Those assumptions which, when stated as definitions and axioms, seem to come from nowhere, bursting upon us like bolts out of the blue, had really been subjected by Spinoza to the critical examination of a sort of inductive logic.

The 'Ethic Demonstrated in Geometrical Order' appeared posthumously in 1677. It had been ready for the press since 1675, and had been seen by some of Spinoza's inmost friends in earlier forms at least ten years before. Attacks upon it began promptly upon its publication, if not earlier. For half a generation they were plentiful. This was the period of its ill-fame, during which good Thomas Moore could exclaim, "So, then wood, mire, lead, and dung are God!" and when Toland invented the inappropriate word *pantheist* on purpose to describe its author. Long before the middle of the eighteenth century it had come to be regarded as settled that Spinoza had merely developed a few ideas that had been thrown out by Descartes, and that his notions had been definitively exploded. This opinion received something of a shock when, in 1780, Lessing declared himself a Spinozist, although mistakenly; and an interesting discussion of Lessing's supposed Spinozism followed between F. H. Jacobi and Moses Mendelssohn. Then, Herder confessed to a sort of Spinozism, as later did Goethe. The system could not, after such events, well sink back into obscurity. About 1816 H. C. W. Sigwart began that more careful and critical study of what Spinoza really did mean to which many writers have contributed in a fuller and fuller stream of literature to this day, until the study may now almost take rank as a special branch of science, Spinozology. Certainly, the last word about it has not yet been said. Perhaps the problem is insoluble. At any rate, beyond a certain point, any opinion that can at present be put forth must rank as a merely personal one. In 1852, an important book by Spinoza was brought to light, together with many significant letters. Since then, the data of the problem remain unaugmented.

The present work consists of a commentary upon the 'Ethic' based upon a very careful and well-considered study of the whole of the writings of

Spinoza, together with those of the cream of his commentators, some two dozen in number, not, however, including several that were well worthy of attention, such as the book of Berendt, and Friedländer, that of Höffding, and one or two extremely important papers in the journals. In the reviewer's opinion, some of the interpretations of Mr. Joachim are entirely inadmissible; yet the book will be esteemed by all students of Spinoza. For beginners in the study, it has one particular merit, that of recognizing the accidental character of the geometrical form of the 'Ethic,' and, in the main, ignoring that form in the exposition. To go into further particulars about the truth of the rendering of Spinoza's thought would only be setting one unproved opinion against another.

We will venture, however, upon one remark, which is overlooked by Mr. Joachim and most of the other commentators. It is that, educated in Holland when he was, the notions of philosophy which Spinoza first received, and which, in the main, form the bed-rock upon which he built, naturally would come, and it is easy to see that they did come, form the Dutch reformed peripatetics of that time, Burgersdyk, Heereboord, and the others. There is no trace in Spinoza of any direct acquaintance with medeæval scholasticism. The Dutch Aristotelians were influenced to a considerable, but limited, extent by scholastics. This bed-rock of conceptions was overlaid in Spinoza's mind by pretty wide philosophical reading. The influence of Bruno, Descartes, Hobbes, for example, is plainly discernible. But the main

features of his philosophy are consistent with Aristotelianism slightly modified, and not at all so with the other doctrines which subsequently influenced him.

There is no philosopher of whom there is more quite unprofitable study than Spinoza. In order to study any philosophy with due profit it is necessary to understand it; and, in order to understand it, it is necessary to begin by placing one's self in the state of mind of the author at the beginning of his speculations and follow out the course of his thoughts. We ought not to say that there is no good at all in pursuing philosophical reflections with a book open before us which we do not understand; but in order to study the works of a given philosopher with the profit that they may afford, there is no other way than that we describe. First of all, then, in order to study Spinoza with profit, it is requisite to soak one's mind in the general way of thinking of the Dutch Aristotelians, and this will be far from being in itself the valueless study which our Cartesian prepossessions (for such still prevail) lead us to imagine it to be. But all those philosophers wrote in Latin, and their works are untranslated. Next it will be indispensable, in order to understand Spinoza, to take minute account of all his writings, the rest of which are not composed in the enigmatical style of the 'Ethic.' One of these writings is in Dutch, the rest in Latin. Under these circumstances, the fact that some half-dozen English translations, partial translations, and paraphrases of the 'Ethic' are in circulation (one, at least, in a second edition), while none of the other necessary aids are extant in English, is sufficient proof that the 'Ethic' is chiefly read by numbers of persons who never penetrate the Euclidean husk to reach the real meat of the book beneath. The truth is, it is the style that attracts readers to the book, the style of Euclid. To an unprepared student who cannot control his curiosity about the book and is determined to dip into it, we would say, At least, take Mr. Joachim's book along with Spinoza's text; and remember that even then, in the opinion of the majority of Spinozologists, you are only making a moderate approach to the true philosophy of Spinoza.

75 (17 July 1902) 53: NOTES

CSP, identification: MS L 159.193. See also: Fisch, *First Supplement*.

Prof. A. A. Atkinson's 'Electrical and Magnetical Calculations' (D. Van

Nostrand Co.) is clear, concise, and thoroughly well thought out. Any young man who is interested in the application of electricity and is fortuante [[sic]] enough to procure this little volume at an early period of his studies, will find he gets more service out of it per page than from almost any other on his shelves.

75 (24 July 1902) 71: NOTES

Attributed to Peirce by Fisch in *Second Supplement*. "Thorpe" here is an error; the author of the book under review is Forsyth. Peirce had reviewed the first three volumes of Forsyth's *Theory of Differential Equations* at 71 (19 July 1900) 59, so this note is, therefore, probably by Peirce.

The fourth volume of the 'Theory of Differential Equations,' by Prof. Thorpe (Macmillan Co.)--virtually the fifth, since his 'Treatise' ought to be regarded as an introduction to the 'Theory'--is perhaps the last volume, though one more, at least, ought to follow. It is the most practical of the four, since it relates to ordinary linear equations. Nothing quite equivalent to this work exists in any language; nothing at all in our language supplying the same need of all who use differential equations.

75 (24 July 1902) 71: NOTES

This note is probably by Peirce in view of his authorship of a longer review of this book at 75 (2 October 1902) 273.

Drude's 'Theory of Optics,' translated by C. R. Mann and R. A. Millikan (Longmans, Green & Co.), is the only work in our language, if not in any language, which presents the modern theory in the form of a text-book. No one whose business it is to be acquainted with the modern theory of light can afford to leave this work unstudied.

75 (24 July 1902) 79: Studies in Physiological Chemistry, Sheffield Scientific School.

Edited by R. H. Chittenden. Charles Scribner's Sons. 1901. 8vo, pp. 424.

Qualitative Chemical Analysis.

By Albert B. Prescott and Otis C. Johnson. 5th ed. entirely rewritten. New York: D. Van Nostrand Co. 1901. 8vo, pp. 420.

Victor von Richter's Organic Chemistry.

Edited by R. Anschutz. Translated by Edgar F. Smith. 3d American from the 8th German ed. Philadelphia: P. Blakiston's Sons & Co. 2 vols., 8vo, pp. 625, 671.

The Elements of Physical Chemistry.

By Harry C. Jones. Macmillan Co. 1902. 8vo, pp. 565.

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

Albert Benjamin Prescott (1832-1905) was graduated M.D. from the University of Michigan in 1864. The following year he was appointed assistant professor of chemistry at that school. In 1876, Prescott was named dean of the school of pharmacy, remaining in that position until 1884, at which time he was appointed director of the entire chemical laboratory.

Edgar Fahs Smith (1854-1928) was graduated B.S. from Pennsylvania College at Gettysburg in 1874. He then toured Europe, where in 1876 he was graduated Ph.D. in chemistry and minerology from the University of Göttingen. As both a translator and an author he produced a number of books and monographs on chemical subjects, notably: *Classen's Qualitative Analysis* (trans., 1878), *Von Richter's Inorganic Chemistry* (trans., 6 eds., 1883-1900), and *Elements of Chemistry in Lecture Form* (7 eds., 1908-18).

Russell Henry Chittenden (1856-1843) was a Yale-educated chemist. He developed an interest in physiological chemistry while studying at the University of Heidelberg in 1879. He returned to Yale in 1880 and earned his Ph.D. in physiological chemistry. Chittenden was later elected to the National Academy of Sciences and for several years served as president of the American Physiological Society.

Otis Coe Johnson (1839-1912) both studied and taught at the University of Michigan School of Pharmacy. He served as professor of chemistry from 1873 until 1911. The work here reviewed, which he co-edited with Prescott, became a standard text and guide in qualitative analysis.

Our readers would be far from curious to see minute dissections of technical books performed before their eyes. The volume from the Laboratory of Physiological Chemistry of the Sheffield Scientific School contains some two dozen experimental researches into different points of that science, well sustaining the high character of their home. They are preceded by an interesting catalogue of all the publications of the Laboratory since its institution in 1875.

The 'Qualitative Analysis' of Profs. Prescott and Johnson is a handbook every way modern, distinguished by its completeness. An introduction of 26 pages sets forth concisely the principles of the art, including sections on dissociation, the periodic system, etc. All the rare metals, excepting only such things as Polonium and Radium, are included; and the selection of acids treated is large. The authors, it is plain to see, have searched the "literature" of the subject exhaustively, and have not neglected experimental verification, wherever it was possible. Sufficient details are given as to precautions to be observed, and there are useful explanations of why peculiar phenomena are as they are. Earlier editions are well known, but this present work is largely new and a great improvement. In every respect, it will be most serviceable, especially to the accomplished chemist.

Von Richter's 'Organic Chemistry' has been so often rewritten that one might begin to fear effects of new wine in old bottles. Nothing of the sort appears. With what ingenuity a vast ocean of facts is packed away into these few, thirteen hundred pages, can be learned only by personal inspection. The digest is so uniform that one knows precisely what one may expect to find and where to look for it. The exposition is so luminous that a student who has once acquired the chemical memory can, with a reasonable amount of study, bring away all the outlines and many of the details. The first volume is chiefly given over to the fat-allied bodies; but, after they have been treated, room remains in the same volume for the carbohydrates, the albuminoids, and all that is beyond the ken of chemical theory to-day. That leaves the second volume for bodies of so-called cyclical constitution exclusively. This second volume carries authority as to fact beyond what one would have a right to demand of a compilation. Not only has it been executed with great *general* chemical judgment, but special parts have been revised by men specially acquainted with special groups of substances. The German editor's skill in exposition gains in this second volume, too; and the American translator manipulates his own language with more accomplished neatness. This work may be recommended as quite the most suitable that exists for almost every person who may desire to undertake almost any kind of study of organic chemistry. Common sense will set certain limits to this statement; and there are no others. If every person whom the description

fits were provided with a copy of these volumes, the sales would run up into quite modern numbers.

As to the subject of Professor Jones's book, one can go further. Every man of education would like to know something of organic chemistry; but every such man needs to know the principles of physical chemistry in about the measure in which Professor Jones's volume sets them forth. There are really two departments of science which join hands in this volume; first, the new Physical Chemistry, which takes account of the time of reaction, of dissociation, of the influence of mass; second, that somewhat ill-defined congeries of studies which used to be called Chemical Physics. Why should not these two phrases be allowed to stand, each in the peculiar sense in which it is usually employed? The newer subject captivates by its unexpected and prompt explanations of most important facts for which one had not seen any reason. It looms large from the viewpoint of to-day's science. Professor Jones's exposition of this branch is in almost every point excellent, and in the most important parts is everywhere more than excellent--often admirable. The older subject, the knowledge of which is fully as valuable, although at the present time it is not so active a factor in the metabolism of science, evidently does not interest the author so keenly. This is unfortunate, because this division is naturally placed first, and among the students of this work there will be some who, though somewhat at sea in chemistry, have pretty distinct and tolerably accurate notions of what good logic is and what bad. If a Clerk Maxwell could have been found to perform this more perfunctory part of Professor Jones's task, what a masterpiece might the two have made of it!

75 (31 July 1902) 94-96: ROYCE'S WORLD AND THE INDIVIDUAL

The World and the Individual. Gifford Lectures [on Natural Religion], Delivered before the University of Aberdeen. Second Series: Nature, Man, and the Moral Order.

By Josiah Royce. The Macmillan Co. 1901. 8vo, pp. 480.

CSP, identification: Haskell, *Index to* The Nation. See also: Burks, *Bibliography*; *List of Articles*; MS L 159.194; MS 1461 (draft).

Professor Royce's second and concluding volume discusses questions of intimate interest to everybody. It is more persuasive than the first, of which it enhances the significance. The design of the whole now comes out--to introduce into the Hegelian philosophy of religion such rectifications as must result from recognition of scientific conceptions worked out during the century now completing itself since that philosophy first appeared. Of these new conceptions, some are psychological, some logical; but the chief of them are the new mathematical ideas which cluster about that of an infinite multitude. Mathematicians, perhaps, still linger on the stage, who, in their best days, used to be quite positive that one cannot reason mathematically about infinity, and used to feel, like the old lady about her total depravity, that, this cherished inability being taken away, the bottom

would fall out of the calculus. Such notions are obsolete. Various degrees of infinity are to-day conceived with perfect definiteness; and the utter misapprehension of the metaphysicians about it, above all of Hegel, glares. As a first serious attempt to apply to philosophical subjects the exactitude of thought that reigns in the mathematical sciences, and this, not on the part of some obscure recluse whose results do not become known to the public, but on that of an eminent professor in a great university, to whom the world is disposed to listen with attention, Royce's 'The World and the Individual' will stand a prominent milestone upon the highway of philosophy.

Our space will permit only the most salient features of Professor Royce's theory to be roughly sketched. "An Idea is any state of mind that has a conscious meaning." In reference to meaning, logicians have never failed to recognize "quod fere in omnium ore celebre est, aliud scilicet esse quod appellative [*i. e.*, adjectives] *significant*, et aliud esse quod *nominant*. Nominantur sigularia [[sic]], sed universalia significantur." So John of Salisbury, Abelard's pupil, expresses the distinction. That for the most important signs the signification is intrinsic, the denotation (*quod nominant*) extrinsic, is generally recognized. Professor Royce marks the distinction by the terms Internal Meaning and External Meaning. He conceives of the internal meaning in a special way. Another writer, a quarter of a century ago, proposed this maxim: "Consider what effects that might conceivably have practical bearings we conceive the object of our conception to have. Then our conception of those effects is the *whole* of our conception of the object." Carrying this pragmatistic spirit a trifle further, Professor Royce holds that the internal meaning of an idea is a Purpose, instead of regarding it, with his predecessor, as a germinal purpose. This purpose--obscurely recognized, since not all to which it will lead is foreseen; partially fulfilled in being recognized, since a purpose strives first of all to understand itself; but mainly unfulfilled, since it would not remain a purpose after fulfilment--is the internal meaning, or signification, or depth, of the idea. The purpose is vague--anything that refers to the future is more or less vague; and a sincere purpose to do a thing "right now" actually does it. The purpose is to do a thing under certain circumstances. Completely to define these circumstances, it would be necessary to give a biography of the purposer from birth, without any omission. The purpose is to do something in order to produce certain results. Completely to define the result accomplished must involve a complete representation of the agent's future life. In short, the complete fulfilment of any purpose, which alone is the external meaning of the idea, is no less than the entire life of the thinker. The reviewer will say, for clearness, that neither these nor the majority of the author's positions have been satisfactorily shown to be true (in the reviewer's opinion, but the author holds them to be perfectly demonstrated). The principal of them may rank as verifiable hypotheses possessed of the plausibility and other qualifications that render them eminently worthy of further examination, even if an engine of rare usefulness must be drawn away from other work for the purpose.

That the object of an idea, then, its external meaning, is of the nature of a sign could hardly be gainsaid. But Professor Royce finds it not only a sign but an idea; not only one idea but a "concrete" idea in the Hegelian sense, and that, not relatively, but perfectly, and so of the nature of life; and not only life, but an entire life. "The Being of the real object of which you now think, means a life that expresses the fulfilment of just your present plan."

But suppose the reader to ask, How can an idea, so microscopic a piece of a life, contain within its implication a distinct feature corresponding to every feature of the entire life of which it is only a part? This difficulty is happily removed by the author in a way which ought to be instructive to those metaphysicians whose horizon is limited by the walls of a theological seminary. He resorts to Gauss's conception of an *Abbild*, which has played a great role in mathematics. That is to say, he likens the idea representing the entire life to a map of a country lying upon the territory of that country. Imagine a map of England, absolutely perfect in its minutest details, to lie upon the soil of England, without covering the entire country it maps. Upon this map would be shown the very ground where the map lies, and the map itself, in all its minutest details. In this map of the map, the map will be shown again; and so on endlessly. Here, then, will be a part fully representing its whole, just as the implicit meaning of the idea is supposed to represent the entire life. It is to be noticed that, each successive map lying well inside the one which it immediately represents, unless there be a hole in the country which a ringshaped [[sic]] map encloses, the endless series of maps will converge to a single point, which represents itself throughout each and every map of the series. In the case of the idea, that point would be the self-consciousness of the idea. An idea, being a state of mind with a conscious purpose, must evidently be self-conscious.

There, then, would seem to be a beautiful and a needed (though not a complete) confirmation of the aptness of the metaphor. Yet here the author recoils, and refuses to admit the continuity, or even the analytic continuity, of the map. He insists that no representation represents itself; a hard saying, which exact logic appears directly to refute. "The world of Self," he says, "whatever continuity of internal structure it may, in some aspects, possess, is, in its principal form of expression, embodied in a discrete series of ... individual expression.... Experience at any moment shows how I am conscious of my . . . approaches to selfhood in any way in the form of a discrete series in which one stage . . . is followed by the next." The argument which supports this assertion is not assented to upon all hands. If it be true, there are but two alternatives: either, notwithstanding this, in the respects in which an idea represents an entire life there is continuity, or, at least, a higher infinity than that of the simple endless series; or else the doctrine that to an idea a reality must in some shape correspond must fall to the ground.

According to Professor Royce, an idea fails of being a Self only because it is general and not perfectly determined; which the reader may deem a dark saying unless the sword of Hegel is invoked to cut the knot. Its implicit or germinal inward meaning would, then, be a little Self, representing the entire life as its external meaning. In a similar way, the Self of the man is perhaps included within a larger Self of the community. On the other hand, the man's Self encloses intermediate selves--the domestic Self, the business Self, the better Self, the evil spirit that sometimes usurps his sovereignty. Of course, the system of delineation for

the larger and for the enclosed selves will be different. Here the author draws support from the psychological doctrine of what he calls the "timespan," a doctrine which, so far as it has really been placed beyond important doubt, amounts to little more than that our image of the events of the few seconds last past is, or is very like, a direct perception, while our representation of what happened a minute or so ago is relatively of a far more mediate character. This phenomenon had already been seized upon by several idealistic writers as affording a refutation of dualism; but there is no better way of appreciating the large calibre of Royce's thought than by comparing their styles of putting this idea to the service of metaphysics with his. He imagines that the greater selves will naturally have vastly longer time-spans than the lesser selves. Now a consciousness whose time-span, or "specious present," or "empirical present," as it is variously called, was a thousandth of a second or a thousand years would not ordinarily be recognized as a consciousness at all by a human observer of its external manifestations. The timespan of the All-enclosing Self must cover all time; and this gives a sort of support to the imagination if we wish to reconcile foreknowledge and free will after the manner of Boethius, St. Augustine, and others.

Every reality, then, is a Self, and the selves are intimately connected, as if they formed a continuum. Each one is, so to say, a delineation; with mathematical truth we may say, incongruous though the metaphor is, that each is a quasi-map of the entire field of all the selves, which organic aggregate is itself a Self, the Absolute Idea of Hegel. So far as a philosophical conception can be identified with God it is God.

All reasoning goes upon the assumption that there is a true answer to whatever question may be under discussion, which answer cannot be rendered false by anything that the disputants may say or think about it; and further, that the denial of that true answer is false. This makes an apparent difficulty for idealism. For if all reality is of the nature of an actual idea, there seems to be no room for possibility or any lower mode than actuality, among the categories of being. (Hegel includes modality only in his Subjective Logic.) But what, then, can be the mode of being of a representation or meaning unequivocally false? For Hegel, the false is the bad, that which is out of harmony with its own essence; and since, in his view, contradiction is the great form of activity of the world, he has no difficulty in admitting that an idea may be out of harmony with itself. Prof. Royce, however, seems almost to resent the idea that anybody could suppose that he denied the validity of the distinction of truth and falsehood. He is fairly outspoken in pronouncing sundry doctrines false (a word Hegel hardly uses), even if we do not quite hear his foot come down; and nothing does he hold more false than the usual form of stating the distinction now in question, namely, that a true proposition corresponds to a *real matter of fact*, by which is meant a state of things, definite and individual, which *does not consist merely in being represented* (*in any particular representation*) *to be as it is*, [[sic]] For example, if I dream that I find I can float in the air, this matter of dream is not matter of fact, for the reason that the only sense in which I can float in the air is that so my dream represented the matter. Now Prof. Royce offers to demonstrate by necessary reasoning

that the statement--or, as he expresses it, that "to be real means to be independent of ideas which relate to that being"--is false. His argument to this effect will serve as a sufficiently characteristic, but rather favorable sample of his general style of argumentation.

Having given us to understand that he is going to disprove the proposition, he opens his argumentation by declaring that he does not know what the proposition means. Thereupon, he proceeds to propound a general maxim of procedure for all cases in which one has to refute a proposition without knowing what it means. It is to begin by assigning to it its "most extreme form." This certainly does not signify the most extremely defensible meaning, but rather the most extremely indefensible meaning that the language will bear. The proposition having been refuted in this extreme sense, it will only be necessary afterwards to argue that other interpretations make no essential difference. This maxim, one would suppose, would prove very serviceable to anybody who should have any large amount of that sort of refutation to perform. In accordance with this maxim, Prof. Royce begins by assuming that realists hold that no idea in the slightest degree determines the real object of it, whether causally or in any other manner. Whether this does not overstep the limits of admissible interpretation, seeing that a realist who meant this would deny that any promise can really be kept, or that any purpose can influence the real result, the reader must say. At any rate, it would not seem to be a difficult position to refute.

Now in order that he may get the realist where he wants him, there are two acknowledgments which Professor Royce endeavors to extort from him. To bring him to the first, the author assumes the principle that all causal action is reciprocal, or of the nature of reaction. This is evidently contrary to popular opinion, which holds that while the past has exerted some efficient causality upon the future, the future cannot have any *effect*, in the strict sense of that word, upon the past; and that while the future may have influenced the past by final, or ideal, causation, the past cannot possibly influence the future as the aim of the future. The reader may judge whether a realist of so extreme a type as that which Professor Royce has set up would or would not admit that the real object of an idea cannot have influenced the idea, in the absence of any attempt on the part of Professor Royce to prove his general principle of reciprocity. If he would not, old-fashioned logic (which Hegelians, it is true, hold in high contempt) would pronounce the attempted demonstration to be a bald *petitio principii*.

In order to extract the second acknowledgment from the realist, Professor Royce produces an argument which would seem to have as much force for one kind of realist as for another. He supposes two objects, B and R, to be related to one another as the realist supposes the Being, or the real object of an idea, and the Representation, in the form of an idea of that object, to be related; and he undertakes to define the relation between them. "The definition in question," he says, "is, as a mere abstract statement, easy." One would think so. The realist simply says that B is not constituted by its being represented in R; that is, he says that the fact that B is as it is, would be logically consistent with R's representing it to be otherwise. But in place of this easily comprehensible relation, what fantastic attempt do we find at the definition that was pronounced to be so easy! Professor Royce will have it that the realist holds that the relation is such that no matter how R may be metamorphosed, it is logically possible for B to remain unchanged. In such a sense, what two things in the world are independent? Change the problematic madness of Hamlet into the pacification of the Philippines, and it will become logically inconsistent with the continuance of great disturbances there. But change the doubtful *representation* by Shakspere [[sic]] that the fictitious Hamlet was unhinged into the *representation* that the Philippines were pacificated in 1901, and it will not have the slightest logical consequence for the real state of things. The truth is, that Professor Royce is blind to a fact which all ordinary people will see plainly enough; that the essence of the realist's opinion is that it is one thing to *be* and another thing to *be represented*; and the cause of this cecity is that the Professor is completely immersed in his absolute idealism, which precisely consists in denying that distinction. It is his element, and there is total reflection at its surface. That, however, is what Professor Royce asks the realist to admit as a premise. The conclusion which he deduces from it is that if B is linked as cause to any determination of R, there must be a *tertium quid* by the mediation of which the causation takes place. Now the premise is absurd; and the formal rule is that from an absurd premise every conclusion must be allowed to be logical; that is to say, it is needless to dispute its logicality, the premise being false. The argument, therefore, cannot be called formally bad; nor can we object that a few lines below, in a restatement of the conclusion, B's being linked as cause gets changed into B's having any causal *or other* linkage.

Professor Royce, armed with his wrong definition of realism, goes on to a dilemma to show that, whether the realist says that real things are one or are many, he equally involves himself in contradiction. But, although the characteristics of his style of argumentation become even more prominent in that dilemma, the exigencies of space forbid our following him further. But we should like to say one word to this powerful and accurate thinker who has been so completely led astray in his argumentation by his Hegelian logic: Absolute idealism depends, as Hegel saw that it did, upon assuming that position at the outset. If your refutation of realism is addressed to students who are already absolute idealists at heart, we will not undertake to say whether it will be serviceable for the development of that doctrine, or not. But if it is addressed to realists themselves, it must conform to the logical principles recognized by realists, or be nugatory. Now you know very well that realists do not admit that matter of fact can be apodeictically demonstrated. You ought to know, and surely you do know, that if you drive them into a corner, they will simply modify their admissions so far as may be necessary to avoid self-contradiction, and that from the very nature of apodeictic proof it is absolutely impossible to close off such escape in arguing about matter of fact. The history of the doctrine of parallels illustrates what logic shows to be necessarily the state of the case. But the question of realism is a question of hard fact, if ever there was a hard fact; and therefore your method must be revolutionized if you are ever to convince any master of logic.

75 (7 August 1902) 115: NOTES

CSP, identification: Haskell, *Index to* The Nation. See also: Burks, *Bibliography*; Fisch and Haskell, *Additions to Cohen's Bibliography*; MS L 159.199; MSS 1452, 1452(s) (draft).

--In the *Annales* of the Paris International Congress of 1900 that discussed Comparative History, the report of the fifth section, concerned with the History of Science, contains nothing of greater consequence than one might expect as the printed residuum of such a meeting, after the separate publication of several contributions. The leading spirit of the section was evidently M. Paul Tannery. Heiberg prints for the first time the Greek text of Anatolius on the first ten numbers, a Pythagorean Christian tract by an Alexandrian Peripatetic of the third century. It contains an otherwise unknown fragment of Heraclitus, and has a certain interest as probably representing a lost book of Nicomachus. Several of the longer papers of the volume relate to mediæval medicine and surgery; and there is one by Nicaise on the state of anatomy and physiology at the time of Vesalius and Harvey. Tannery prints nine letters addressed to the celebrated Pere Mersenne, who, at the time of Descartes, acted as a medium of scientific intelligence. Sigismund Günther gives an interesting account of the different compromises between Ptolemy and Copernicus that were proposed in the sixteenth and later centuries. In a paper by André Lalande, the pendulum of opinion about Francis Bacon swings to so wide a deflection that Descartes is almost represented to have borrowed from him the idea of explaining all physics on mechanical principles. It is the 'Valerius Terminus' to which appeal is made for support of this. Galitzyne communicates letters and pictorial sketches sent to Catharine II. in 1783 by the Russian Minister in Paris, to inform her about the ballooning exploits of Montgolfier and Charles. There are some interesting papers about Comtism; for the rest, not very much to attract other readers than minute students in special departments of the history of science.

75 (21 August 1902) 153-154: THORPE'S ESSAYS IN HISTORICAL CHEMISTRY

Essays in Historical Chemistry.

By T. E. Thorpe. The Macmillan Co. 1902. 8vo, pp. 582.

CSP, identification: Haskell, *Index to* The Nation. See also: Burks, *Bibliography*; *List of Articles*; MS L 159.197; MS 1453 (draft).

The subjects of these eighteen essays, barring one or two, are all distinctly great. There is one upon Boyle, concerning whom nothing quite adequate has ever been written. Half-a-dozen relate to the better threshed-out subjects of chemists of Lavoisier's time. Five discourse of precursors of modern chemistry, Faraday, Graham, Wöhler, Dumas, and Kopp; two of modern chemists, Victor Meyer and Mendeléef; one of Canizzaro, who was not quite modern. The remaining three are sketches of general nineteenth-century or Victorian progress in

synthetic chemistry, in British chemistry, and in technological chemistry. With the exception of Canizzaro, and perhaps Kopp, there is none of these subjects that is not a mine of instruction, although not much that is novel remains to be added about Cavendish, Priestley, or Lavoisier. One can hardly say that the author has extracted from any of his subjects its full lesson, unless it be from that of Victor Meyer. The portraiture of his character, life, and works is fine. To appreciate it, however, the reader needs to be familiar with the chemistry of the aromatic bodies.

Or nearly as much value is the long chapter about Graham, occupying nearly ninety pages, and composed of two independent papers. One would not antecedently suppose that so much not generally known remained to be said of so prominent a figure. It is shown that Graham was one of those who enter upon their experimental careers filled with some idea sufficiently fecund to produce great and inexhaustible work. This, in Graham's case, was the idea of atomicules in motion--an idea whose career has, at this day, perhaps not fairly been initiated. He was a pupil of Thomas Thomson, a disgracefully bad analyst, but a good vehicle for the dissemination of Dalton's doctrine, and, what is more, a discerner of men and of scientific genius. Thomson was impressed by Graham from the very first, and from the first treated him with something like deference. Such conduct on the part of a man of European celebrity--for such Thomson was--without a doubt greatly stimulated the development of his pupil's powers. Let the world pay its debt of gratitude to Thomson for this. The modern doctrine of valency virtually contents itself with a modification of the Berzelian idea that chemical forces are polar; and no doubt there is some truth in that, so far as it goes. Only it is pertinent to inquire how they come to be so. But Graham conceived that even the atoms of hydrogen, much more those of the other elements, were composed of many parts moving without absolute constraint. We need not say how much reason has since been found, some of it recently, for accepting that opinion. But it is difficult to imagine how, say, a thousand atomicules of hydrogen can be held together in one molecule without a chance agglomeration of a thousand and ten or more in another. Why should it be, then, that all the molecules of hydrogen, or any other simple gas, are equal or nearly so, as Graham's experiments proved them to be? As soon as anybody shall so much as suggest an adequate hypothesis to explain that fact and the observed approaches to Prout's law, the real quality of Thomas Graham will begin to shine out. Over his thought from boyhood there towered a misty shape which was always becoming compacted into the consistency of that puissant Jinn, the whole vast kinetic theory of matter. These reflections, and many others, suggested by Dr. Thorpe's account of Graham, lead us to accord a high esteem to that chapter.

The first chapter is on Boyle. An account of Boyle's broader conceptions, with their previous history, and that of their subsequent influence upon philosophy and upon science (which, in both directions, has been greater than is generally known), would have formed no small contribution to our comprehension of ourselves. But all that, if the author was able to furnish us with it, has been crowded out by long extracts from one of the most intolerably long-winded writers who

ever abused our language. To the prolixity of garrulous conversation, Boyle added a pedantical choice of words whose only good excuse might have been that they effected compression. The 'Reflexions on a Broomstick' is hardly a caricature. Dr. Thorpe understands Boyle the chemist very well; but technical chemistry is quite the smallest side of that original and prepotent mind.

The essay on Scheele makes use of Nordenskiöld's book; and since few of us have time to go through that volume, this meagre account of its contents will not be unwelcome. This is not the only case in which Dr. Thorpe has drawn the substance of a sketch from a single well-known source, quite legitimately.

Cavendish, Priestley, and Lavoisier, who, together, occupy considerably over a hundred pages, have all of them been pretty thoroughly understood for a long time. Of course, an irresistible inclination will induce a Frenchman to overrate Lavoisier or misground his appraisal. This manifests itself even in Berthelot, himself a great man, and as fair a Frenchman as could be found. Lavoisier never made an original experiment in his life, but he saw which the decisive experiments were, he repeated them with such high precision and such circumspect precautions that confidence was commanded, he coordinated them with a clear, scientific logic of which no contemporary was the master; thus he gave the death-blow to a theory of extraordinary vitality, and he enthroned in its place a theory, partly his own, which was mainly true. The account which Dr. Thorpe gives of him and of his work is perfectly just. Dr. Thorpe has no other explanation to offer of Priestley's adherence to phlogiston than the old one, that Priestley was incapable of placing himself in an unwonted point of view. We doubt whether the history of Priestley's opinions in metaphysics and theology sufficiently supports that explanation. There seems to be something worth considering in another view that has been broached, namely, that the vitality of the doctrine of phylogiston [[sic]] was, in some occult or subtle manner, due to the circumstance that it was true in its way.

One of the more striking of the essays is that on Jean Baptiste Dumas. It is largely drawn from Hoffmann's *Eloge* in the *Berichte*; but not so the following:

"If," says the author, "we compare the chemistry of to-day with that of the stirring times when Dumas, half a century ago, was matched almost single-handed against the German school--against such Titans as Berzelius, Liebig, Wöhler--we are amazed at the wealth of material which has been opened out. The change, thus directly or indirectly traceable to the labors and conceptions of Dumas, is as great or even greater than that achieved by the overthrow of the Phlogistians. If Lavoisier was the author of the first French Revolution in Chemistry, Dumas was the creator of the second."

Unquestionably, Dumas was a very considerable genius; and there is none of the many men of science who came into contact with him during the period of his secretaryship of the Academy of Sciences but must remember the delightful old gentleman with warmth and with a sense of obligation. Nevertheless, truth is truth; and even so much as a claim that his relation to the present conception of a chemical compound molecule was that which his baptismal name might suggest, would be definitely inadmissible. But was he not the author of the doctrine of substitution?

So Dr. Thorpe represents him to have been; and there is just this much truth in it, that, while it had previously been known that chlorine could drive hydrogen from various compounds, it was Dumas who first showed that this happens if almost any organic body is treated with chlorine; and he added that an analogous reaction would occur if the body was exposed to the action of bromine, of iodine, *or of oxygen*. But when Berzelius attacked him for maintaining the monstrous proposition that chlorine could behave like hydrogen, Dumas replied:

"M. Berzelius m'attribue une opinion précisément contraire à celle que j'ai toujours émise, savior que, dans cette occasion, le chlore prendrait *la place* de l'hydrogène. Je n'ai jamais dit rien de pareil.... Si l'on me fait dire que l'hydrogène est replace par du chlore qui joue le même role que lui, on m'attribue une opinion contre laquelle je proteste hautement, car elle est en contradiction, avec tout ce que j'ai écrit sur ces matières."

Thereupon he goes on to say very explicitly that the opinion attributed to him is neither more nor less than a ridiculous (*outrée*) theory which has been urged by one Laurent. He means Auguste Laurent, who, in fact, had said:

"Un composé organique constitue un ensemble, formé par la réunion d'un nombre d'éléments simples ou composés, éléments que l'on peut remplacer à volonté dans ce composé par des groupes analogues [the context shows that he means what we should now call groups of the same valency], sans altérer la physionomie générale, l'harmonie, ou *le type* de ce composé."

It was this Laurent who, in consequence of the bitter resentment of Dumas against the man who had ventured so to modify *his* doctrine, died, as has often been said, and with little exaggeration, substantially of inanition and almost at Dumas's door, and who, by the above sentence, voluminously expounded and elaborated, has established his clear title, with clear-headed chemists, to being considered the father of the leading conception of modern chemistry.

The truth is, that in all the several scenes of life in which Dumas was busy, he ever showed himself to be an adroit man. He introduced the word "substitution" into chemistry, while loudly proclaiming his rejection of the only meaning that word was fitted to convey. *He*, one of the Forty--*he*, the fastidious selecter of words--did this. So, then, should it turn out, in process of time, that there really *was* a substitution, it would be remembered that he had first said so, and his disclaimer would be forgotten (just as we see Dr. Thorpe forgetting it); while, should it turn out that there was *no* substitution, the inappropriateness of the word would be lost sight of when it had become familiar, and at all events his declaration would be borne in mind. Just so, in regard to Prout's law, Dumas managed to take up such a position that both parties claim his support. But he was a little too adroit on the day when he bethought him quietly to appropriate certain results of Liebig's, for he found himself straightway transfixed, nailed to the counter, without a reply to make. As for his waging war, single-handed, against all the chemists of Germany, it is as easy to understand Dumas's looking upon the situation in that light as it is St. Thomas Aquinas's anticipating as a chief felicity of heaven the triumph of looking over the battlements upon rivals to sanctity frying

beneath; but the one is as near a true religious aspiration as the other is to a true scientific sentiment. He was so far from arguing single-handed, that he tardily adopted, and endeavored to pass as his own, Laurent's "outrée" doctrine.

We have endeavored to impart to our readers a sufficient prenotion of the matter of this volume. We ought to add one word of warning against not a few small misstatements of facts of chemistry and chemical history, mistakes utterly trivial and hardly worth more than the mere mention, were it not a curious psychological fact (or must we, nowadays, say a *psychical* one?), that so excellent a chemist as Dr. Thorpe should make so many indisputable slips. We will just mention one as a specimen. The author has been saying how brilliant for England in chemistry were the first two decades of the nineteenth century. So they were, since Davy's work and Dalton's were done then. But he goes on to give two or three instances by way of proof, and among these he names the discovery of acetylene, although it has only recently been brought to the attention of chemists and non-chemists that acetylene was first discovered in 1836. But the author probably had it in his mind that acetylene was discovered by Davy, momentarily forgetting that this Davy was not Sir Humphry, but his obscure protege Edmund, otherwise known principally for some early activity in applying chemistry to agriculture.

75 (11 September 1902) 209-211: PAULSEN'S KANT

Immanuel Kant: His Life and Doctrine.

By Friedrich Paulsen. Translated from the Revised German Edition by J. E. Creighton and Albert Lefevre. With a Portrait. Charles Scribner's Sons. 1902. 8vo, pp. xix, 419.

CSP, identification: Haskell, *Index to* The Nation. See also: Burks, *Bibliography*; *List of Articles*; MSS L 159.197, L 159.208, L 159.210; MS 1454 (draft).

Of the three "parts of the soul," as they used to be called, Sensibility, Energy, and Thought, Kant was decidedly deficient in the first and by no means a hero in the second. That he was genuinely great in thought would seem to be overwhelmingly proved by Vaihinger, from the manner in which he has commanded the attention of all subsequent thinkers. Yet very many of these thinkers, if not most of them, would hold Kant to have been wrong in almost every one of his arguments. Let us reexamine his capacities in sensibility, energy, and thought.

As for sensibility, we call to mind a single passage in Kant's writings as having been admired æsthetically. It is the well-known parallel between the starry heavens above and the moral law within. That genuine eloquence must be attributed to this passage is sufficiently attested by the general admiration it has excited, for it clothes an ethical doctrine which, nakedly presented, would be repugnant to the majority of admirers of the passage. This seems to be the one passage in all Kant's writings that can really be called fine. Professor Paulsen is of opinion that Kant might have become one of the great writers of Germany. He gives sundry reasons

for thinking so--such as that Kant's style is marked by great emphasis, that he has a goodly stock of fine phrases and no little ingenuity in bringing them in, and that his "waggishness" is strongly marked. There is no reason to suppose that Kant might not have made a good writer, like anybody else, had he been trained under a good master. Any exaggeration of tone would have been repressed, his elegant extracts dismissed, and his wit subjected to good taste. A good writer, of course; but whether a great writer, or not, is one of those questions of which Kant himself would have said that they transcend the limits of possible experience. Measuring his sensibility by known facts, we find that his style, though it has qualities which excite the gratitude of a student who many times rereads and deeply ponders every section, is devoid of any other grace than that of keeping to the point--is not even always grammatical. Kant never contemplated matrimony, and apparently was never in love. He never had an unreasonable attachment. Though for years he was a distinguished lecturer on physical geography, he was never moved to go to look upon a mountain, never even tramped to the neighboring sea, never saw other town than his own little East Prussian capital. In sensibility, then, Kant must be rated as below the average.

Energy is of two kinds; that which reacts upon the outer world, and that which inhibits one's own impulses. We must be careful not to mistake a deficiency of either kind for an excess of the other. Kant was never moved to any enterprising action, nor even to making any troublesome observations. On the occasion of his being reprimanded for his religious philosophy by minister and king, the little fellow meekly promised to say no more upon the subject. It is true that he was seventy years of age; but then he was a bachelor, without dependents, and by far the most illustrious person in Germany, not even perhaps excepting Goethe. He declares, in a well-known paper, that he has read Swedenborg's 'Arcana Cælestia.' If he really did that, it was the most heroic effort of his life. He would have been better employed in reading Hume's 'Essays' or 'Treatise on Human Nature,' which concerned him more than any other books in the world; but Paulsen is quite right in saying that he never did read Hume in the sense of apprehending his meaning. It must be granted that Hume is an enigmatical writer. His so-called "easy" writing makes hard reading enough. It allows the superficial student to read into it ideas that the author never intended to express, especially the student unacquainted with what was going on in the English world of letters of the period. But take the ordinary traditional logic. A schoolboy can master that. Yet Kant's pamphlet on the 'Falsche Spitzfindigkeit' is devoted to setting forth as a novel discovery of Kant's own the very doctrine of the reduction of syllogism taught in every book of traditional logic. The only real novelties it contains are two or three absurd blunders. Kant probably did read Baumgarten's 'Metaphysica'; but one must doubt mightily whether he ever really read any other book of philosophy.

These things are most significant. In self-control Kant appears to be a prodigy. A man more systematic than he would not be reckoned among the sane. When, during his afternoon constitutional, he reached a certain corner, the good people of Königsberg would pull out their watches, not to see whether Herr Professor

Doctor Kant was on time, but to see whether their watches were going right. His more important books were put together, as he expressed it, architectonically. That is, just as architects, until recently, used to insist upon designing buildings upon an arbitrary plan supposed to have certain merits, but not determined by the purposes which the buildings were to subserve; just so, Kant would enslave himself to an elaborate system of divisions and subdivisions--*Haupttheile*, *Theile*, *Abtheilungen*, *Bücher*, *Hauptstücke*, *Abschnitte*, and *Paragraphen*--laid down beforehand, not arising from the peculiar character of his theme, but supposed to be dictated *à priori* by reason and to be derived from the idea of pure reason. Such method either bespeaks extraordinary self-control or a singular defect of *élan*. Several circumstances besides Kant's apparent inability to read a work on philosophy somewhat incline us to the latter hypothesis.

At any rate, it was exclusively in the way of thought that Kant can be deemed great, if he was great at all. There are different kinds of thought: there is mathematical thought, that works by diagrams; there is the thought which, from observing a fragment, divines a whole; and there is logical analysis. Kant was certainly not a mathematician. In scientific theorizing, however, he was decidedly strong. He is accounted by astronomers the author of the Nebular Hypothesis. In his younger days, he was a physicist; and he always remained a physicist who had taken up philosophy (naturally, less strikingly so as his powers declined), contrasting in this regard with Fichte, Schelling, Hegel, and Schleiermacher, not to speak of Baader, Günther, etc., who were all theological students, and as strongly with Jacobi, Fries, Krause, etc., who came to philosophy by the route of theology; and even more or less, with Schopenhauer, Herbart, Beneke, and all the others before Fechner and Lotze, who, at any rate, breathe rather the atmosphere of the seminary than that of the laboratory. Every scientific reader feels the philosopher of Königsberg to be of his kindred.

When we think of the stupendous amount of close study which intellectual men of every stripe have bestowed upon Kant, and when we ask ourselves, What is it, then, which has attracted all this attention? We are led to answer, it was his power of constructing a theory, which is the kind of intellectual feat that marks the man of science--the Young, the Faraday, the Darwin. We shall not, of course, be misunderstood as saying that constructing theories made any of them the great men that they were, any more than it did Kant. As a scientific man beneath the skin, Kant is comparatively free from the besetting fallacy of the philosophers, which may be described, without exaggeration, as consisting in producing arguments to prove a micron, at most, and in concluding a light-year, at least. Kant, perceiving in some measure this universal fault of the philosophers, was naturally led to his evident ambition to be the arbiter of philosophical disputes. But he could have exercised this office only in the weak manner of the Eclectics, allowing so much weight to this consideration and so much to another diametrically opposed to it, if he had not fortunately been gifted with a great strength in logical analysis, that enabled him at once to do full justice to the arguments and tendencies of both sides, and to make both contributory to a third unitary conception. Yet even his logical analysis would not have sufficed, if it had not been for a supereminent share in a characteristic that may be remarked in all the more powerful scientific intellects, the power of making use even of conceptions that resisted his logical analysis, and of drawing from them nearly the same conclusions as any clear mind would have done that had analyzed them. We cannot, in a few words, make our meaning very clear; but one might say that an ordinary intelligent mind has an upper layer of clear thought, underlaid by muddled ideas; while in Kant's mind there appears to be a pure solution down into those depths where daylight hardly penetrates. He thinks pretty correctly even when he does not think distinctly.

The volume under consideration contains a careful account of Kant's place in history, of his life and character, and of his philosophy, by one of the most accomplished and popular of the German philosophers of to-day. It is not a suitable guide for a beginner in Kant. The 'Critic of the Pure Reason' is, perhaps, as wholesome a book as a student of force could find with which to begin the study of philosophy. But the only accompaniment to it that is advisable at first is a textual comment. Such books as Paulsen's are best left for later perusal. We need not say that the student must not allow himself to imagine that in going through the 'Critic' for the first time without preparation he can understand Kant entirely, far less duly estimate him, until he has read the discussions which led up to the 'Critic.' Deeper students will find this volume interesting and convenient. It leaves hardly any question of metaphysics untouched.

We have said that it is drawn up with care. We will now give two specimens of its inevitable inaccuracies. In summing up Kant's historical position, Paulsen says that to have cleared the ground and pointed the way to a poetic naturalistic pantheism as the fundamental form of the conception of the world, is the imperishable service of Kant. This not only forgets that Lessing introduced "poetic naturalistic pantheism" the year before the 'Critic' appeared, and that its propagator, Goethe, was uninfluenced by Kant, but conveys the idea that Kant's importance is exclusively theological and poetical; and accordingly, in the summary of his philosophy, his scientific writings are left unmentioned, and, throughout, his relations to theology are made infinitely more important than his relations to what is generally called science. But Kant, as we have said, was, on the contrary, mainly a man of science--not oblivious of aspirations towards God, freedom, and immortality, but yet dwelling in the realm of experience; and his theory of cognition--its general design, at least, and some of its corner-stones--still stands, as far as scientific thought is concerned, firmly established. Under these circumstances, and since he himself was not a pantheist, it is unjust to sum him up as a forerunner of what he condemned.

The following is an example of another kind of inaccuracy. On p. 147 we read:

"How synthetic judgments *à posteriori* can have actual validity seemed to him to be no problem at all. If he had raised the question, it would have shattered the whole structure of the 'Critique.' He would have been forced to reply that there can be no such judgments; synthetic judgments *à posteriori* are a *Contradictio in adjecto*."

Compare this with the following from page 8 of the 'Critic of the Pure Reason':

"In synthetical judgments, I must have, in addition to the concept of the subject, A, something else, X, upon which thought may react, in order to cognize a predicate, B, as belonging to A, although not a part of it. In empirical, or experimental, judgments there is no difficulty in fulfilling this condition. The X is merely the complete experience of the object of the concept A, which is but a part of that experience. [Having expanded this remark very clearly, he concludes]: Experience, then, is that X which extends beyond the concept A, and upon which the possibility of the synthesis of the predicate B with the concept A is founded."

We thus find that Kant does consider the very problem which Professor Paulsen says he does not consider. He does not, indeed, consider it in all its branches, but he does so quite sufficiently to show that his answer, had it been more complete, would have borne not the slightest resemblance to the absurdity which Professor Paulsen says he would have been drawn into. The answer that Kant gives is easily susceptible of natural expansion to cover every possible phase of the question, quite in opposition to the theological logicians of Germany. Kant looked upon such questions as a clear physical thinker would--that is to say, in a manner of which Professor Paulsen has not the least conception.

We will add one word concerning the title of the 'Critik der reinen Vernunft.' The word *Kritik* already existed in German, meaning a critical writing. But Kant declares, with all his emphasis, that that is not the word he uses. He borrows a word from the English of Hobbes and Locke, and spells it (in his first edition) with a C. He used it, as the English writers had done, to mean the art or science of criticising. Since this word *critic* exists in our language in that meaning, and is, in fact, the very word Kant borrowed, while the word *Critique* is English, if at all, only in the sense against which Kant almost violently protests, the first word of the title should be restored to its English form 'Critic' in translation.

In the best philosophical use of English words, "reasoning" is a well-known operation of a mind, and "reasoning power" (or, less well, "reason") is the faculty of performing it. "The Reason" is a totally distinct faculty by which we are supposed to know the truth of first principles. "Reason" means nothing more nor less than conformity to the best result of deliberation. Kant, not being insane, did not propose to criticise Reason. Neither did he criticise the Reasoning Power, unless to approve it in one paragraph. But what he chiefly criticised and had reference to in his title was the faculty of knowing first principles, *The* Reason. Consequently, his book, the 'Critik der reinen Vernunft,' is a work concerning 'Critic of the Pure Reason.'

75 (18 September 1902) 229-230: The Principles of Logic.

By Herbert Austin Aikins. Henry Holt & Co. 1902. 8vo, pp. 489.

CSP, identification: Haskell, *Index to* The Nation. See also: Burks, *Bibliography*; *List of Articles*; MS L 159.212; MS 1455 (draft).

Herbert Austin Aikins (1867-1946) was born in Toronto and was the son of the first dean of the Toronto Medical School. After taking an A.B. degree from the University of Toronto in

1887, Aikins became an instructor in philosophy at the University of Southern California. In 1891, he was graduated Ph.D. from Yale University and in that year began a series of one-year appointments at Yale, Trinity School, and Clark University. From 1894 until retirement in 1937, Herbert Aikins was professor of philosophy, psychology, and mental hygiene at Flora Stone Mather College of Western Reserve University.

Amid the flood of logical treatises that gushes from the press, passes under our notice, and soon vanishes in the ground-mist, this book is distinguished by some slight differences from the uniform color of the general mass. We seem to detect in it the beginnings of a more living and effective thought. It is by no means a scientific treatise devoted to the exposition of the theory of scientific reasoning. It is didactic mainly, and extremely elementary--spoon-food for the puerile mind. The author is influenced most by Venn's helpful (though often mistaken) work with a somewhat similar title. He has studied Mill and Jevons. He has picked up an idea or two from Lotze. But the authors he seems most to esteem are of the grade of Whately and Minto. He declares his intention of treating logic in a thoroughly objectivistic manner.

Perhaps our readers would like to know what is meant by an objectivist treatment. We will explain. Every man who sits down to write a logic, unless he is to content himself with copying right and left without regard to consistency, as many do, must ask himself, "What am I to set up as the criterion by which to distinguish good reasoning from bad?" Good reasoning is reasoning which conduces to the truth: and every single act of reasoning feels itself to be pursuing a general method conductive to the truth. This indisputable fact suggests the objectivistic criterion according to which, in order to determine whether a given reasoning is good or bad, one must examine whether, from the nature of things, the method which the reasoning understands itself to follow must lead to the truth, supposing that there is any such thing as attainable truth, and supposing that it leads to any conclusion at all; and the method must lead to the truth in the sense in which the reasoning understands that this method leads to the truth. With more or less consciousness and consistency, oftener less than more, the mass of English logicians stand upon that ground. The Germans, on the other hand, regard our instinctive judgments of rationality to be the ultimate criterion. That which a man's reason deliberately approves is *ipso facto* good reasoning. This is the subjectivist position.

The subjectivist says to the objectivist, "So you distrust the instinct of rationality? To what, then, do you resort in order to establish or overthrow the validity of reasoning? Why, to reasoning itself! How, then, do you know that this critical reasoning is valid? Even if I grant you that it is so, what informs you of this? Nothing but the instinct of rationality, *par die*! So, then, you only come back to our criterion after an aimless beating round. You are compelled at last, willy-nilly, to place your trust in the instinct of rationality. Besides, your critical reasoning requires premises. On what do they rest? On experience, say you. Experience presents itself in the form of percepts; but premises are propositions, and so of a different nature from percepts. Moreover, not *all* your premises can be perceptual judgments. One, at least, must be more general. What can assure you of the truth of that, if you refuse to trust to instinct?"

To this the English logician replies, "I am not skilled in your artificial instruments of thought; so, if you please, we will consider the very facts themselves. You speak of man's having an instinct of rationality. I suppose you mean that a man is sometimes constrained to believe a thing without being able to say distinctly how he is constrained, and that among the things which he is so constrained to believe is that numerous consequences are reasonable. If this is your meaning, then when you talk of 'trusting' to the instinct of rationality, you mean that we should acknowledge that we cannot help regarding a consequence as perfectly evident. When we are under such compulsion, it is altogether out of our power to have the smallest shade of doubt; and we seem to see plainly that we never could be brought to doubt it. To call the recognition that a proposition seems beyond all possible doubt and perfectly evident 'trust,' and to give this as a reason for following my first impressions of rationality in cases where I can and do entertain doubt, is as if you were to say to a man who had been compelled to surrender his watch to a highwayman, that, since he has already 'trusted' the highwayman with his watch, he will behave inconsistently if he refuses to go bail for him when the police catch him. A man's voluntary conduct cannot be compromised by anything beyond his control. To ask whether a given method of reasoning is good or bad is either mere pretence, or it implies a doubt whether the reasoning be good or not; and such criticism is idle unless the man has some control over his reasonings. In fact, it is next door to absurd to say he has a doubt and yet can exercise no control over his thought. The moment he sees room for doubt, he does doubt; and to ask him to decide according to the prompting of instinct, which is, more probably, a loose association of ideas, is to ask him to surrender reason altogether."

That is the way in which the dispute stands at this moment; the German not yet, apparently, having taken cognizance of what has been said only out of Germany. We can, however, imagine the discussion to be carried a few steps further, somewhat as follows:

The German: "But after all, why not trust to the instinct of rationality? You know that instinct is incomparably keener and surer than reason."

The Englishman: "Would you consel [[sic]] the entire abandonment of reason? If you are only thinking of cases in which the dicta of instinct are imperative and unconditional, I have already admitted that then there is no option, for the proposition which instinct so urges upon us appears evident. But what if uninstructed instinct should say, 'I incline to prefer method A to method B,' while whispering, as she always does in those cases if you listen well, 'but, really, you must not trust to me'; while reason, on her side, makes it clearly evident that, in the long run, method B must lead to the truth, and there is nothing in the nature of things to prevent method A from going hopelessly wrong?"

The German: "You English are for ever talking of the 'long run.' What is this 'long run'?"

The Englishman: "It is a succession of instances, as they present themselves in experience, indefinitely long, yet not endless."

The German: "Well, I might, perhaps, admit that pure science, which can afford to wait a century, or, if need be, five centuries, or indeed any time not endless,

before coming to a conclusion, might trust to reason's 'long run,' provided instinct, on reconsideration, compels it to do so. But how about an individual who has to make up his mind promptly concerning a practical proposition, and be ready to act when the occasion arises?"

The Englishman: "The individual should consider that his case is of insignificant importance in comparison with countless other cases."

The German: "Yes, yes; we know how lofty is your British altruism, especially when it happens to be altruistic altruism; but the practical man had better do what his instinct teaches him is reasonable without regard to your objectivistic logic."

The Englishman: "Equally without regard to your subjectivistic logic; unless he is to do what he likes not because he likes, but because your system teaches that he likes to do as he likes."

This explanation will serve to show what the strictly objectivistic tratment [[sic]] is that Professor Aikins professes to adhere to. We shall not pretend to be neutral. We think that the objectivistic method is the only way of enlightening instinct; and that wherever logical rules are to be of any use, they must be rules of objectivistic logic. But Mr. Aikins professes to strike an original path only in applying the objectivistic method to necessary reasonings, where no rules of logic are needed--for that which is evident cannot be made more so. Moreover, he chiefly has in view practical reasonings where men must be guided by instinct or lose all claim to good sense. Neither is Mr. Aikins at all consistently objectivistic. If he had not told us so, we never should have discovered that he made any endeavor to be so. He talks of "the three laws of thought" in the good old way, the somewhat obsolescent way. He speaks of "the test of Inconceivability"; thus making it something to be trusted to, and thus surrendering the key to the English position. He ought to have said, "I will not make inconceivabitlity [[sic]] a *test*; but whatever, with all my efforts, I cannot entertain the smallest doubt of, I won't pretend to criticise." The best we can say is, that the author appears to recognize, in some measure, the futility of the common books, and makes some endeavors to do better. Whether it be from immaturity, insufficient reading and reflection, or lack of vigor of thought, he has not succeeded to any striking extent. But he writes in an agreeable style and make himself intelligible to every boy. There is a list of exercises filling nearly sixty pages of fine print; and many of these are very good indeed. There is also a sufficient index.

75 (2 October 1902) 273: **The Theory of Optics.**

By Paul Drude. Translated from the German by C. Riborg Mann and Robert A. Millikan. Longmans, Green & Co. 1902. 8vo, pp. xxi, 546.

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

Robert Andrews Millikan (1868-1953) was awarded the Nobel Prize in physics in 1923 for experiments done at the University of Chicago in 1909-12 in isolating the electron and for his research in 1912-15 on the photoelectric effect. His work on the electron was carried out by measuring the fall in electric fields of tiny drops of oil.

This is a thoroughly modern text-book, not handbook, of its subject. The fact that it carries a preface by Professor Michelson is a sufficient assurance of its scientific character. As any really to-day's treatise on any branch of general physics must do, it supposes an elementary acquaintance with the calculus; and its pages bristle with equations which signify hard work for the reader and full compensation therefor. Descriptions of instruments and statements of experimental results are summary, not to say skeletal.

Still, the essentials are given. As a matter of course, optics is here treated as a branch of the theory of electricity. The work consists of four parts (which nobody can suspect a German professor of simply numbering consecutively)--the first, on geometrical optics; the second, on the phenomena of light treated in the general manner of Fresnel; the third, on the electrical theory and the optical characters of crystals, metals, etc.; the fourth, on radiation. The assignment of one principal part to radiation is a mark of modernity; and the distribution of space among the four parts is significant. Taking the second part, which is a trifle under 150 pages, as a standard of comparison, the geometrical part is three-fourths as long, the electrical part is half as long again as the second, the discussion of radiation one-third of the same unit. The book is not at all overloaded, like so many German books; nor is there any undue partiality to any particular topic. The matter is judiciously selected, and contains nothing more than ought to be familiar to everybody whose business or amusement it is to be acquainted with the theory of optics. The fault, if there be any, is rather in the omission of interesting topics. The student will derive from it all the pleasure that there is in the sense that one's thoughts are guided by a master-mind.

75 (23 October 1902) 329-330: AVIATION

Travels in Space: A History of Aerial Navigation.

By E. Seton Valentine and F. L. Tomlinson. London: Hurst & Blackett. 1902. Pp. 328.

Aerial Navigation: A Practical Handbook on the Construction of Dirigible Balloons, Aerostats, Aeroplanes, and Aeromotors.

By Frederic Walker. D. Van Nostrand Co. 1902. Pp. 151.

CSP, identification: Haskell, *Index to* The Nation. See also: Burks, *Bibliography*; *List of Articles*; MSS L 159.211-212; MS 1456 (draft).

Should the reader entertain the theory that books showily printed on calendered paper, with abundant illustrations, more or less splurgy, are not particularly apt to prove attractive reading, we cannot say that 'Travels in Space' will tend to convince him he has been wrong; albeit the nature of its subject renders it readable. It would be unreasonable to expect it to present the strange experiences of ballooning with all that life and reality that Mr. Bacon's 'By Land and Sky' did, last year, because that was a masterpiece. A glance at the volume will inform him that it is

not a work of research, like Mr. Chanute's 'Progress in Flying-Machines,' from which, by the way, it copies extensively, almost verbatim, without acknowledgment, and to which it is vastly inferior in all respects in which the two works come into comparison, excepting in a very few details. It is later in date by more than eight years, and its scope is wider. The field still remains open, however, for a really workmanlike history of aeronautics.

The building of an airship is as much more difficult than the building of a steamer, like the *Kaiser Wilhelm II.*, as the latter is than the throwing of a trussed suspension-bridge across the North River. Consequently, Mr. Walker has judged a duodecimo of a hundred and fifty pages to be the proper sort of volume in which to convey the airship-building art. For, once problems reach a certain pitch of difficulty, and the more profound they are, the less is the knowledge generally thought requisite for attacking them. The first chapter of Mr. Walker's "practical" book is entitled "The Laws of Flight." The only statement of a law which it contains is the following:

"When a moving body is directly opposed by a *vis mortua*, such as a pressure or resistance like that of gravity, the measure of such *vis mortua* required to neutralize the force [of the moving body] and bring the moving body to rest must form the basis of the measurement of the force."

Thus, the persons who Mr. Walker assumes are to undertake the construction of airships, and for whose encouragement he has provided his handbook, are supposed to be in need of this information; while further dynamical science, he would appear to presume, is quite beyond their comprehension. Later in the book, it appears that they are persons who need to be told what a sine and cosine are. What Mr. Walker fails to tell them, but, on the contrary, implicitly denies, is that, with such an outfit, they will make great fools of themselves if they undertake the building of an airship.

Vessels to sail the air are of four types. The first is that of a machine with ascensional power, but with no motor. Such is a simple balloon or other aerostat, a kite-balloon, or a system of attached balloons. Much may be done with a skilfully managed balloon. Its great advantage over other air-sailing vessels lies in its comparative safety. Let any other kind of airship decisively come to grief, and instant death ensues for all its crew. But if a balloon bursts, not too near the ground, the calm and skilful aeronaut can take measures to save himself. This accident happened to Wise in Pennsylvania in 1836, at an elevation of 13,000 feet, but he was so far from being reduced to a pulp by the fall that, jumping up, he remarked upon the heat of the lower atmosphere, and, before many minutes had elapsed, had determined to repeat the experiment at the first opportunity. The fatal falls (other than drowning cases) have usually been from moderate heights, or have been due to the fright or inexperience of the operator. When Simmons met his death in 1888, he fell only 50 feet; yet neither of his two companions was killed, and one of them was not even injured. Capt. Dale's balloon in 1892 burst at a height of 600 feet, he and Mr. Shadbolt, a professional aeronaut, being killed, while two amateurs who were with them escaped unhurt; but experts opined that with proper management all might have been saved. This comparative immunity arises from the fact that the lower half of a falling balloon of the ordinary shape invariably cups into the upper half, forming the best of parachutes. Immunity, therefore, does not extend to aerostats stiffened with hoops or made of aluminium [[sic]]. A serious fault of the ordinary balloon is that there is no level at which it is in equilibrium unless the gas be confined, which is too unsafe. When it goes up, it retains the same ascensional force, and continues to be accelerated upward until it loses gas; and its momentum of perhaps a couple of tons moving four or five hundred feet per second will carry it up long after the gas has so swelled and spilled that, by the time it ceases to rise, it is much heavier than the air, and would come down to earth if ballast were not thrown out; and so it goes, alternately rising and falling until its ascensional power is quite wasted. Mr. Walker does not make this matter at all clear, but talks, as aeronauts are apt to do, of the level "to which the balloon must rise," just as if it were a closed bottle. A metallic balloon would be free from this objection, having a definite level at which, if tight, it would remain in equilibrium, or oscillate above and below it, indefinitely. Schwartz's machine of 1894 demonstrated that an aluminium balloon can be made sufficiently light (its ascensional power must have been about 7,000 lbs.) and can be filled with hydrogen; but it is very unfortunate that the inexperienced operator took fright and destroyed the airship, though not his miserable self, before it had risen high enough to show whether or not at its level of equilibrium it would have been able to withstand the pressure of gas within. The excess of gas would naturally be allowed to escape; but if this escape were too rapid, all the advantage of the metallic construction would be lost, while if it were not very rapid, the excess of pressure from within would become very considerable. Though such things are subject to calculation, actual experience is extremely welcome. To-day it is probable that such a vessel would be made of magnalium, not of pure aluminium. Somebody with a spare million could make an interesting experiment by combining the metallic balloon with a suggestion of the celebrated Monge that has never yet been tried. He was eminently a practical man as well as a mathematician of the first order. His suggestion was that of an air-snake, to be composed of twenty-five aerostats strung together, the vermicular or serpentine motion being brought about in a vertical plane by the transfer of ballast from one to another.

The second type is that of machines having both ascensional power and motors. Mr. Walker maintains that this is the only practical form, on the ground that this alone affords safety in case the machinery goes wrong. Plausible as this sounds, facts are against it, and reason too. Notwithstanding the thousands of ascensions that still take place in vessels of the first type for every one in a vessel of this second type, four times as many men have been killed since 1892 in ascensions of the latter type as of the former. It would be quite absurd to maintain that carrying a motor adds to the security of a balloon. That the addition of a balloon to an airship with a motor is a most serious source of peril would seem obvious enough, even if indirect effects of danger are left out of account. It may, however, be that a ship of this type supports a minor accident better than does any other. The breaking of the steering-apparatus of Count Von Zeppelin's great air-ship at its grand gala trial on Lake Constance did not prevent its accomplishing a little excursion and effecting a beautiful descent upon the lake; and M. Santos-Dumont, on his

first ascension in Paris, broke his rudder, successfully landed, mended it, and continued his performance. Many engineers of standing have declared in favor of this type, which is the only one that has as yet attained some undeniable success; yet those who have most deeply studied the problem are opposed to this type. Sir Hiram Maxim, in a preface which he has contributed to 'Travels in Space,' argues that "it is not possible to make a balloon strong enough to be driven through the air at any considerable speed [meaning above six or seven miles an hour] and at the same time light enough to rise in the air." But he gives no assurance that this judgment is based upon calculations relating to magnalium balloons. Besides, it has been urged in reply, by Von Zeppelin and others, that a velocity of seven or eight miles an hour, or even less, would be all-sufficient for the peculiar purpose to which air-ships must be restricted. For, it is said, it is quite unreasonable to suppose that a vessel sailing the air should ever be able to compete with an ocean steamer, and quite ridiculous to imagine it should ever carry freight or many passengers. Its distinctive superiority lies in the fact that, moving in the three dimensions of space, it can never be intercepted or obstructed except by the most improbable chance. Its service must, therefore, always be to go where nothing else can go, without carrying or bringing back anything but intelligence. Its function will be to hunt up lost explorers, to spy out an enemy's doings, to visit the upper atmosphere, and in short to act as a reporter. Now for this business it is contended that great speed is needless. One can but feel, however, that it is highly desirable that the reposting airboat should not be carried quite away from its course by anything short of a moderate gale; which would demand a velocity of at least twenty miles an hour.

The third type is that of machines provided with motors, but heavier than the air. This is the form advocated by Langley, Maxim, Barton, Hargreaves, and, in short, the general body of those who have in our day studied the subject in a scientific manner. It is the only form which by any possibility could ever decidedly distance the "ocean greyhound" of to-day. Its real merits cannot be estimated until it has been embodied in some practical shape.

The fourth type is that of instruments neither possessed of ascensional power nor carrying any engine. To be sure, they may, and hitherto generally have, supposed a man to be kept hard at work during their trips. But how little his could amount to, as mechanical work, becomes manifest when we reflect that the more powerful of a man's muscles are unadapted to the long-sustained production of impulses at a greater frequency than, say, two per second. If therefore, such impulses were to be relied upon to prevent the instrument from falling, since in the interval from one to another, the machine would have fallen four feet, it follows that the labor the man would be called upon to perform would be equivalent to that of taking the instrument (say, a hundred-weight) on his back and running up stairs with it at the incessant rate of eight feet per minute, or four hundred and eighty feet per hour. Each reader can speak for himself as to how many hours at a time he would contract to keep up that lively exercise.

It has many times been demonstated [[sic]] that there is no very formidable difficulty in constructing an instrument weighing about a hundred pounds which shall lift a man, or even two men, up into the air in a fresh breeze, and carry them up into the wind. It is supposed that they could sustain themselves indefinitely, if they were

skilful enough, without any particular expenditure of energy, in the same way in which birds ranging in size from the lark to the condor soar. A condor will weigh eighty pounds and will soar all day long without any sign of effort or of fatigue. Various facts go to support the theory of Professor Langley that is [[sic]] is by taking advantage of the puffiness of the wind (its "internal work," as he calls it) that birds soar; though it is not certain that other factors, of which three readily suggest themselves, may not contribute to the effect. It is quite certain that a considerable weight is one requisite. The most successful of the flights of Le Bris occurred one day when the rope by which his instrument (which was intended to carry only himself) became accidentally wound round a second man. Le Bris, not noticing what had happened, carried the man up two or three hundred feet into the air, and forward into the wind for a furlong or so, and could apparently have gone indefinitely further. But when he had descended sufficiently to set his captive passenger free, he found that without that ballast he could no longer fly. Thus far, however, no man has found it possible to acquire the necessary skill to manage such an instrument, in advance of getting killed by his blunders. The thing has not really had a fair trial. Le Bris was a very poor man, a common sailor, and circumstances prevented his practising on the water, although his machine had been specially constructed with a view to that. Consequently, before he could learn the art, his machine was smashed; and he lacked the means to reconstruct it.

Although Mr. Walker contemplates the construction of airships of the second type alone, yet, owing to this type reuniting the positive features of the others, his volume contains many facts pertinent to the construction of any airship. As far as our verifications have extended, his numbers are accurate. But nothing more inaccurate and unintelligible than his statements of mathematical rules and formulæ can be imagined. For example, on pp. 17 and 18 is an attempted explanation of the manner of calculating the elevation from the pressure of the air. Not until one has corrected several misprints, including the uniform printing of exponents as factors, do the difficulties of finding out what the man means (although the reader knows what he ought to mean) fairly emerge. They are not confined to any one sentence. A number has been obtained, and, being correct, there is substantially but one way in which it could have been reached; yet what relation there is between what is said and this operation, one cannot make out. So it is, in lesser degree, throughout the volume.

75 (13 November 1902) 390: The Origin and Significance of Hegel's Logic:

A General Introduction to Hegel's System.

By J. B. Baillie. Macmillan Co. 1901. 8vo, pp. xviii, 375.

CSP, identification: Haskell, *Index to* The Nation. See also: Burks, *Bibliography*; *List of Articles*; MS 1457 (draft).

This book is, on the whole, of any that we have seen, the most helpful for a student prepared to take up the study of Hegel. It is not designed as a substitute for reading Hegel's own works, but, as the title-page proclaims, as an introduction to

such reading. It explains how Hegel's 'Phänomenologie,' 'Logik,' and other works came to be written, which is what the student of any system should desire to learn, first of all. In saying this, we are supposing that the student is not a neophyte in philosophy. When a man first takes up the study of philosophy, his difficulty, at the outset, is that he is already possessed by a crude system of metaphysics, and that, while he has a vague curiosity to know why others do not think as he does, he really has no desire to learn. After that first difficulty is conquered, he has to make a study of some one system of philosophy, which, however, it is impossible for him really to understand at this stage, because he does not comprehend the original state of mind of the author at the time his original studies were begun. In the case of Hegel he must, as a matter of course, understand Kant, and especially the deduction of the categories, not only as his doctrine appeared to Kant himself, in his two editions, but as it appeared to the young theological students who read it while it was fresh. He will necessarily make some study of Fichte's 'Wissenschaftslehre' and of some of the earlier writings of Schelling. He can then take up this book of Dr. Baillie's with profit, and thereafter the study of Hegel (in German, of course, for the 'Logik' is, in a sense, a dissertation on the German language) will not present any insuperable difficulties, unless Hegel's own inaccuracies be considered such. In particular, he should have carefully read Dr. Baillie's admirable concluding chapter, entitled "Criticism." His only danger will then be that of overlooking, what Hegel entirely overlooks and Dr. Baillie does not distinctly recognize, that thought and "immediacy" are not the only factors of experience. To avoid that danger he ought to be penetrated with the spirit of science, to understand English thought, that of Herbart, that of Fries, and be well acquainted with modern exact logic.

Hegel is a vast intellect. The properly prepared student cannot but feel that the mere contemplation of the problems he presents is good. But that the study of Hegelianism tends too much toward subjectivism, and is apt to break that natural power of penetrating fallacy which is common to all men except students of logic, especially of the German stripe--seems to be the result of experience.

75 (27 November 1902) 430: Theory of Differential Equations. Part III. (Vol. IV.): Ordinary Linear Equations.

By Andrew Russell Forsyth. Cambridge (Eng.) University Press; New York: Macmillan. 1902. 8vo, pp. xvi, 534.

CSP, identification: Haskell, *Index to* The Nation. See also: Burks, *Bibliography*; *List of Articles*; MS 1458 (draft).

The successor of Cayley here truly presents us with the fifth volume of his useful work on differential equations; for his more practical 'Treatise' on the same subject really forms an essential part of this. No further volume is promised; but the author does not declare that he will give no other, and we are inclined to think that one more will come. Certainly, enough topics have been passed over to furnish forth two, and that richly. Although it cannot be called a complete handbook, the work has a tolerably definite plan and a judicious one. Whatever would more particularly interest an algebraist or student of the theory of groups is omitted. Whatever stood right across the path of a writer on Differential Equations is treated in a general way. Works of several American mathematicians are necessarily expounded in this volume. Prof. G. W. Hill's method forms the subject of one of the ten chapters, and that is meagre allowance. An unfinished MS treatise of the late Prof. Thomas Craig has been made use of. Theorems by Osgood, Bôcher, Van Vleck, and others appear. A peculiar sort of suggestiveness attaches to that of Bôcher.

Of course, such a book can have no interest to the generality of our readers except in this respect. The premises upon which pure mathematics rests are few and simple. So far as they are capable of definite numeration, there are about a score of them. The reasoning is wholly deductive. If, therefore, deductive reasoning were what the logic books represent it to be; if, as Kant says, it merely explicated what is confusedly thought in the premises; if, as Mill says, it merely registered what had already been accepted, then the total number of mathematical conclusions could not exceed the total number of possible combinations of premises--or, say, something like a million, including the most trivial. By this time, then, pure mathematics ought to be approaching exhaustion. Doubtless, the current impression among ageometrical persons is that such is the case. Yet the four volumes of Professor Forsyth's 'Theory' present, in somewhat full outline, only about two-thirds of the discoveries made during the nineteenth century in a subject which has occupied about one-tenth of the total energy of mathematicians; and far from there being the slightest sign of exhaustion, the bulk of the new work is increasing in geometrical progression, while it is constantly growing more and more profound and broad. As many new *methods* of value now appear in a decade as there were born of new *theorems* in the same interval a hundred years ago. Here is a subject dealing with nothing but the abstract creations of the mind; a subject, too, in which comparatively few are able to make discoveries; and yet it may be doubted whether sixty volumes could give a very much fuller account of the mathematical discoveries of the nineteenth century than could be given in the same space of the discoveries in so rich and universally accessible a field as biology. At any rate, the fact that there is no utterly overwhelming discrepancy affords food for rumination.

75 (18 December 1902) 485-486: ELLWANGER'S PLEASURES OF THE TABLE

The Pleasures of the Table: An Account of Gastronomy from Ancient Days to Present Times.

By George H. Ellwanger. Doubleday, Page & Co. 1902. 8vo, pp. 477.

CSP, identification: Haskell, *Index to* The Nation. See also: Burks, *Bibliography*; *List of Articles*; MSS L 159.219, L 159.221; MS 1459 (draft).

George Herman Ellwanger (1848-1906) was an American horticulturist. He studied in France, Germany, and Italy and was graduated A.M. from Rochester University. Other books by Ellwanger include *Meditations on*

Gout (1895) and *The Garden's Story* (1889).

George H. Ellwanger, M.A., is a man (for his observations on truffles would, by themselves, suffice to persuade us of the error of 'Who's Who in America' in making him his parents' *daughter*), and is, indeed, a well-known author, not destitute of wit, and an adept at handling the pen. Nobody will expect to find here the *esprit* and steely style of Brillat-Savarin--a mere knack, after all, since Balzac could exactly imitate it throughout a whole volume. But this work has the advantage of being agreeable reading, and not that revolting mixture of physiology with appeals to the reader's personal consciousness. Its tasteful covers enclose hardly a single page that is not positively entertaining. The writing has all the lightness and propriety that its subject demands, with a certain appropriate aroma of French, as indefinable as that of a dish of fresh truffles still covered by the napkin, a just-opened bottle of Léoyille-Poyferré alongside of it--

"Et je ne comprends pas quel expert inhabile

A pu dans les seconds classer le Léoville,"

says a poet worthy of Mr. Ellwanger's attention, Biarnez--mingling its perfume. There are no Gallicisms in the syntax nor in the acceptations of words; the English is irreproachable, is scrupulously, almost fastidiously, correct. There are a great many translated passages of some length, both in prose and in verse; and these are done so deftly (where the versions are not borrowed) that it is a pleasure to compare them with the originals. The style is plastic, shaping itself to the subjects of the different chapters. There is in it, through the greater part of the volume, a dash of Charles Lamb, very suitable to a book about books, so long as it is not so strong as to seem put on. For, mainly, this is a book about books, a sufficiently complete history of gastronomy.

Of original discussion of nice questions of preference in eating there is not very much. Nor can we regret it. If there be any direction in which the author's discrimination is less unerring than the laws of nature, it is just in this matter of eating and drinking. From a writer who would heat a dining-room to 70 degrees or 73 degrees, who pronounces that Mohammedans and Hindus have no cuisine worthy of the name (thereby calling to mind one of the lightest, most appetizing, and best-wearing of the dishes of this sublunary orb, pilaf, not to speak of kabobs, of those *entrées* into which the gourd enters, nor of those wonderful concoctions of mincemeat and esculents that are cooked in hermetically sealed porous earthen vessels; calling to mind, too, so original and satisfactory a repast as one of which a good curry forms the centre); who prefers the veal of "Germany," taken indiscriminately, to that of French Lorraine, and the *pré salé* of Normandy and Brittany to the wild mountain mutton of Asia Minor; who holds that the sweet breath of the lettuce ought to be contaminated with that of onions, instead of being wafted to the empyrean on the cherub wings of garlic; who praises what is coarsest and indigestiblest in the "American cuisine," and overlooks the many delicacies that abound on humble tables scattered through our remote districts--from such a writer we can content ourselves with receiving but a meagre nosegay of his gastronomic decisions.

His erudition, on the other hand, is boundless, or bounded only where print is bounded; and some of his chapters must perforce have resulted from his own researches, since there are no secondary authorities from whom they could have been drawn. He need not fear that any reader can fail to appreciate the range of his acquaintance with the books. In giving passages from Dionysius of Sinope, from Cratinus the Younger, from Philemon, from Hegesippus (whom he calls Hegesander), from Artemidorus Aristophanius (whom he calls Artemidor), and from other such, there was no occasion for assuming an air of having searched their writings through, since the learned and the simple will otherwise be sufficiently impressed with the author's industry, while everybody particularly interested in gastronomy will know perfectly well what the single source of all those fragments is.

The work is one of real value; but if we are asked whether or not it is accurate, we shall be reminded of a question and answer once overheard in a Nahant barge: "Is Asy's wife pious?" "Well, she's '*piscopal*-pious." So of this book, we may say that it has an after-dinner accuracy. Brillat-Savarin is referred to throughout as "Savarin," and in one place it is formally stated that the name was Brillat de Savarin. Now, while we make no pretension to private information, and while we are quite aware that persons who wished to speak flatteringly of him used sometimes to call him M. de Savarin, just as one might call Fouquier-Tinville M. de Tinville, if that could conciliate him ("He, bon jour, M. de Corbeau"), yet we believe the name was as it is universally given. Presumably, the male stock had originally borne the name Brillat, to which Savarin had been added as a sort of quartering, as with thousands of such bourgeois designations. Berchoux's sprightly poem is said to have been published in 1801, although Mr. Ellwanger must be familiar with the fact that it went through three editions in 1800. But probably at the moment of writing the sentence he had in mind some statement that it appeared in the first year of the nineteenth century. The most celebrated of all taverns, Aux Trois Fréres Provencaux, is, on page 213, called "The Provincial Brothers," as if they were *provinciaux*. The story about the knighting of the sirloin by Charles II. is given without any warning against the ridiculous dirivation of a word in use in English, as Wedgwood shows, from the time of Henry VI., and still older in French. Of course, the prank may have been actually played by Charles II., but it is more like to be fabulous. On page 29, Caelisu Apicius, the writer of the cook-book, is spoken of in immediate juxtaposition to the famous Marcus Gabius Apicius, in such a way (both being called simply Apicius) as to convey the idea that they are one person. Further on (p. 41), the relation of the one to the other is correctly explained. Nicomedes is called King of the Babylonians, instead of King of Bithynia. The Greek cocotte Barsine appears as Bariné, as if she were a hetaira of Paris. The early Greeks are said to have been in the habit of taking four regular meals a day; but another statement about them is eminently true; namely, on page 9 we read: "Coffee, of very remote use in Abyssinia, was unknown to the early Greeks and Romans." These are merely a small selection from the illustrations we have noted of the kind of accuracy of the work.

The volume is a very beautiful and tasteful one, printed with Caslon-like type and the blackest ink, upon paper which, though calendared, is not too heavy. There are some three-dozen charming illustrations reproduced from old prints, with delightful vignettes and ornaments. It is so sumptuous that when one finds it entertaining and instructive enough to be well worth having in any dress, one is quite taken by surprise.

75 (25 December 1902) 506-507: Sundials and Roses of Yesterday.

By Alice Morse Earle. The Macmillan Co. 1902. 8 vo, pp. 461.

CSP, identification: Haskell, *Index to* The Nation. See also: Burks, *Bibliography*; *List of Articles*; MSS L 159.220-221; MS 1460 (draft).

Alice Morse Earle (1853-1911) first gained recognition by writing on the social and domestic history of colonial New England and New York. Among her other publications are *China Collecting in America* (1892) and *Customs and Fashions in Old New England* (1893). During her life, she was active in the Society of Colonial Dames and the American Historical Association.

Mrs. Earle gives reproductions of a selection numbering well over two hundred of her photographs of sundials, with a smaller number of those of roses, which, by the way, do not take so well. She has joined to the pictures first-hand information about the originals, has touched upon the subject of dialling, and glanced at sundry other topics *à propos de bottes*. The attractive element of the volume is the antiquarian-emblematic-poetical perfume that lingers about the dials. A charm there certainly is about almost any sundial. Still, something like a collector's mania must be added to the ordinary interest in sundials before a person can read Mrs. Earle's volume right straight through without flagging. She seems herself to feel that this is so, and endeavors to lend variety to the subject by chapters about roses, about Rosicrucians, about rural saints and prophets, with desultory passages not a few. But it is not the question whether a book has some dull pages or not, but how enjoyable it is at its best. As somebody says of a poet, it is not how many of his arrows go astray that counts, but whether any of them has struck the Dordonian oak, and sent a resounding shiver through all its limbs and branches. On the contrary principle, how many of the most delightful books--including all the old ones--would have to be condemned! It is always the reader's privilege to skip.

'Sundials and Roses of Yesterday' is much of it very interesting reading to anybody, and all of it to somebody. The authoress could not have furnished us with a livelier variety and still have described as many individual dials as she has done, without which the devotees of dials would have been dissatisfied. To our thinking she has been unduly afraid of being heavy and dry. As long, for example, as she deemed a chapter on dialling called for, why would it not have been better to procure its preparation from some adept in descriptive geometry? If Miss Charlotte Angus Scott or Mrs. Christine Ladd Franklin could have been persuaded to lend this help, the elegance of their exposition of a problem--after all,

not so rebarbative as it may be supposed before being fairly scanned--would have added a new grace to the book. To be sure, as it is, two rules are given for constructing a horizontal dial, of which the first is neat, were the rationale of it only made plain. But there are no directions for any other form of dial than that least interesting, least beautiful--and we should add least useful one, were the ratio of zero to zero more intelligible. The second rule given in the book is intended to bring the art of dialling to the level of the comprehension of Southern plantation negroes, and is a deplorable failure. It not only requires the poor fellow to know what latitude is, but also to lay off an angle equal to that latitude. Neither requirement was necessary; and after all he won't get a good dial. Mrs. Earle tells of simple dials that give the time without danger of more than four minutes' error, as experience shows. But does she remember that the penumbra always causes the reading of the time to be about a minute too near to noon, an effect that diffraction considerably increases, while refraction always acts in the same direction and may add two minutes more to the error; that the equation of time sometimes exceeds sixteen minutes, and that the difference between local and standard time may be half an hour? Let all these act in the same way, as they sometimes will, and they foot up to more than four minutes; but, of course, the readings can be corrected.

Nowadays, when there are railway trains to catch and dentists to pay for any unpunctuality on our part in keeping our appointments with them, and the fashion originally set by time and tide of waiting for no man has been generally followed, the sundial has been degraded to an educational device. It is true that if, instead of observing a shadow, one observes an image formed by a lens, and observes it with a microscope, and, in place of the sun, uses a number of stars, high and low, correcting for level, azimuth, and collimation, then indeed our dial becomes a modern astronomical transit, and is the most precise possible instrument for correcting a clock or chronometer, though not exactly handy for direct use in catching a train. For that purpose, a good watch that has recently been compared at noon at any telegraph office is preferable.

Very tolerable clocks had for centuries been in common use when Galileo first watched the swinging lamp in Pisa Cathedral. Yet it was not until the nineteenth century that mean time (in contradistinction to every variety of sun time) came to be universally employed; and no mechanical clock or watch could keep pace with the inequalities of the sun in any satisfactory manner. Consequently, when we see an old wall dial, though its age be but two centuries, we may be reasonably sure that it had, in its day, an honest utility. It is an individual to be respected. But what, pray, is a horizontal dial in the garden of a modern gentleman of business but a despicable lump of affectation? Does he permit any member of his household, if he can help it, to esteem time of so little worth as to be measured out in hours?

The author's extreme dread of being dry or heavy is illustrated in the chapter on Rosicrucians, concerning whom, she informs us, she has read very much, and concerning whom so little that is really solid has ever been written--enough, however, to make it the business of any chapter on the subject to present the

proof that this brotherhood owed its origin to a lie concocted about A. D. 1600 by Johann Valentinus Andreä, alias Basilius Valentinus, alias Florentinus de Valentia, etc., and Johann Thölde of Frankenhausen. With all her learning on the subject, Mrs. Earle should have known that something like this was the case; but the glamour of mystery has been sweeter to her than the plain truth. From something said in the preface, we were led to hope that this chapter would be ushered in by a reproduction of the rare and fine bookplate of the "liebliche Bruderschaft des himmlichen Rosenkreutz" of Andreä and Tholde, with its view of heaven and hell. But we found only a decorative rose.

1903

76 (29 January 1903) 99-100: **Euclid: His Life and System.**

By Thomas Smith, D.D., LL.D. Charles Scribner's Sons. 1902. 12mo, pp. 217.

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

Dr. Smith's style is light and easy, *plena litteratæ senectutis oratio*; for the author is one who can speak of the quantification of the predicate as a novelty (it was only introduced in 1827), and who, in the words, "In our time, Gauss has shown," etc., refers to a publication of 1801. He has not forgotten his Greek, for he reads Proclus, and he constantly reminds us most winningly of what the education of a gentleman used to be. *Quid enim est jucundius senectute, stipata studiis juventutis*?

The little volume is not intended for scholars, but for those who know no more of Euclid and his science than they learned in the high school; and where the reader's attention may threaten to tire, he is refreshed by something of a facetious turn until he is ready to resume the more serious discourse. One will naturally not expect the author to have the least inkling of the way of thinking of modern mathematicians about the 'Elements.' He treats the "theory of parallels" in the good old way, taking his stand with those who were valiantly resolved to demonstrate that the theorem that the three angles of a plane triangle are equal in sum to two right angles, follows as a necessary consequence from certain premises concerning a plane, although it stared them in the face that these premises are equally true of the surface of a sphere, while the sum of the angles of a spherical triangle exceeds two right angles. Stated in this way, their undertaking was manifestly predestined to eternal failure. One-half of this state of things was clear to the mind of Euclid. That is to say, his confusion of thought about one-half of it arose from two subconscious assumptions, the recognition of which would have made him wholly right. One of these was that space is immeasurably great. That he assumed this appears (among other places) in his supposed proof (I. 16) that the angles of a triangle are not *greater* than two right angles; and that he assumed it irreflectively is shown by the language of his second postulate compared with the use he put it to. It reads that a terminated straight line can be produced continuously ({kata to syneches}); but he applies it as if it read "can be prolonged beyond any assigned length." His other unconscious assumption is that all the figures with which he deals are finite. This is shown by his axiom (called the eighth) that the whole is greater than its part. For, of course, Euclid knew well enough that a straight line terminated at one end only, and endless in the further direction, is not made any shorter by cutting off a finite part of it, since what remains can be shoved along to cover the extension occupied by the whole, and, being endless, leaves no part uncovered. These two assumptions not being explicitly made, his proof of the sixteenth proposition which we have just (substantially) quoted, remains

imperfect. To prove that the sum of the angles of a plane triangle is not *less* than two right angles, he plainly saw that a special assumption was necessary, and stated this in his fifth postulate (which Professor Smith calls the twelfth axiom) in such a form that it should manifestly appear the matter of fact it really is, and not a matter of pure reason, or an axiom. The whole tribe of old laudators of Euclid (of whom our author is one), though they all but deify him, fail to give him credit for understanding this matter, which they do not understand themselves; but, on the contrary, regard the postulate which proclaims his understanding of it as the greatest blemish upon his work.

This booklet is not intended for people who care to gain a profound understanding of Euclid, or to acquire any minutely accurate information, and therefore small slips are innocuous. But we should deceive our readers if we did not warn them that such slips there are, and thick as leaves in Vallombrosa. For example, we are told in sundry places that Euclid the mathematician was not Euclides "of Megæra." That is true, since that dreadful personage had no Euclid. But neither was our Euclid that eristic philosopher who lived in the town that had come to be called the "Edifices"--{Ta Megara}, the quantity of which Dr. Smith, with his Greek, will perceive if he thinks of the chatter of Penelope's suitors.

"{myesteres d' homadesan ana megara skioenta}"

With singular obliviousness, he says that Alexander survived his master, Aristotle, for a year, although in different places in the book the dates of both deaths are rightly given. It is the same with the mathematical work. On p. 208 he begins an attempted demonstration of a proposition which has been fully proved to be indemonstrable. Here is a bit from which to judge it: "The triangles ABC, BAD have . . . an angle DAC . . . = angle ACB." Now, even without a figure, it is pretty clear that DAC is not an angle either of the triangle ABC or of the triangle BAD. It is no misprint, but a slip that brings the whole demonstration to ruin. Dr. Smith has four triangles, formed by the two diagonals and four sides of the quadrilateral ABCD, and it was requisite to show that the pair of triangles he mentions were equal. He has become confused among the four, has inadvertently substituted a different triangle for one of them, and the whole demonstration is, consequently, a blunder. A "proof," substantially of the same thing (though the author appears not to see that it proves this or nothing), on page 213, is even more plainly inconclusive.

England is the only civilized country where it often happens that an educated man will write a book in which he makes it plain, sometimes even blandly confesses, that he has not taken the trouble to acquaint himself with the most accessible and pertinent facts. We must not blame an individual for following the established customs of his own country, but we are bound to instance at least one case among many in which Professor Smith shows himself the thorough Briton that he is in this respect. There is a considerable account and criticism of Legendre's treatment of parallels, which was certainly called for by the general plan of Professor Smith's book. But this account and criticism is based exclusively on Brewster's translation of Legendre's 'Éléments de Géométrie.' Now Legendre became dissatisfied with his original treatment of the matter, and in his third edition completely revolutionized it. Subsequently, he decided that his new demonstration was invalid, and in his ninth edition returned to the method of Euclid. Still later, he imagined that he had found a way out of the labyrinth, and in the fourteenth (and no doubt in some intermediate editions to which we are unable at this moment to refer) he made a new attempt, very curious and subtle, as it had to be to deceive Legendre, but quite worthless as a proof--namely, he distorts his triangle, so as to make one of the angles smaller and smaller, while the sides are so lengthened that the area of the triangle remains unchanged; and he thinks he proves that his process can go on until, that angle vanishing, all three sides lie in a straight line, *although the area of the triangle remains finite*, and without investigating to what limit the values of the other angles tend. Legendre, in an appendix, gives a still different attempt at a demonstration, which really begs the question, and in one edition is substantially admitted to do so. Of all these changes, Dr. Smith knows nothing, and his criticisms of what he does know of Legendre's work (concerning which two opinions are no longer possible for competent men) admit points that ought to be denied, and deny what ought to be admitted.

A particularly amusing case of writing in complacent consciousness of lacking proper information is a certain speculation about entirely known facts regarding the commentary on Euclid by Campanus--a speculation provoked by a similarly ignorant passage from a certain 'Short History of Mathematics,' which pushes the national custom to a conspicuous eminence. But we cannot give more space to this.

There is much in the volume about the educational value of elementary geometry. In the broad sense that mathematics is good mental discipline, and that modern mathematicians are the only men who reason with precision, we think the author quite right. That is as far as we can go along with him. To judge of the matter it would be indispensable to inquire how far elementary metric geometry reasons correctly, and whether or not, in rendering its reasoning sound (without which it must be positively mischievous), it would not necessarily become far too difficult for the average boy. There is a way in which, by easy exercises, geometry would gradually develop great logical strength in almost any boy; but it supposes such a revolution in the methods of presentation and of instruction as would be practicable only in a country where teachers were more genuinely educated and had more leisure and stimulus to study than they have in ours. In default of that, the very inferior, but much easier, subject of the theory of numbers affords a far sounder discipline of the logical powers.

76 (5 March 1903) 194: NOTES

CSP, identification: Haskell, *Index to* The Nation. See also: Burks, *Bibliography*; Fisch and Haskell, *Additions to Cohen's Bibliography*; MS L 159.225; MS 1464 (draft).

Henry Augustus Rowland (1848-1901) was graduated from the Rensselaer Polytechnic Institute with a degree in civil engineering in 1870, and for a short time worked as a railroad surveyor. In 1872, he returned to Rensselaer as instructor in physics, but he left that post to study under Helmholtz in Berlin. Upon returning to the United States, Rowland was appointed chairman of physics at the Johns Hopkins University. Among many other honors, he was awarded the Rumford medal for researches in light and heat, and the Draper medal for researches in spectroscopy. An anecdote on Rowland relates that he was retained by the Niagara Falls Company as an expert on making reports on the utilization of the power of Niagara Falls. After completing his duties, he found himself in the position of having to sue the company for his fee. At the trial, counsel for the company asked Rowland to state the difference between his status and that of an English engineer concerned in the case. He replied that the Englishman was one of thousands, but he was one of three in the United States. And when asked to name the half-dozen greatest living scientists, he stated under oath, "Helmholtz, Kelvin, Rayleigh, and myself--no others."

--Henry Augustus Rowland being a name upon which attention will inevitably be arrested in any extensive future history of the development of human knowledge, the future reader of that history may ask, "How came such a tree to grow to such proportions in such a soil?" Well, it happened that the duty of tending that tree fell upon a university president of such singular discernment as not to take fright at meeting with a real live man, a man obtrusively and naively real and personal; and so the tree was supplied with the desirable fertilizer, and quite indispensable vacancy, without which its growth might have been vigorous, but never could have attained to largeness and symmetry. Had Rowland been a growth of French soil, the publication of his complete works would have been undertaken by the Government, and would have been executed in such style as seemed worthy of a nation in the van of civilization. Let us hope that some complete publication may somehow be made yet. Meantime we receive from the Johns Hopkins University a cheapish reprint ('The Physical Papers of Henry Augustus Rowland') of his experimental works, some of them too much abridged to answer all the purposes of the critical student. The volume contains, besides, some public addresses and other writings which we are thankful to find thus made available. Those works by which Rowland most stirred physical thought, and upon which his place among those American physicists who, since Rumford, have influenced fundamental conceptions (if any other such there be), must mostly depend, are omitted. It is said, in excuse for this strange method, that Rowland himself did not desire the republication of those papers. We are not told why; but Rowland certainly did not depart from the usual type of genius in that his judgment was less sure than that of ordinary men. Those who knew him would not be surprised to hear that he had passed through a phase in which, like Pascal, he thought mathematics an idle amusement. But this should not have influenced the editors.

76 (19 March 1903) 226: A CORRECTION

The editorial reply to this letter is by Peirce in view of his authorship of the notice that Ames criticizes. See also: Burks, *Bibliography*.

Joseph Sweetman Ames (1864-1943) was a widely published author on topics of physics and general science. He began his higher education in 1886 at Johns Hopkins University, and graduated Ph.D. from there in 1890. Ames was well known in academic as well as in scientific circles. He was made an honorary member of the Royal Institute and was a member

of the National Academy of Sciences. An honorary LL.D. was bestowed upon him by Washington College in 1907.

TO THE EDITOR OF THE NATION:

SIR: My attention has been called to a brief review, in a recent number of the *Nation*, of the Physical Papers of the late Professor Rowland, and, as Secretary of the committee in charge of the publication of these papers, I think it necessary to call your attention to certain misstatements in the article referred to.

In the first place, the reviewer says that the reprints of some of the experimental works are "too much abridged to answer all the purposes of the critical student." As a matter of fact, every published paper dealing with Rowland's experimental work, either from the point of view of theory or results, is reprinted in full in this volume, with the exception of the figures in the *Tables* of Wave Lengths, copies of which Tables, however, accompany each volume. Not one word is omitted. The description of the methods by which these Tables are obtained is published in the volume. It may be that the reviewer is under the impression that the extracts from certain French scientific papers in regard to Professor Rowland's last determination of the Ohm are abstracts of published articles, whereas, in reality, Professor Rowland never published any description of the methods or results of this research.

In the second place, the reviewer refers to "some public addresses and other writings," thus implying, possibly, that there were other similar writings which were not included in the volume. In reality, *all* Professor Rowland's "addresses and other writings" are reprinted.

In the third place, those papers on purely mathematical subjects which are omitted from the volume, four in number, are in no case those which "most stirred Physical thought and upon which his place among those American physicists who since Rumford have influenced fundamental conceptions (if any such there be) must mostly depend." If the reviewer will look for one moment at the lists of the papers omitted, or, better still, if he will read the papers, he will see for himself why they were omitted. It should be noted, moreover, that every paper, mathematical or not, dealing with the fundamental conceptions of Physics, which Professor Rowland published, is, without exception, reprinted in this volume. Opinions may differ in the years to come as to the relative value of Rowland's contributions to science, but there can be no doubt that his thoughts and his experimental investigations in regard to the properties of heat, light, and electricity will always be of permanent value. Every paper published by Professor Rowland bearing in the remotest degree upon the above subjects is included in this volume.

It is a matter of regret that to any one the character of the publication of this volume should seem to have taken a "cheapish" form. It was the effort of the committee to give the volume such a character in respect to paper, type, illustrations, and binding that it would in every way be a fit memorial of their late colleague. As a matter of fact, the volume in its external appearance compares most favorably with the publications of the collected works of Lord Rayleigh, of Hopkinson, of Reynolds, and others which have recently appeared.

It is a pity that the reviewer did not see fit to call attention to certain features of the volume which will make it always useful to all students of physics--such as the publication of many papers which had been entirely lost sight of, and the detailed description of Rowland's wonderful ruling engine, the mechanism of which is here for the first time explained in print.

I am, sir, very truly yours.

J. S. AMES.

JOHNS HOPKINS UNIVERSITY, March 7, 1903.

[We have received from our contributor the following statement concerning this matter.--ED. NATION.]

"Professor Ames's letter convicts me of a grievous mistake of a complex nature. I received a copy of the volume with the request for a note upon it, and very unwisely accepted the commission when it was not in my power to make a sufficiently careful examination of it. The copy sent me was not accompanied by the wave-length determinations for which the name of

Rowland is now uppermost; and two misapprehensions of mine, due to my well knowing his insistence on the publication of all details of experimentation, led me to think that these had been omitted in other cases. I had read Rowland's first mathematical memoir, which seemed to me to contain a striking enlargement of conceptions of electricity, and to place him in a higher rank of science than his experimental work. I was aware that there was some controversy in regard to its soundness; but there have been before mathematical works which proved upon examination to be unsound, yet which incontestably advanced human thought in no small measure. If it be true that Rowland's mathematical work is a total wreck and must be consigned to oblivion, I shall be very sorry as an American and as an admirer of the glories of the Johns Hopkins University. No doubt, Rowland will still remain a physicist of very high importance.

"As to the description of the famous ruling machine, I assumed that this gave the working drawings with explanation. If I had looked more carefully at them, I should have seen that this was not their character. It appears that they are new drawings, executed to scale for the purpose of this publication; and this sufficiently evidences the care that has been bestowed upon it. My epithet 'cheapish' marked my general dissatisfaction; but, with Professor Ames's explanation, it reduces itself to an expression of taste merely."

76 (2 April 1903) 269: NOTES

CSP, identification: Haskell, *Index to* The Nation. See also: Burks, *Bibliography*; Fisch and Haskell, *Additions to Cohen's Bibliography*; MS L 159.229; MS 1465 (draft).

We hardly knew what to expect of Professor Royce's introduction to Fiske's Cosmic Philosophy in the new four-volume edition (Houghton, Mifflin & Co.), for surely if anything is foreign to Royce's thought, it is Fiske's. It proves to be, without exception, the best introduction to a nineteenth-century philosophy that we have seen. In skill, the comment is a good match to the text. It renders Fiske's work much more useful than it has been, by pointing out just what there is in it and where its originality comes in. It fills about a hundred and fifty pages easily read, easily referred to. As compared to Spencer's own encyclopædic instrument of torture, Fiske's lively treatise always had a vast advantage as a way of making acquaintance with the fundamental philosophical doctrines of the evolutionary pioneer; and this advantage is now increased by the attractiveness of the new edition, which we were almost tempted to pronounce the most pleasing dress in which any American book of philosophy has ever been arrayed. That, however, would be forbidden, if by nothing else, by one volume from the same Riverside Press: we mean the elder James's 'Substance and Shadow'--a book not to be forgotten on any account.

76 (30 April 1903) 349-351: THE NATIONAL ACADEMY MEETING

CSP, identification: Haskell, *Index to* The Nation. See also: Burks, *Bibliography*; *List of Articles*; MSS L 159.231, L 159.234.

WASHINGTON, April 23, 1903.

The session of the National Academy of Sciences, which has just been brought to a close, has been one of unusually varied interest. From the point of view of utility to human life, the most important paper was one by Dr. Weir Mitchell, in which he announced the acquisition of an antidote to the poison of the rattlesnake. It was many years ago that Dr. Mitchell originated the theory, soon demonstrated by himself and Dr. Reuchert, that the venoms of the cobra and of the rattlesnake, though they appear at first sight to be as different as possible in their effects, have the same qualitative composition. The bite of the cobra is almost always promptly fatal, without much local soreness or swelling. The victim dies before any such symptoms can manifest themselves. The bite of the rattlesnake is not usually fatal; but frightful swelling ensues, followed by a breaking down of the health, from which the person may not recover for years, or not at all--phenomena only too well known in those parts of our country where the *Crotalus* abounds. Yet both venoms are mixtures of the same two in different proportions. One of these is a nerve poison, acting, roughly speaking, like strychnine. If it does not kill promptly, its effects pass off, the matter is eliminated from the system, and that is the end of it. This constitutes nine-tenths of the cobra venom. An antidote for it has been known for some time. The other constituent, which is predominant in the poison of the rattlesnake, acts like a zymotic poison, dissolves some of the fine tissues of the blood cells and its vessels, and in particular destroys the resistance of the blood to bacterial poisons generally. It does not kill at once, but brings about fearful lesions, and lays the system open to all sorts of malignant influences. It is against this poison that Dr. Flexner of Philadelphia, a well-known student of Dr. Weir Mitchell,

has succeeded in finding a protective serum which has been tested upon guinea-pigs [[sic]] and other animals, and whose practical efficacy has been demonstrated.

Of a widely different kind of practical interest was a paper by Mr. Alexander Graham Bell, describing an invention applicable particularly to the construction of kites. The idea seems to have been a corollary from a remark of Professor Newcomb to the effect that since the weight of a flying machine of given shape would be proportional to the cube of a linear dimension, while the area of the supporting planes would be proportional to the square only of that dimension, the smaller a flying machine of given shape, the greater would be its proportional lifting-power. From this it would seem to follow that a flying machine or a kite ought to be built up of small independent elements. In order to obtain the greatest three-dimensional stability, the eminent inventor gives to each of the elements out of which the structure of a kite is to be built up the form of a regular tetrahedron or triangular pyramid, formed of six equal bars. He calls attention to this tetrahedral form of elementary framework as recommending itself by its extraordinary stiffness and lightness, not merely for kites, but wherever framework is to be used. It would certainly seem to merit the attention of engineers. Mr. Bell forms a larger tetrahedral element of structure by connecting four such tetrahedral frames so that their bars shall be parallel, leaving an octahedral vacancy between them; and with numbers of such larger tetrahedra he builds various kinds of structures for which lightness and three-dimensional stiffness are needed. In particular, by stretching membranes over all the small triangles that are parallel to two of the triangles of one of the smallest elements, he obtains a kite which experience has proved to be a very light flyer. Photographs were exhibited showing kites built up in several ways from such elements, and in actual flight.

Two distinct studies, of which accounts were given, seem to have been provoked chiefly by the extreme discordances which different determinations of melting-points often show. One of these, by the celebrated chemist, Prof. J. M. Crafts, had been directed towards a more accurate method for the measurement of temperatures up to about 400° C. The proposal was to observe directly the vapor tension of naphthalene, which boils at 218° C., of benzophenone, which boils at 306° C., or of mercury, which boils at 360° C., according to the temperature whose ascertainment might be desired. This method would require, as a preliminary, a very accurate series of experiments to be made, once for all, in order to ascertain the tensions of those vapors at different temperatures. The other study, by Mr. Arthur L. Day, who was introduced by Dr. Becker, and who explained the matter with admirable perspicuity and most agreeable delivery, related to the phenomena of the heating and cooling of anhydrous borax, as the beginning of an investigation into the melting-points of rock-forming minerals.

It appears that if a body with a sharply definite melting-point, such as silver, be allowed to cool, under constant conditions, from a temperature considerably above the melting-point, the cooling proceeds regularly, though at a slightly diminishing rate, until solidification sets in, when its temperature suddenly becomes fixed until the entire mass is solidified; after which the cooling begins again at somewhat the same rate as before. Analogous phenomena are observed when the body is

heated from the solid condition. But when borax melted to a thin liquid is allowed to cool to a glass, the only hitherto known form of solid anhydrous borax, the cooling is continuous and without any particular irregularity from beginning to end, the heat of solidification being gradually evolved as the borax cools; and the same phenomenon appears when the glass is melted. If, however, the molten borax during the process of cooling, and while at any temperature between 741° C. and 490° C., be jarred or subjected to other influences which usually induce crystallization, a very marked retardation of the cooling will at once ensue, no matter at what temperature between those limits the jarring or whatever agency it be takes place; and what results after the cooling will be found to consist of a mass of crystals, a hitherto unknown form of this salt. If, now, anhydrous borax in this new crystalline form be heated again, it will be found to behave like silver, having a perfectly definite melting-point at 741° C. Quietly cooled, it will be reconverted into the vitreous form, or, if sufficiently shaken at any point below the melting-point of the crystalline form and above 490° C., it can be recrystallized. Below the latter temperature the vitreous form becomes incapable of crystallization by any means that could be employed. These facts suggest a probable explanation of the discrepancies in the observed melting-points of rock-forming minerals, although there is no reason to suppose that all the large disagreements in determinations of melting-points are thus to be accounted for.

Prof. Crafts gave, besides, another instalment of his researches into the catalysis of concentrated solutions researches which ought to excite a lively interest among the higher physicists, and doubtless do so. Two other chemical papers were extremely interesting. One of these, it is true, was merely historical, being an account, by Professor Barker, of the researches of the late Mathew Carey Lea chiefly into the mode of action of light in photography, showing how patience and genius had, after a long chase of elusive facts, finally led that admirable chemist to run down the so-called photosalts. Some beautiful specimens of Carey Lea's allotropic silver prepared by him were shown at the meeting. Pure silver of the color of gold, and pure silver approaching the color of copper, are very suggestive sights in view of the chemical relationship of these three metals; silver being intermediate between the other two in chemistry as it is in trade. There was also a whole sunset of gradations between the golden-colored silver and a soluble silver of a purple hue. Another paper by Prof. Barker refuted last year's assertion of Hoffmann and Zerbann that thorium owes its radioactivity (or unceasing emanation of peculiar rays) to admixtures of uranium, thus advancing our understanding of that subject by one essential step. But the interest of this particular point was quite absorbed in that of Dr. Barker's clear account of the whole history of research into radioactivity, beginning with the discovery of the Becquerel rays in 1896.

It appears that the radioactive elements are four: thorium (which has the highest atomic weight of any known element), radium (whose atomic weight has been ascertained to be about 225), polonium, and actinium (both of unknown atomic weights). Specimens of the salts of the first three were exhibited. Actinium has never gone out of the laboratory, where alone it has been found. The radiations of these bodies are of three different kinds. The simplest, known as the á-rays,

consist of those particles a thousand of which are said to compose an atom of hydrogen. They are shot out with a velocity of the same order as that of the propagation of light, and are charged with negative electricity. Others, known as the {a}-rays, consist of relatively large and heavy particles, of slower motion and positively charged. The third kind, known as the {g}-rays are remarkable for their penetrative power, their intensity not being reduced as much as one-half by passing through three inches of metallic aluminium. In some of the exhibits they had so shone through a brass stencil-plate that the letters could not be made out on the photographs. The radiations are all invisible, but there are two ways of recognizing them. They can be photographed, and, by rendering the air a conductor of electricity, they discharge any pair of oppositely charged bodies between which they pass in a suitable way. Two of the radioactive bodies--to wit, radium and thorium--seem to emit effluvia, which have the property of rendering any bodies they reach radioactive; so that, after a long course of experimentation, Professor and Mme. Curie found that the furniture of the room and even their own persons had become so radioactive that it was impossible to make any use of an electroscope, and their work had to be suspended. Polonium has no such power of exciting secondary radioactivity; nor has uranium. Professor Barker exhibited photographs which, in view of the circumstances under which they were taken, conclusively proved thorium to be primarily radioactive when entirely free from uranium.

Of the remaining papers, the one most generally interesting was that of Mr. George E. Hale of the Yerkes Observatory, giving an account of his work with his spectro-heliograph, for which the Academy at this meeting voted him the Draper medal, and for which he received his month the Rumford medal of the Boston Academy. It would be impossible to give much idea of this elaborate work without fine engravings, for which we can only refer to the magazines; they will doubtless do justice to it. We must limit ourselves to saying that Dr. Hale has found means to photograph, all over the face of the sun, a particular stratum, or rather two distinct strata, of the lower part of the chromosphere, which is that outer rind of the sun whose splashes or eruptions, whichever they may be, produce the red prominences visible to the naked eye in total eclipses.

A paper by Mr. Lewis Boss, a model of skilful manipulation of a vast mass of numerical data, showed precisely the effect of differences of brightness of stars in accelerating or retarding the observed times of their transits. A very elaborate and meritorious study of the tides of the northern Indian ocean, by Mr. R. A. Harris of the Coast and Geodetic Survey, who was introduced by Prof. Cleveland Abbe, gave evidence that the Survey, under its present management, is not forgetting the obligations entailed upon it by its scientific past. *Noblesse oblige*. The reading of a biographical memoir of T. E. Holbrook by Dr. Theodore Gill, the eminent ichthyologist, closed the preceedings [[sic]] very appropriately with a handshake between science and human life. Dr. Holbrook was a South Carolinian, who died at a good age during the war of the Rebellion. He was an ichthyologist of the finest scientific quality, well know to Louis Agassiz, to Jeffries Wyman and a few more, but known to very few even of the scientific world. Forty years after his death the National Academy reads, as it were, the "Siste, viator," and drops a tear over the monument that Dr. Gill sets up over him. As usual, in the hurry of the meeting, a number of good papers went unread.

The Academy added five regular members to their number: Chamberlin of Chicago, geologist; James of Harvard, psychologist; Thurston of Cornell, engineer; Webster of Clark University, mathematical physicist; and Mark of Harvard. biologist. It also elected the following foreign associates: Picard of Paris, Marey of Paris, Backlund of Pulkova, J. J. Thomson of Cambridge, Brögger of Christiania, Ray Lankester of London, Vogel of Potsdam, Pfeffer of Leyden, Mendeléef of Petersburg, Zirkel, the petrographer, and Koch of Berlin. The next meeting will be held in Chicago, beginning November 17.

76 (21 May 1903) 418: NOTES

CSP, identification: Haskell, *Index to* The Nation. See also: Burks, *Bibliography*; *List of Articles*; MS 1466 (draft).

--An octavo of 543 pages (Longmans, Green & Co.) is entitled 'Higher Mathematics for Students of Chemistry and Physics,' by Dr. J. W. Mellor. It consists of a limping treatise on the calculus and matters connected therewith, including trigonometry, the theory of equations, and the like, to which are appended fifteen pages of numerical tables. It will prove a handy instrument for anybody who has not the same thing in more convenient form on his shelves; but its limitations will be a perpetual vexation. Such a book ought to be written by a master of the subject, a very different person from Dr. Mellor. Mathematics is useful to the physicist in two ways. First, it enables him to solve his own problems instead of employing a mathematician. In this respect it is a great convenience, but not indispensable. Secondly, it supplies him with fundamental conceptions and methods of thinking without which he never can rise from the ranks of the army of science. A volume like this can be of service only in the former way. The prime necessity for a chemist or physicist who proposes to do his own sums is a mastery of synthetic geometry. Beginning with the new doctrine of multitudes and ordinal numbers as a propædeutic, he should first familiarize himself with the results and methods of topics--the geometry of a universe of distortable but unbreakable things, no standard of straightness or of length being supposed. Next should come graphics, the doctrine of unlimited straight lines, comprising perspective, projective geometry, graphical statics, etc.--invaluable tools, all. Next, metrics, comprising the elements of geometry, Euclidean and Non-Euclidean, the Brocard geometry, etc. Of all this synthetic mathematics Dr. Mellor gives not one word; and his book should have been entitled, not Higher Mathematics, but Mathematical Analysis. Passing to this, the student should begin with algebra and analytic geometry, of which Dr. Mellor gives extracts. He should then make himself as familiar with the calculus of finite differences in its entirety as with his mother-tongue. Dr. Mellor allots about twenty-five pages to the fringes of this subject. Then should come the differential calculus, including trigonometry and the theory of functions, in its fullest modernity. Here quaternions may be taken up. Finally, such subjects as probabilities,

rigid dynamics, molecular dynamics, hydrodynamics, viscosity, aerodynamics, must be studied. It is a vast course; but one cannot take high rank among the coming generation of physicists or chemists at any cheaper rate. A hand-book of rules and formulæ, of which there already are several, though none which accurately meets the need, would be, of course, an immense convenience. Numerical tables are best given in separate collections. A collection better suited to the uses of physicists than any now existing could easily be made, but in any case he would require his separate Crelle's 'Rechentafeln.'

76 (21 May 1903) 419-420: HEGEL'S LOGIC INTERPRETED

Hegel's Logic: An Essay in Interpretation.

By John Grier Hibben. Charles Scribner's Sons. 1902. 12mo. Pp. 313.

CSP, identification: Haskell, *Index to* The Nation. See also: Burks, *Bibliography*; *List of Articles*; MSS 1467, 1467 (s) (drafts).

John Grier Hibben (1861-1933) was president of Princeton University from 1912 until 1932, succeeding Woodrow Wilson. Hibben was graduated A.B. from Princeton in 1882 (valedictorian of his class), A.M. in 1885, and Ph.D. in 1893. As a teacher, he concerned himself with biblical studies, logic, and psychology.

Hegel made easy; Hegel brought to the level of the meanest capacity. It is really surprising how clear these things can be made, if they are disembarrassed of some tangles which are perhaps not so essential as they have been thought to be. The first pages delight one; but as one reads on, one begins to think that the effect is very much as if one were to explain to a child of five in the clearest terms, quite on the plane of its thought, all the facts and truths of life, domestic and social. That that child would understand life just as an adult understands it, is true in the same measure that Professor Hibben's reader will understand Hegel's philosophy as Hegel understood it. Those parts of Hegel's doctrine which set ordinary logic at defiance--that is to say, the woof and warp of his whole work--are treated as being merely a manner of phrasing. As we turn over the leaves, our eye lights often upon such expressions as "only an epigrammatic expression" (p.86), "his epigrammatic manner" (p. 130), "striking epigram" (p. 37), "an enigmatical form" (p. 115), "the Hegelian figure" (p.

157), "should be taken in a figurative rather than a literal sense" (p. 113), "in his characteristically paradoxical manner" (p. 152), "a paradox, as thus expressed" (p. 151). Once only do we find any intimation that to Hegel himself these things were more than that; and even then it is passed by without one full sentence being given to it.

Whoever has dipped into Hegel ever so little, will ask in astonishment how it is possible to interpret him so. The explanation seems to be that the master's incessant overstraining of reason, without which his system could not have been put together, seems in this disciple, as in many another, to have had the effect of destroying all the tonicity of his thought and leaving it lax and flaccid. Here is a professor of logic who cannot even state the ordinary doctrines of his own discipline accurately. "The general standpoint . . . was that thought presents to us . . . alter

native judgments, one of which must be wholly true and its opposite wholly false." Who ever maintained such a position? The opposite of the wholly true is that which is *more or less* false. But, in accordance with this, Professor Hibben claims that "it is only when his [Hegel's] entire system is unfolded that we can have any basis for judging" it. Until a man's whole say is said, one will generally be unable to pronounce that it is wholly true; but he need sometimes go but very little way before betraying that it is more or less false, or even quite unscientific. "The traditional law of logic known as that of excluded middle [meaning the *principle* of excluded middle] is enunciated thus: 'that of two opposite predicates, one, and only one, can be assigned to one and the same subject.'" Now if a professor of logic deliberately teaches what he ought to know is false, how can we otherwise excuse him, or avoid one or another more serious accusation of him, than by saying that he is a loose thinker? The principle of excluded middle does not belong to the so-called "traditional" logic, since, although it occurs in Aristotle as a definition of the kind of opposition called contradiction, and in other passages in dissent from a statement of Plato's, yet it plays no important part in Aristotle's system, and was first made a fundamental principle by Wolf. The whole purpose of the name and of the enunciation of the principle is to render it clear that what Professor Hibben wrongly states as this principle involves two distinct principles: first, that an affirmative and its corresponding negative predicate--as "black" and "not-black"--cannot both be true of the same definite subject, in the same definite respect; and second (and *this* is the principle of excluded middle) that of two such predicates one or the other must be true of any *single individual*.

The author is equally unsuccessful in explaining the "principle of sufficient reason." He calls it "the fourth law of thought, which is associated with the name of Leibniz, and is known as the law of sufficient reason, viz.: 'Everything must have its sufficient ground,'" and he proceeds to expound it in two or three pages. This principle, for such it is usually called, not only is "associated with the name of Leibniz," but was originated by him in a more exclusive sense, perhaps, than that in which any other philosophical principle of equal renown can be attributed to any one author. Nothing bearing more than a faint resemblance to it has been found in Plato, Aristotle, the Stoics, Suarez, Descartes, Spinoza, or any other philosopher, not even in Yakob Thomasius. It is variously worded in German, but usually somewhat thus: "Alles Zufällige hat seinen zureichenden Grund." Here, the German word *Grund*, corresponding to *raison* in Leibniz's formula, should be translated "reason." Whatever Hegel meant by *Grund*, it is not the "reason" of this principle. Another inaccuracy in Prof. Hibben's statement of it, though usual enough, lies in the words "must *have*." Leibniz was far too nominalistic to think that existing things really *have* reasons. He may seem to say so, using the phrase "there is a reason"; but what he means and sometimes says is, that a "sufficient reason for the thing's being as it is *can be found* out by the intellect." One of the recent treatises on logic formulates the principle very correctly, at least on one side of it, as follows: "Jedes behauptende Urtheil is zureichend zu begründen." It is undoubtedly implied by Leibniz that a really sufficient reason must refer to some operative condition of the reality of the fact. But to assert, as Hibben and some other Hegelians

have done, that Hegel's theory of reality ("Die Lehre von Wesen") is implied in the 'Monadologie,' is simply to put into a strong light the deleterious effect upon the brain of much reading of Hegel.

If we had the space to give to it, we could show that there are few, if any, statements in this book concerning the ordinary doctrines of logic or concerning the present state of logical discussion that are anything like accurate. Professor Hibben's own thought, in the few instances in which he has permitted himself anything like a free use of reason, is washy; and the maxims that he holds up for admiration--virtually, for example, that precise definitions, and precision generally, should be eschewed--are calculated to confuse discussion and to prevent its issuing in the test of experiment. Such, for instance, is the maxim "that every term which we employ in philosophical thinking should represent . . . an idea of universal and necessary significance, and that such a notion cannot have a one-sided, abstract, and rigid meaning." The proper maxim should be that ideas of universal and necessary significance and of a world of protean application may be taken one or two at a time as the *subject* of philosophical reflection; but that this reflection is of no use or meaning except so far as it is expressible in rules having reference to some conceivable practical upshot of all the thinking; so that it is the one-sided, abstract, and rigid conceptions that ought to be the philosopher's tools. Civilization, so far as physical science--including physiology and bacteriology--has had a hand in it, is the result of trying to find out what one-sided, abstract, and rigid formulae express the way in which events will happen; and quite amazing is

Professor Hibben's assertion that experientialists hold that "it is the function of thought to interpret experience, and not to anticipate it."

But, notwithstanding all we have said, it remains true that, if one does not care to trace and examine that movement of Hegel's thought which is supposed to be accurate, but wants only so much of his results and their relations to one another as is viewy and broad, then this book furnishes what one desires with greater success than any other we have seen. Of course, it is confined to the 'Encyklopädie,' and almost entirely to the 'Logik,' and that mostly as Hegel himself left it. It is a syllabus for lectures.

But all these Hegelians--Harris, Wallace, Hibben, Everett, etc.--who dog the steps of their master in almost textual comments, are profoundly unfaithful to the spirit of Hegel and of his philosphy [[sic]]. The 'Logik' was intended to be a mirror of the whole development of mind; and Hegel, with all that romanticism that was characteristic of his epoch, was far more essentially and determinedly a man who wished to be up to date in all his mental development. Now ninety years have paraded before us since the 'Logik' was written; and the result is that it now condemns itself. In the first place, the system, not in its deeper and truer spirit, but as it is worked out, and notwithstanding a sop tossed in one of the closing sections, is anti-evolutionary, anti-progressive, because it represents thought as attaining perfect fulfilment. There is no conceivable fulfilment of any rational life except progress towards further fulfilment. The 'Logik' is supposed to mirror the history of mind; and its first step is made to correspond to Thales, who ninety years ago seemed to stand at the threshold of thought. Thales, however, lived only twenty

five centuries ago; and we now know that men read and wrote fifty centuries before him, while the development of mind began countless eons before man became man. And it is evident enough that all Hegel's categories properly belong to his third grand division, the *Begriff*. What, for example, could be more monstrous than to call such a conception as that of Being a primitive one; or, indeed, what more absurd than to say that the *immediate* is *abstract*? We might instance a dozen such self-refutations. That the Hegelians should have allowed the obviously unsuccessful development of the doctrine of *Wesen* to stand all these years uncorrected, is a striking instance of the mental fossilization that results from their method of study. A powerful and original study of what the true Hegelian doctrine of *Wesen* should be, according to our present lights, might breathe some real life into a modified Hegelianism, if anything could have that effect.

76 (28 May 1903) 436: NOTES

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

Mr. E. T. Whittaker's 'A Course of Modern Analysis,' while not forming a complete and rounded whole, will greatly interest the genuine student of pure mathematics; and even those who are pretty well up in the subjects with which it deals will be very glad to refresh their ideas with this compact book, especially on account of the recent results that it contains, as well as for its giving some developments that cannot be called recent, but which have as yet hardly made their way into text-books. We do not consider its standard of logicality or of accuracy of statement to be the very highest; but this is perhaps an advantage, as it keeps the reader's mind on the *qui vive*. After eight chapters concerning series, residues, and connected topics, it proceeds to give an excellent chapter each to the Gamma Function, Legendre Functions, Hypergeometric Functions, Bessel Functions, and Laplace's Equation, and three chapters to Elliptic Functions--that is, to Weierstrass's forms, to Jacobi's forms, and to General Theorems, respectively.

76 (4 June 1903) 462-463: **Personal Idealism: Philosophical Essays by Eight Members of the University of Oxford.**

Edited by Henry Cecil Sturt. Macmillan. 1902. 8vo, pp. 393.

CSP, identification: Haskell, *Index to* The Nation. See also: Burks, *Bibliography*; *List of Articles*; MSS L 159.215, L 159.223; MSS 1468, 1468(s) (drafts).

The writer of the opening essay of this volume is one of Oxford's importations of four or five years ago, George Frederick Stout, of St. John's, Cambridge, called to Oxford when his eminence in psychology had become unquestionable. He here undertakes to resolve some of the sophisms of Ward and Bradley, mingling logic and psychology in the manner peculiar to him, until the reader doubts whether

Mr. Stout could answer correctly which of these subjects it is that he is dealing with. The second essay is by Ferdinand C. S. Schiller, author of 'The Riddles of the Sphinx,' and it is the liveliest, and, as one would say, the most brilliant, in the book. William Ralph Boyce Gibson discusses the problem of freedom in its relation to psychology. Without astonishing originality or clearness of thought, he presents considerations substantially sound, and so commonly overlooked as to be well worth urging. There is, he says, besides the study of the modern psychologists, another science dealing with Mind as conscious of final causation. This naturally suggests a swarm of questions, some of which Mr. Gibson passes without notice, while some he answers or half-answers. George Edward Underhill's paper on "The Limits of Evolution," which argues that the evolutionist cannot deal with origins and unavoidably assumes the existence of laws not subject to development, may be reckoned as padding. Robert Ranulph [[sic]] Marett treats of "Origin and Validity in Ethics," preaching the clear truth that Validity is the primary principle in this field.

One would expect that students who are moved by the conviction that enough has not been made of personality in philosophy, would anchor their bark on the rock of ethics. Yet of these eight essays, two only are ethical, since Mr. Gibson, though he treats of the problem of freedom, does so in the sole interest of psychology. The succulent paper of the editor, on "Art and Personality," sandwiched for no obvious reason between two dry slices of ethics, will prove, we think, the most useful to philosophy of the whole eight. We shall not insist that the writer shows signal skill in hitting his nail squarely on the head, but he manages, after a fashion, to get a sufficient part of it driven home. At any rate, he certainly brings together a considerable number of items of thought bearing upon the question of æsthetics which it will be highly convenient to have thus collected. We hope to hear more from this new philosopher. Messrs. Boyce Gibson, Marett, and Sturt belong to a class of thinkers whose work we shall value more and more as the day of heroes in philosophy fades away *al ponente*. Dr. Frederick William Bussell considers "The Future of Ethics: Effort or Abstention?" Finally, Rev. Hastings Rashdall, best known for his book on the mediæval universities, attempts to analyze personality, and gives in his adhesion to the limitation of God, as against the absolutism of Bradley and other metaphysicians in vogue.

The tendency vaguely described on the title-page is probably destined to play a prominent rôle in the thought of the twentieth century; but even those who believe that some such view will ultimately be found to approve itself after the oscillations of opinion shall have subsided, can hardly expect this publication to shake opinion as it must some time be shaken if metaphysicians are ever to come to any agreement. Mr. Schiller thinks they never will do so; and, furthermore, that they never ought to. Philosophy, he thinks, ought to be regarded as a matter of personal fancy. "The whole history of philosophy shows that the fit of a man's philosophy is and ought to be as individual as the fit of his clothes, and forms a crushing commentary on the intolerant craving for uniformity.... For this reason any philosophy is better than none." That is, one must not go metaphysically naked, like Truth in her well, but whatever opinion one takes a fancy to,

will answer every essential purpose. Lutoslawski's master, of unpronounceable name, can hardly more magnify the element of human wilfulness. Nevertheless, the assortment and confrontation of opinions, if carefully studied, may have a fine effect. There is eventually to be a "harmony" of metaphysical systems, though no "uniformity," differences of philosophical belief being "too deeply rooted in human idiosyncrasy to be eradicated." Mr. Schiller does not believe there are any hard facts which remain true independently of what we may think about them. He admits it requires a hard struggle to make *all* facts suit our fancy, but he holds that facts change with every phase of experience, and that there are none which have been "all along" what history decides they shall have been. This doctrine he imagines is what Professor James means by the "will to believe." He is resolved that it shall have been so.

The main point of the essay, however, is that axioms are explanatory hypotheses--"postulates," the author calls them--which are suggestions coming from our needs, and which, in a measure, are found to fit the facts while in a measure they are forced upon the facts by formalisms. No doubt axioms and explanatory hypotheses may with some justice be thrown together under one heading, but the general theory is considerably more satisfactory than the author's attempts to apply it to the formulæ of logic, such as "A is A," for such a formula is simply an attempt to formulate in part what we perceive that we mean by "is." It thus rather resembles the assertion that a color before the assertor's eyes is red--that is, it resembles a direct judgment of perception; although doubtless this, too, might with some justice be likened to an explanatory hypothesis.

The general movement of thought which the book represents has certainly great vitality; and this signifies that it is destined to develop further. All the writers have plainly been much impressed by the method of the book, 'Riddles of the Sphinx.' Their bark is not anchored to any special position, and is destined to be carried far--they know not whither. We believe them to be the hardy navigators who will adhere to their method as long as it seems to them rational, wherever it may carry them.

76 (11 June 1903) 482: Dictionary of Philosophy and Psychology.

By James Mark Baldwin. Vol. II. The Macmillan Co. 1902. Royal 8vo, pp. 892.

CSP, identification: Haskell, *Index to* The Nation. See also: Burks, *Bibliography*; *List of Articles*; MSS L 159.217, L 159.228; MS 1469 (draft).

Many evidences of different kinds reach us of the good service that this work is already rendering, notwithstanding the imperfections inevitable in any such composite book, and notwithstanding its lack of those formal perfections and uniformities upon which our American dictionaries and cyclopaedias are apt to insist to the neglect of the weightier matters of the law, to the point of leaving them, dry, innutritious, and unvitalizing. Professor Baldwin, in the preface of this concluding volume (of the Dictionary proper), puts forth more an excuse than a defence for one of the few features of it that have been disapproved in almost every quarter; urging that the diminutive biographical notices which he has scattered through the vocabulary are that half-loaf that is said to be better than no bread. This hardly meets the stricture commonly made, which was to the effect that the entire omission of these supererogatory crumbs would have left room that might either have been filled to better purpose, or to better purpose have lightened the avoirdupois of the volumes.

But a more interesting question suggests itself. Upwards of seventy of the most reputable philosophers whose services a distinguished editor could secure, have here set down their opinions upon the special points of philosophy of which they are reputed best qualified to treat. They have not argued their doctrines, since this is a dictionary, not a cyclopædia; but they have defined them. All the principal groups of schools are more or less represented in the assemblage of contributors; even the idealists, whose showing is probably the least adequate. One naturally peruses their utterances to see what impression one can derive from them as to the prevalent tendencies of philosophy at the opening of the twentieth century; for surely this is an aspect under which it may be hoped that this dictionary will never lose its interest.

The most prominent of the philosophical signs of the times, as here displayed--so it strikes us, at least--is the manifest strenuous endeavor of the students of every department of philosophy to impart a "scientific" character each to his own particular branch, *i. e.*, to make it conform to the conditions which have caused the success of the modern acknowledged sciences. The progress is satisfactory. At least one branch of psychology has already taken its place among the special sciences, whose array others are well upon the way toward joining. The movement is not confined to psychology. There is much of a scientific character in ethics; and the critical part of logic has, in some hands at least, come to submit itself to the same criteria as those that have long been acknowledged in science. There seems every reason for hope concerning metaphysics and other branches.

Another mark of our philosophy is the disposition to make psychology the key to philosophy--categories, æsthetics, ethics, logic, and metaphysics. Something of this has existed since Descartes; but since about 1863 every student of philosophy, even though he be one of those who consider the present psychological tendency excessive, has placed a new and higher estimate than before upon the scientific value of psychology. Here was seen one science, than which no branch of philosophy, in the days when men disputed about the *primum cognitum*, was more enveloped in metaphysical fog, which yet almost suddenly, that mist lifting, had come out bright and clear as a June forenoon. How could it but happen, as it certainly did, that men should think that the best way to resolve any problem of philosophy would be to reduce it to a question of psychology? The future must determine precisely what the value of this method may be. It has its opponents. For some years after the movement once became general, no strong voice was raised against it; and ten or fifteen years ago psychologists of the first rank could dream of establishing the truths of their science without any metaphysical assumptions whatsoever. Some writers use such language even yet; but careful examination has convinced the

better part that even physics has its metaphysical postulates, and that psychology is peculiarly dependent upon them. If that be the case, the philosophical sciences and psychology would have each to be built upon the other, if the psychological method is to be maintained. They must collectively form an arch--or, rather, a Saturn's ring, for an arch has the ground to rest upon. Whether that can be sound logic or not is a question to be carefully examined.

Another symptom of the philosophy of the day which is interesting to the general public is a very appreciable reaction against the whole family of opinions that are nearly related to agnosticism--some of them as little fond of others as any cousins in the world. A logical scruple seems to be the motive of this reaction. It is felt that the only possible justification for so much as entertaining a hypothesis must be that it renders the facts comprehensible, and that a theory which substantially amounts merely to supposing facts to be incomprehensible, fails to render any facts comprehensible. But if one once admits this, he can hardly stop at this point. It would seem that his further reflections must result in something like a resuscitation of the Scotch philosophy of common sense. Accordingly, some writers who used (justly or otherwise) to be regarded as skeptics, are now instancing the stress put upon the light of nature by Galileo and other authors of modern physical conceptions, and virtually even by Faraday, Kelvin, etc., as helping to show that a belief akin to Reid's is an essential condition of progressive science.

One other lineament of contemporary philosophy is called to our attention in turning over the leaves of this volume. It is surprising to see how readable it is--a result due, no doubt, in part to editorial skill, and partly to the writers not having to enter into all the details of argumentation. It is infinitely more agreeable to read than any of the recent philosophical works which betray literary ambition. Metaphysicians are a slow-thinking breed; but they seem duller than ordinary not to perceive that a literary style in philosophy is an incongruity whose days are numbered. Soon the majority of contributions to philosophy will begin to take the form of memoirs, like those to other sciences, such as mathematics, which is no more special than is philosophy. Now a scientific memoir written in any but the most severely unadorned language could not be more ridiculous if it were set in hexameters like the contributions of those eminent savants Parmenides, and Empedocles. It is a truism to say so; and this truism enwraps another, which is that there is such a thing as a good style and a bad style for philosophy grown science. A good style is one which approximates as closely as possible to a self-explaining diagram or a tabular array of familiar symbols. In short, it will be necessary for philosophers to awake to the fact that there is such a thing as the ethics of words, which for them should be about the most sacred part of the moral law; and the sooner they begin to turn their attention to this, the sooner they will experience the satisfaction of the scientific man's conscience, who is faithful to his duty of gathering premises as the basis of inferences which only distant generations can draw, and in drawing will first discover what scrupulous pains have been taken to make those premises accurate.

76 (18 June 1903) 497-498: NOTES

CSP, identification: Haskell, *Index to* The Nation. See also: Burks, *Bibliography*; *List of Articles*; MS L 159.224; MS 1470 (draft).

--In 'Kant's Prolegomena to any Future Metaphysics, edited in English by Dr. Paul Carus, with an Essay on Kant's Philosophy, and Other Supplementary Matter' (Chicago: Open Court Co.), we find a careful and excellent translation of a work whose importance Kant's students sometimes fail to appreciate, while the present editor extravagantly says that it "is indubitably the most important" of Kant's writings. What is *indubitable* is doubted by no competent person. Dr. Carus perhaps refuses this title to all other writers on Kant. He says in the preface: "The present translation is practically new, but it goes without saying that the editor utilized the labors of his predecessors, among whom Prof. John P. Mahaffy and John H. Bernard deserve special credit. Richardson's translation of 1818 may be regarded as superseded and has not been consulted, but occasional reference has been made to that of Prof. Ernest Belfort Bax."

We are glad Dr. Carus did not let it go altogether without saying that he had "utilized" the translation of Mahaffy (published only after revision by the Rev. Dr. John Henry Bernard); but his readers might have preferred to be informed of the extent to which such utilization went. In point of fact, Dr. Carus's "practically new" translation agrees, in all but a small percentage of its matter, with that of Mahaffy *verbatim et literatim*; and this would seem to be dictated by good sense, supposing Mr. Mahaffy consents. Why make a new translation where an existing one is good? The most striking difference is that Mahaffy prints in italics the phrases which Kant spaced to the same effect. There are also some quite trifling divergencies in the translation of a few of Kant's technical terms. A few idiomatic expressions have been more familiar to Dr. Carus, and here the changes, though small, are unquestionable improvements. It may be added that Mahaffy's translation was in part based on that of Richardson, though the alterations are so great that the fact would hardly have been discoverable without Mahaffy's statement of it, duly made in his preface. The "supplementary matter" consists, one half of it, of selected extracts from various writers, all well worth reading and some of them decidedly amusing for the reflected light they throw on their authors. The other half is Dr. Carus's account of Kant's philosophy. To our thinking he subtracts somewhat from Kant's doctrine and thus renders it more acceptable, without beginning to cut deep enough to meet the exigencies of modern reflection.

77 (16 July 1903) 57-58: Studies in the Cartesian Philosophy.

By Norman Smith. The Macmillan Co. 1902. 12mo, pp. 276.

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

No matter how fresh one may come from the perusal of the works of Descartes himself, of the history of Bouillier, and of some of the swarm of more recent explicators of his metaphysics, one will find much to learn from the pages of this little book. It is composed in the manner which seems at this time to be most approved--that of arranging the topics, not according to the conceptions of the philosopher to be expounded, but according to those of the expositor, and of setting down under each head whatever the four winds of heaven can blow together from all the quarters of the subject's *Opera omnia*. In this way one can make an interesting cento; but whether it will represent any stage of development of the thought of the philosopher to be expounded, will be a question to be answered separately in each case. Critics who use this method are apt to forget that the thought of the most stupid and wooden of metaphysicians will undergo considerable development in the course of years, and that this is far more true of any philosopher who is likely to impress men as great. Still, there is no method of composing a work of this kind (or, for that matter, of any other) which can absolve the reader from the necessity of critical thought in getting the juice of it; and the very circumstance that a philosopher will take different views of any difficult matter at different stages of his development renders their juxtaposition instructive in regard to the abiding characteristics of his thinking. Nobody would go to a concordance to gain one's first acquaintance with a philosopher any more than with a poet; and Mr. Smith expressly says in his preface that "this volume is not designed to be an introduction to the study of Descartes." But a work of a nature somewhat approaching that of a concordance is of extreme utility as an aid in forming one's matured conception of whatever philosopher it may concern.

We must not, however, convey the impression that Mr. Smith makes it his business to compare all of Descartes's different statements upon any point. He does not write as a biographer nor even as a historian of philosophy. He does not treat the utterances of Descartes as historical curiosities, but rather argues as to their truth or falsity much as if he were examining a contemporary writing on philosophy. His whole exposition is so permeated with his own argumentations that one could not always gather from reading it exactly how much of it is actually in Descartes, and how much Mr. Smith thinks would be fairly attributable to Descartes considered as if he had written in the twentieth century. There is much in the book concerning which it is difficult to form a definite judgment in the absence of a full explanation of Mr. Smith's own philosophy; and no such explanation is vouchsafed. One may say, however, in general, that Mr. Smith finds fault with Descartes for not being nominalistic enough.

Mr. Smith is of opinion that the really important part of the system of Descartes is his philosophy of the sciences, and that this has but a very artificial connection with his metaphysics. Yet he confines his interpretation entirely to the metaphysics and to the writings on method in their relation to the metaphysics. He certainly succeeds in making the Cartesian metaphysics appear more crude and grossly monstrous than ever, whether its principal fault be that which he attributes to it, or whether it be not quite in the contrary sense that it sins. In either event, Mr. Smith's conspectus of the Cartesian metaphysics is profitable reading. Leaving that, he passes on to some decidedly interesting chapters on the Cartesian elements of the philosophies of Spinoza, of Leibniz, and of Locke; and these are followed by an approving account of Hume's attack upon Cartesian rationalism and spiritualism;

the volume closing with a chapter on Kant, whom Mr. Smith seems to value as a positivist and an agnostic.

77 (23 July 1903) 81-82: **Inorganic Chemistry: With the Elements of Physical and Theoretical Chemistry.**

By J. I. D. Hinds. John Wiley & Sons. 1902. 8vo, pp. 566.

CSP, identification: Haskell, *Index to* The Nation. See also: Burks, *Bibliography*; *List of Articles*; MS 1471 (draft).

John Iredelle Dillard Hinds (1847-1921) was an American chemist. Upon graduation from Cumberland University, he accepted the chair of science there. Having distinguished himself as a teacher. he was appointed dean of the university in 1894. In 1899, Hinds accepted a teaching position with the University of Nashville and, in a repetition of history, became dean there in 1907. Hinds is credited with the invention of the photometric cylinder, and he wrote several works on education and chemistry.

A dweller in town can do very well with no knowledge of zoölogy or botany; but everyday life nowadays demands acquaintance with a very considerable body of chemical facts, principles, and methods. For a first introduction to the subject, we have a work of genius, Mendeléef's 'Principles.' For physical chemistry there is the treatise of Professor Jones. We cannot see why it would not be better to begin one's descriptive study with the simpler, more instructive, and better understood--albeit the bulkier--organic branch. The reverse is the universal practice. It is a tradition from a time when the Berzelian theory appeared to render inorganic bodies the simplest things in the world, while organic chemistry was a chaos. However, all the books are written on the assumption that the student takes up inorganic chemistry first, and thus he is well-nigh compelled to do so. It is just as it is with geometry, all mathematics being written on the assumption that metrical geometry is studied first, projective geometry next, and topical geometry last of all, although no fundamental understanding of metrical geometry is possible except on a basis of projective geometry, nor of projective geometry except on a basis of topical geometry. The force of history asserts itself even in pure science. But arrange the curriculum as one will, there comes a day when the young chemist has to sit down and commit to memory a vast load of facts about the elements and their compounds. For that severe labor we have seen no single volume better adapted than this of Professor Hinds. It is well packed with well-chosen facts stated with the utmost simplicity; and there is not one that it will not pay a modern man to know by heart. A considerable number of extremely easy experiments are suggested, and the preface to this volume informs us that there is another for use in the laboratory. Such a volume is wanted.

One of the features which particularly adapts this treatise to memorizing is its strict adherence to Mendeléef's arrangement of the elements. Just here, too, is the principal fault of the book. It not only never suggests the inadequacies of that arrangement, as even Mendeléef himself often does, but it stuffs the new earthy metals into the Mendeléefian pigeonholes in a manner to make Procrustes grin. Praseodymium and neodymium, two elements so much alike that years of fractional crystallization are required to effect the separation of them, are here placed in different

groups with a black line running between them to show that one of them is basic, the other acidic. Gadolinium and thulium are earthy metals whose elementary nature is not the most certain of facts. They both come from the same mineral and are difficult for an ordinary chemist to distinguish. One of them is made by Prof. Hinds to be intermediate between silver and gold, while the other is grouped with chlorine, bromine and iodine; and there is no reason whatsoever for this except that their atomic weights, which are dubious to the last degree, bring them into those places. There are other cases just as bad. In short, the "periodic law" of the elements is treated as if it stood on a par with the three laws of motion.

No chemist who thoroughly comprehends the logic of such inquiries and who knows the history of attempts to classifly [[sic]] the elements as it is set forth in Professor Venable's 'Development of the Periodic Law,' can fail to admire supremely the wonderful discovery of Mendeléef. But in physical inquiries it often happens that the student notices a regularity which, upon following it up, continues to be fulfilled, but only in a rougly approximate sense. This state of things shows that there really is some such regularity, but that it has not been correctly formulated. There must really be such a law, or it would not continue to be fulfilled at all. It cannot be correctly formulated, or it would be fulfilled more exactly. Such a law often goes on very satisfactorily for a long time, if one is not too particular, and then suddenly goes quite to pieces, though perhaps later it may come out clear and definite again. This is what we ought to expect in the case of Mendeléef's law; for throughout it is quite wanting in anything like numerical definiteness. It should be regarded as proved that the relations between the metals of the rare earths are not represented at all in Mendeléef's table; and if its lower lines can be regarded as fitting the facts at all, the fit is a mighty loose one, a sort of ready-made fit that would suit very different facts just as well. We ought to add that any complete discussion of the evidences of the periodic law ought to take account of several other considerations which we have not space to notice here.

It is a serious fault in Professor Hinds's work that it treats the law as if it were perfect. It not only teaches what is not proved to be true, but even what is proved not to be true. It may be said that the table, at any rate, lends a support to the memory. So it does; but it would do so all the better if its inadequacies were pointed out. They would forcibly strike the student's attention and serve as landmarks, so to speak, in what otherwise might appear to him as a desert of uniformity.

77 (30 July 1903) 98-99: CLERKE'S ASTROPHYSICS

Problems in Astrophysics.

By Agnes M. Clerke. London: Adam & Charles Black; New York: Macmillan. 1903. 8vo, pp. 567. With 31 plates and 50 figures in the text.

CSP, identification: Haskell, *Index to* The Nation. See also: Burks, *Bibliography*; *List of Articles*; MS L 159.229; MS 1472 (draft).

The signal merits of this work will be recognized at once, nor will it cease in future to be esteemed as a breathing portraiture of youthful twentieth-century science

in the department of the physiology of suns. It is a book needed by all persons who endeavor to keep up an all-round intelligence of the advances of human knowledge, and will soon be found lying well worn on the tables in all observatories. Executed with a laborious thoroughness that never tires, it is brought to the polish of a popular style--if anything, too much so. Yet it is not a book of popular science: it is a popular book on professional science--a thing seldomer to be found and quite otherwise enlightening. It is not with knowledge, organized or not, that the active thoughts of the man of science are busy; but on the contrary with all sorts of hypotheses, the most dubious things in the world. To be acquainted with science as it lives in his mind will not help one to build a flying machine, a yacht, or a wireless telegraph, but will educate one in the conduct of inquiry. It is this side of astrophysics which is presented in this volume, by an author who, if not a very eminent astronomer, has participated in the life of an observatory, and who mingles with the ablest astronomers of England and of the world, recognized by them as one of their guild. No little astronomy can be learned by merely looking through the admirable illustrations to the volume, which, now that observation is so largely conducted through the medium of the camera, are in many cases as near the real thing that the astronomer uses as single prints can be to multiplied negatives.

Having thus had the pleasure of recording the principal merits of the book, we are in duty bound to acquaint our readers with such of its apparent shortcomings as will chiefly concern them if they read it. One of these, we feel quite sure, is an error of judgment in the matter of style. Tables and algebraical formulæ are avoided almost entirely. Now tables and formulæ may be dry and hard to comprehend, in the sense that the relations that they are fitted to express are so; but when it is precisely such relations that must form the substance of the discourse, and not for a paragraph only, but for five hundred closely printed octavo pages, by all odds the least fatiguing way of apprehending them is the way that exhibits visual analogues of them--as tables and formulæ do--and then points out in these icons the special features upon which attention must be directed, with a measure of emphasis proportionate to the effort required. Instead of endeavoring to do this, our author painfully searches out ornamental ways of stating abstract relations, with a view to literature. If she has occasion to mention that the parallax of Procyon, as determined with the Yale heliometer by the admirable skill of its manipulator, is 0".325, she will say that "Dr. Elkin has measured for it a parallactic shift of 0".325." The idea of a shift of any kind being measured for the little dog! If she wants to suggest that every star presumably turns on an axis, the word "rotation"--not, after all, quite rustic--will not be half elegant enough for her. She must say "it has a movement of gyration." So it is in countless cases. In short bits to be read one at a time, like those of Smyth's 'Celestial Cycle,' such rococo expressions will merely excite a smile; but when it comes to five hundred solid pages of detail to be read consecutively, they add to the labor of lading the mind with it all. For at each such novel expression the reader has to stop and consider what can be meant; and it is often a little enigma.

The only other fault of the book to which we shall advert is a mere matter of individual opinion, provided logic is a matter of individual opinion, as some hold

it is, and some even that it ought to be. We might give a graded series of examples of what we mean. We are told (p. 204) that "Canopus may be no further off, but cannot be nearer than a light-journey of 296 years." The foundation for this statement is that an attempt to determine its parallax gave zero as the result, with a probable error of 0".011, which parallax, according to Euclidean geometry (whose applicability to such vast triangles is open to question), corresponds to a distance of 296 light-years. But what is meant by the "probable error" of a determination is that value of the error (or rather of such part of the error as the method of least squares can take account of) which is equally likely to be greater or less than the real value. Therefore the inference (so far as any is justified) is that were the same inquiry pushed indefinitely it would be just as likely to make Canopus more than 296 light-years from us as less. It may be added, however, that such result would in all probability not be many times less.

Again, we are told that, "upon Doppler's principle" (or words to that effect), a certain star must be approaching or receding from the sun. In a memoir intended exclusively for professional astronomers such an expression would be innocent enough, but in writing for a larger public one has to guard against too literal interpretations. Doppler's principle does not necessitate any such thing, but only that *if* the star were approaching or receding, then, so long as no unknown cause acted, the spectral lines would all be shifted toward the red or toward the blue, as they are observed to be. But it is to be remarked that there is some reason to suspect that unknown causes do occasionally interfere with the effect.

Having had our scold at Miss Clerke's style, it will be but fair to give our reader a bit or two of it. Very likely in such doses its flavor will please:

"Sun-spots are not simply rents in a shining veil, exposing an obscure substratum. They are not superheated regions, where the processes of condensation are suspended. The photosphere is screened, not perforated, by them. Moreover, the screening is by interposed vapors. Umbral absorption is mainly, if not altogether, of the gaseous kind. It is essentially linear and banded. No part of it can be safely attributed to the action of a foggy precipitate such as modifies elsewhere the 'surpassing glory' of the disk. They probably differ in this respect from 'pores' and 'veiled spots,' but specific inquiries on the point have yet to be made. There are strong indications that spot-spectra originate under conditions of increased pressure and diminished temperature. Still, the coolest umbræ must be hotter than the reversing layer, for otherwise the Fraunhofer lines would show bright against them, and, as we know, they cross them in dusky array. This circumstance is fundamental in solar thermal relations, yet has been generally overlooked" (p. 96).

"Total eclipses have ceased to be indispensable for the prosecution of chromospheric studies. Day by day the red rim of the sun, with the strange forms protruding from it, can be viewed spectroscopically; and day by day the same objects vestured in violet can be photographed under the broad shelter of the Fraunhofer K-line. Nevertheless, noontide darkness, when it comes, brings very appreciable help. Differences are noticeable between what can be seen in and out of eclipse. According to the late Professor Tacchini, the chromosphere always appears deeper under cover of the interposing moon, because it is surmounted by a pinkwhite margin, giving continuous light, and therefore spectroscopically invisible in daylight. Some prominences are probably of analogous composition. Only their skeleton forms come out in the crimson radiance of hydrogen; they are compacted and clothed with white materials, the shining of which is effaced by the glare of common day.... The objects called 'white prominences' belong indeed wholly to the pageantry of eclipses. First noticed by Tacchini at Caroline Island, May 6, 1883, they showed as lucid jets about a hundred thousand miles high, with a surface like granulated silver. Attempts made, after the return of daylight, to view them prismatically proved fruitless; they gave forth no hydrogen or helium rays. Again, at Granada, 29th August, 1886, a gigantic helical structure, described by Mr. Maunder as 'of the intensest silver whiteness,' towered three hundred thousand miles above the limb of the moon" (pp. 109, 110).

The continuous reader would have been spared a pause if the author had printed *helical*, to show she simply meant eddying. She could not be expected to employ so vulgar a term as that. Another hitch in the reader's thoughts takes place when this structure is said to "tower three hundred thousand miles from the limb of the moon" though near a hundred million miles from the moon. The *limb* of the moon exists only in the vision of the spectator, and distances from it are properly measurable in degrees, minutes, and seconds, not in miles.

"An eclipse, visible in the Western States of North America, 29th July, 1878, disclosed a surprising spectacle. In lieu of the ordinary radiated corona there were seen 'bristles' of light at the sun's poles, enormous 'wings' at each side of the equator. Professor Langley observed the phenomenon from the summit of Pike's Peak in Colorado, at an elevation of 14,000 feet in a stainless sky. Thus favorably circumstanced, he was able to trace one wide beam to a distance of about five millions of miles from the sun, the other fully twice as far. The direction in which they lay proved, when carefully measured, to agree closely with that of the zodiacal light, and 'a faint central rib' emphasized the coincidence.... At the time of this eclipse the sun was in a state of exceptional tranquillity, and a search through the solar archives brought out the notable fact that a similar apparition had, eleven years previously--spots then too being nearly extinct--been described and depicted by Grosch of Santiago.... The concurrence of these phenomena with critical epochs in the sun's activity started the idea, due, in the first instance, to Mr. Ranyard, of varying coronal types. It was amply borne out by subsequent experience. From eclipse to eclipse, throughout the eleven-year cycle, the corona exhibits changes of form in marked conformity to spot-vicissitudes" (p. 127).

77 (13 August 1903) 141: Light Waves and Their Uses.

By A. A. Michelson. (Volume III. of the Octavo Series of the Decennial Publications of the University of Chicago). Chicago: University of Chicago Press. 1903. Pp. 166. With 108 figures in the text and 3 colored plates.

Waves and Ripples in Water, Air, and Æther.

By J. A. Fleming. E. & J. B. Young & Co. 1902. 12mo, pp. 299. With 85 figures.

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

Albert Abraham Michelson (1852-1931) emigrated from Germany to the United States at the age of 2. He was a graduate of the U. S. Naval Academy (1873) and taught at Case School of Applied Science, Clark University, and the University of Chicago, where he was head of the physics department. Michelson's most notable achievement was the invention of the interferometer, which enabled him to determine linear distances in terms of the wave length of light. So valuable was this invention that an international commission of weights and measures in Paris employed Michelson to devise a method by which the standard meter length could be accurately reproduced. After a series of experiments, Michelson established the length of the meter in terms of cadmium light waves.

Sir John Ambrose Fleming (1849-1945) was a British electrical engineer, and is best known for his invention of the wireless valve. Most of his academic life was spent as professor of electrical engineering at University College, London. In England, Fleming was known as one of the leading expositors of photometry, and he was closely associated with Marconi's wireless transmissions.

Professor Michelson's book is devoted exclusively to an account of his own researches, the great importance and beauty of which are well known. This compact outline of them in a handsome dress will be welcomed by everybody who is interested in optics. This investigator's whole course of thought and of experimentation has been the logical result of his looking at the action of all optical instruments from the point of view of interference. We only wish that the exposition had been even more strictly autobiographical than it is.

Professor Fleming's little volume embodies six lectures delivered to a juvenile audience at the Royal Institution. Two are devoted to water waves, two to air waves, two to Hertzian and other ether waves. In the first two of these three divisions the author displays a charming power of making things plain. How, for example, a group of waves can have a velocity much less than that of any of the waves that compose it is rendered entirely obvious. Here, too, we find various most interesting observations which, though already printed, were novelties to us and are certainly not easily accessible, unless one is provided with files of such things as *Pearsons's*, *Cassier's*, and *Harmsworth's Magazines*, of various technological periodicals, and of other sources to which there is no admission for any rational being except on business. But when he comes to Hertzian waves, Professor Fleming is seriously handicapped by two circumstances unfavorable to lucid exposition. In the first place, he is himself in the very thick of the struggle to advance this subject. We are aware that people who consider the matter hastily often reason that active investigators ought to produce the most easily comprehensible expositions, because clear thought always produces clear expression. But this reasoning involves two very false assumptions--that clear expression is the only thing required to render an exposition easily intelligible, and that those who first succeed in thinking out a problem think the solution in the clearest possible way. The truth is, that every invention, in its first workable form, is unnecessarily complicated; and that which is complicated is hard to understand, however clearly it be explained. But, in the second place, Professor's Fleming's mind is full of the new electron theory.

Now this theory, at the present moment, seems to be somewhat in the condition of the Copernican hypothesis before Kepler. That is to say, it seems to offer an immense simplification in one respect, while it leaves the details quite as complicated as before, if not more so. These two circumstances appear to us to have decidedly weighted down the author's explanation of wireless telegraphy. We take the liberty of doubting whether the juvenile audience really understood it. At any rate, we are confident the matter might have been rendered more comprehensible without the sacrifice of any important feature.

77 (3 September 1903) 189: NOTES

CSP, identification: MS 1473 (draft).

Chemists and microscopists designate certain mixtures and processes by their devisers' names; and of these terms Mr. Alfred I. Cohn, favorably known by a former book on indicators, has now compliled [[sic]] a dictionary of some three thousand entries, entitled, 'Tests and Reagents' (D. Van Nostrand Co.) He disclaims all pretensions to completeness; and certainly it were to be wished that Carnot's test for potassium had been inserted, as well as Flandin and Danger's method of detecting arsenic, this having as good right to a place as the method of Fresenius and Babo that Mr. Cohn duly books. Devanda's alloy, being used in analysis, might be looked for; and it were better not to disappoint expectations having any shadow of justification. Still, on the whole, we do not think the compiler need fear dissatisfaction on the score of omissions. Nor, on the other hand, can harm be done by the appearance of the fuming liquor of Cadet under an odd disguise suggesting that eminent pharmaceutist's searching out a test for cacodyl in the year 1760, nor in Scheele's green figuring in similar fashion. What will be perplexing is that a person wanting to know, for example, what Mayer's solution is, will find himself confronted with twenty-three articles headed "Mayer," with no way, short of going through them all, of making sure which of them is commonly known as Mayer's solution. Nevertheless, despite imperfections incident (among other things) to first editions of original dictionaries, the book promises to be useful to those for whom it is intended, as it already is curious to those for whom it is not intended. A forty-three-page index enhances its value.

77 (10 September 1903) 208: NOTES

CSP, identification: MS 1474 (draft). See also: Burks, *Bibliography*.

Dr. C. Krauch's book, 'The Testing of Chemical Reagents for Purity' (D. Van Nostrand Co.), is a standard work of the highest authority and of wide utility. Its translators, T. A. Williamson and L. W. Dupré, have done their work well, and have sometimes substituted references to English books for those to German books, when they have the same value. The author has contributed some emendations.

77 (10 September 1903) 208: NOTES

CSP, identification: MS 1475 (draft). See also: Burks, *Bibliography*.

Frederic Auten Combs Perrine (1862-1908) was a distinguished electrical engineer, well known in both the academic and business worlds. He was graduated A.M. with honors from Princeton University in 1886. Seven years later, he was appointed professor of electrical engineering at Stanford University.

Dr. F. A. C. Perrine's work entitled 'Conductors for Electrical Distribution' appears to have left few problems pertaining to its subject untouched, however minute they may be--from the most economical way of transferring bars from one roll to another to the best way of drawing specifications for telegraph poles--and to have reduced every one to a question of exact science. The vivid clearness of the elaborate descriptions of the manufacture of wire, its finishing, insulation, and wrapping into cables, is like visiting the different works under the guidance of their masters.

77 (10 September 1903) 219: NOTES

This note, by an anonymous author, is background information for the note by Peirce immediately following.

The inefficiency of the British universities and the illiberality of the British public towards institutions of learning were Sir Norman Lockyer's text in an address delivered last week before the British Association for the Advancement of Science. The speaker contrasted the number of richly endowed or State-supported universities of Germany and the United States with the handful of English colleges which languish without either private benefactions or Government aid. So serious seemed the emergency to the speaker that he advocated a Government fund for increasing the "brain power" of the nation, and suggested the immediate appropriation of £24,000,000--the amount of the naval budget for 1889-90--for new buildings and equipment for existing institutions. Such a suggestion would hardly get a hearing in Parliament, but the fact that it is made in all seriousness by a noted scientist reinforces the familiar criticism that in educational matters, and especially in technical instruction, England is many years behind the times. Of course, there is little likelihood that during Capt. Mahan's lifetime England will be willing to strike a budgetary equilibrium between "sea power" and "brain power," but it is possible that the Government may be forced into supporting at least one central school. For we are inclined, with Sir Norman Lockyer, to doubt if the American invasion is complete enough to have inculcated the American habit of lavish giving for educational purchases.

77 (17 September 1903) 229: NOTES

CSP, identification: Haskell, *Index to* The Nation. See also: Burks, *Bibliography*; *List of Articles*; MS L 159.242.

--Is it possible that Sir Norman Lockyer displays an understanding of British human nature, in endeavoring to stimulate it to vast expenses for universities, by pointing out that in this respect that country is far behind Germany and America? If such were the best argument to use, England would be looking into a dismal future. What people conscious of great vitality and genius was ever fired with the idea of following in the wake of others? One would not find much response to such an appeal in Washington, nor in Paris. Would not Sir Norman do better to address his countrymen in some such language as this? "For the last three centuries every single idea of really sovereign preeminence in science has been largely (in most cases undisputedly) of British paternity: the Inductive Philosophy, the Corpuscular Theory, Attraction, the Differential Calculus, the Atomic Theory (and the type theory of chemistry), Natural Selection, the Mechanical Theory of Heat (or that first principle of it which was the solid core of the great doctrine of the Correlation and Conservation of Forces), the Theory of Light as transverse vibrations, followed up by the true Theory of Electricity, the Electro-magnetic Theory of Light, and the Electron Theory of Matter. The new science of radiations, if it has importance enough to be mentioned in this connection, has grown directly and uninterruptedly out of Crookes's experiments. To continue these services to civilization is no more than our plain duty. *Noblesse oblige*. They must be continued; they will be continued. British soil is fertile in men of the highest types of genius. It shall not be found that their fruit is not forthcoming because Great Britain's purse was not long enough to sustain British science against foreign competition." The above are the sober facts; they will bear scrutiny. If they were put before the British Parliament and the country, would not patriotism be moved?

77 (1 October 1903) 263-264: BRITISH AND AMERICAN SCIENCE

CSP, identification: Haskell, *Index to* The Nation. See also: Burks, *Bibliography*; *List of Articles*; MS L 159.248.

A fortnight ago, in speaking of Lockyer's appeal to his countryment for the support of British science, we showed how to apply the old saw that the way to get the best performance from a human being is to encourage him. To-day we will try applying the same maxim in speaking of the future of American science. We wanted to allow the Anglophobists (who never allow the sacred fire to die on their hearth) ample time to dispute our proposition if they could--the proposition, we mean, that for three hundred years not a single conception has taken sovereign preeminence in science that has not been largely--in most cases, even without contest--of British parentage. But our proposition remains undisputed. Its substantial truth seems to be tacitly acknowledged.

Whatever could be said to blunt its point has, no doubt, been indicated in a letter in another column from a rarely accomplished and ingenious scholar signing himself "H. T." Whether this subtle writer is serious, or whether he is only making believe that he opposes us, our readers can guess as well as we, if they note that he does not explicitly deny our proposition, and that he throws in our way two or three very pertinent suggestions in support of it. Certainly, two more striking examples of what are *not* conceptions of sovereign preëminence in science than those he furnishes would be sought in vain--the theory of numbers and the theory of functions. A famous mathematician, being asked why he should be so in love with the theory of numbers, replied that he loved it because it was a pure virgin that never had been, and that never could be, prostituted to any practical application whatsoever. Not only no *practical* application, but (so far as one can look into the future) no *scientific* application, either, is likely ever to be made of one or other of those two theories, outside of pure mathematics itself. In short, they are as narrowly technical as anything can be. They are Leibnitzian monads whose activity, intensely interesting in itself, is imperviously secluded from the business of life and from the main business of science.

Only compare this isolation with the loud resonance awaked in every harp and organ of science by those discoveries which truly have been sovereignly preëminent through science--the inductive philosophy; the corpuscular philosophy with the atomic theory and its progeny; universal attraction; the differential calculus (certainly discovered by Newton, not certainly also by Leibnitz after enjoying Newton's conversation); the theories of elasticity (Boyle and Young), of heat as *vis viva* of molecules (Bacon and others), of electricity (Gilbert, Faraday, etc.), of light as transverse vibrations (Young) and transverse vibrations of an electro-magnetical kind (Maxwell); natural selection (Darwin and Wallace); universal evolution (Spencer). We forgot to mention one of the greatest discoveries of all, made by an humble clergyman of the Church of England, Gay--the discovery that the association of ideas is the autocrat (or, at least, the first of two consuls) that governs all the activities of the human mind, so far as they are subject to any mental law. For the theory of numbers and the theory of functions it can be said that they far surpass chess both in beauty and in their broadly intellectual character; but to call them ideas of sovereign preeminence in science would be to fall into one of those extravagant statements to which mathematicians are only too prone, as when Henry John Stephen Smith--one of the protagonists of the theory of numbers, for all his being an Englishman--spoke of a decline in a people's mathematical activity as if it differed from all other historical developments in having but a single possible cause.

We have to thank "H. T." for another suggestion contained in his remark that, of the pure mathematicians of England's last half-century, the two most prominent were the Hebrew Sylvester and the half (or quarter?) Russian, Cayley. To be sure, there is a simpler and more conclusive proof that the singular relation which Great Britain has sustained to science has not been due solely to Teutonic blood, namely, the density of the entirely uncultivated German (if this species be not quite extinct). The days are very, very distant when it will be possible to disentangle the causes of national character; but as to the matter in hand, there is one cause that strikes any good American observer of intellectual English society. Montesquieu, who possessed an intimate knowledge of so many countries (he was, by the way, a Foreign Member of the Royal Society), put on the title-page of his immortal book this motto:

"Prolem sine matre creatam"

(offspring produced without a mother). A close friend asked him in what sense this had any particular application to the 'Esprit des Lois.' He replied, after some reluctance: "A truly great work must owe its birth not only to a man of genius as its father, but also to a society of intellectual freedom as its mother." Probably in no country is thought of almost all kinds so completely untrammelled as in England. The chief external hindrance, everywhere, to supremely original scientific speculation lies in a certain spirit to which, for want of a better word, we may give the name of *pedantry* (one could hardly call it *obscurantism*). We mean that spirit which caused Poggendorff to refuse for his journal the now far-famed paper of Mayer about the thermodynamics of gases; that spirit which for a whole generation silenced, through the German universities, every contribution to philosophy that was not Hegelian, and which to-day as completely silences there everything that is Hegelian.

And this brings us round to the brief word we proposed to say to the young scientists and philosophers of America. Good and sufficient reasons have in the past acted to conceal the scientific genius of the American people in money-getting and in settling the order of things in this country. But now that those reasons are losing their force, and that you are turning to pure science, above all trust to your own wings. Beware of excessive subservience to the opinions that happen to be in vogue in the German universities. Imitate the Germans in those things that deserve imitation. Emulate them, for example, in that which has contributed not a little to German scientific leadership, their national self-confidence; in their persistency as well. Those two qualities have made that people the world's leader in all *Fach-Forschung* (if we may be allowed to coin the word)--a preëminence all the prouder that it is founded on two moral virtues. You have infinitely more reason to believe in your own scientific powers than they had, one brief century ago. In the nature of things, you will soon outgrow your school-boy deference to your master's *dicta*, and, trusting to your own genius, will surely develop a new and more philosophical type of scientific man.

77 (1 October 1903) 265: THE DECLINE OF MATHEMATICS IN ENGLAND

This is the letter by "H. T." that Peirce mentions in the immediately preceding note.

TO THE EDITOR OF THE NATION:

SIR: The statement made by you in the last issue of the *Nation*, page 229, may be easily answered by quoting such important branches of mathematics as theory of numbers and modern theory of functions, which are almost entirely due to benighted

Continentals. Some time ago, when the English had three eminent mathematicians left yet--a Hebrew, a half-Russian and an Irishman--they used to say that they had generals, but no soldiers, in mathematics. Now the generals are dead and no soldier has risen to the rank of general. What a well-minded Englishman thinks of the present state of mathematics in England may be inferred from Professor Greenhill's review of the German translation of Professor Perry's book, published in *Nature* about a year ago. Without the slightest intention of composing a "Sovvenire di una gran nazione" to the English people, I cannot help quoting the momentous words of Henry John Stephen Smith: "A decline in the mathematical productivity of a nation amounts to a retreat on the whole line."

Yours very respectfully, H. T.

September 17, 1903.

[Our comment will be found on another page.--ED. NATION.]

77 (15 October 1903) 308-309: What is Meaning?

By V. Welby. Macmillan Co. 1903. 8vo, pp. 321.

The Principles of Mathematics.

By Bertrand Russell. Vol. I. Cambridge (Eng.): University Press; New York: Macmillan. 1903. 8vo, pp. 534.

CSP, identification: Haskell, *Index to* The Nation. See also: Burks, *Bibliography*; *List of Articles*; MSS L 159.236, L 159.243, L 159.260, L 463. The Peirce-Welby correspondence (L 463 plus materials from the Welby papers at the University of Toronto) is a very significant exchange in the history of philosophy. To a considerable extent, that set of letters was prompted by this pair of reviews. For an admirable edition of this important set of letters (which includes an extensive editorial introduction), see

Semiotic and Significs, ed. Charles S. Hardwick (Indiana University Press: Bloomington, 1977).

Two really important works on logic are these; or, at any rate, they deserve to become so, if readers will only do their part towards it. Yet it is almost grotesque to name them together, so utterly disparate are their characters. This is not the place to speak of Mr. Russell's book, which can hardly be called literature. That he should continue these most severe and scholastic labors for so long, bespeaks a grit and industry, as well as a high intelligence, for which more than one of his ancestors have been famed. Whoever wishes a convenient introduction to the remarkable researches into the logic of mathematics that have been made during the last sixty years, and that have thrown an entirely new light both upon mathematics and upon logic, will do well to take up this book. But he will not find it easy reading. Indeed, the matter of the second volume will probably consist, at least nine-tenths of it, of rows of symbols.

Lady Victoria Welby's little volume is not what one would understand by a scientific book. It is not a treatise, and is free from the slightest shade of pedantry or pretension. Different people will estimate its value very differently. It is a feminine book, and a too masculine mind might think parts of it painfully weak. We should

recommend the male reader to peruse chapters xxii. to xxv. before he reads the whole consecutively, for they will bear a second reading. The question discussed in these chapters is how primitive men ever came to believe in their absurd superstitions. This has generally been supposed to be the simplest of questions. Lady Victoria does not deign to mention La Fontaine's pretty fable (the sixth of the ninth book; the whole of it is worth rereading if you have forgotten it) of the sculptor and his statue of Jove:

"L'artisan exprima si bien Lecaractere de l'Idole, Qu'on trouva qu'il ne manquoit rien A Jupiter que la parole.

"Même l'on dit que l'ouvrier Eut á peine achevé l'image, Qu'on le vit frémir le premier, Et redouter son propre ouvrage.

"Il étoit enfant en ceci: Les enfants n'ont l'ame occupée Que du continuel souci Qu'on ne fâche point leur poupée.

"Le c[oe]ur suit aisément l'esprit. De cette source est descendue L'erreur Payenne qui se vit Chez tant de peuples répandue.

. . . . .

"Chacun tourne en réalités Autant qu'il peut ses propres songes. L'homme est de glace aux vérités; Il est de feu pour les mensonges."

La Fontaine's theory is somewhat complex, and allows more to the artistic impulse than modern ethnologists have done. They make mythology rather an attempt at a philosophical explanation of phenomena. But the authoress shows by a painstaking analysis that all such theories--La Fontaine's and the new current ones alike--are fatally irreconcilable with those traits of the primitive mind that have struck Tylor, Spencer, and ethnologists generally, as the deepest graven. In place of them she offers a hypothesis of her own, and the reader is tempted to lose patience with her for regarding it only as provisional, so strongly does it recommend itself, until she presents quite another view which one must admit has its plausibility.

The greatest service the book can render is that of bringing home the question which forms its title, a very fundamental question of logic, which has commonly received superficial, formalistic replies. Its vital and far-reaching significance has been even more ignored than usually happens with matters of universal and ubiquitous concern. To direct attention to the subject as one requiring study, both on its theoretical and on its practical side, is the essential purpose of the work. But in doing this the authoress has incidentally made a contribution towards the answer to the question, in pointing out three orders of signification. She has wisely abstained from any attempt at formal definitions of these three modes of significance. She tells us what she means only in the lowest of those three senses. To have gone further would have shunted her off upon a long and needless discussion.

One can see, though she does not remark it, that her three kinds of meaning correspond roughly to Hegel's three stages of thought. Her distinction, too, partly coincides with what was long ago said, that to understand a word or formula may, in the first place, consist in such familiarity with it as will enable one to apply it correctly; or secondly, may consist in an abstract analysis of the conception or understanding of its intellectual relations to other concepts; or, thirdly, may consist in a knowledge of the possible phenomenal and practical upshot of the assertion of the concept. We might point out other interesting affiliations of her thought, sufficient to show that she must be upon the right track.

Lady Victoria, however, does not wish the matter to be agitated in the logician's study alone. She urges that people do not sufficiently take to heart the ethics of language. She thinks that modern conceptions call for a modern imagery of speech. But we fear that she does not realize how deep the knife would have to go into the body of speech to make it really scientific. We should have to form words like those the chemists use--if they can be called words. In particular, she preaches making logic--"significs," she calls it, but it would be logic--the basis or core of education. All those ideals deserve to be pondered. The book is very rich in illustrations drawn from contemporory writing.

77 (22 October 1903) 320: PRACTICAL APPLICATIONS OF THE THEORY OF FUNCTIONS

The editorial reply here is by Peirce in view of the fact that McMahon's comments continue the discussion on British and American Science. See also: Burks, *Bibliography*.

TO THE EDITOR OF THE NATION:

SIR: In your interesting editorial of October 1, you say, concerning the theory of numbers and the theory of functions, that "no *practical* application, and (as far as one can see into the future) no *scientific* application, either, is likely ever to be made of one or other of those two theories, outside of pure mathematics itself." This statement would probably have passed unchallenged a few years ago, perhaps even as recently as the time when the lamented mathematician to whom you allude was devoting himself to that pure and secluded virgin, the theory of numbers; but the mathematicians and physicists have been coming together within the last decade; they are comparing their standing problems, and are learning how to be mutually helpful and stimulating. It is realized that certain physical problems are at a standstill for want of appropriate modes of mathematical

expression, and that there is need for the invention or discovery of new forms of functional relationship. Increased attention is accordingly being directed to the wide field of the theory of functions of a complex variable. The complex plane is one of the meeting-grounds of mathematicians and physicists, and the latter are now quite at home in the presence of that coy handmaiden, the complex variable; indeed, the well-known transformation scene in which she and her image play such a prominent part, is now an important feature in the solution of some practical problems.

The discovery of a new form of function to correspond to a new physical relation is at once a gain to natural science and to mathematics, and it widens the intellectual horizon. A single concrete example of the way in which the mutual stimulus operates may be of interest.

In 1891 Dr. Pockels of Heidelberg, in his treatise on the partial differential equations of mathematical physics, made the following suggestive remarks:

"Both from a mathematical and physical standpoint, multiform functions are important, and it is very desirable that the properties of such functions, their winding-points and singularities, their behavior on Riemann surfaces, etc., should be systematically investigated--in short, all the function-theory questions which were handled in the theory of the Newtonian and logarithmic potential.... In this direction of inquiry without doubt a wide and rich field offers itself."

About six years later, Professor Sommerfeld of Aachen, and his pupil, Dr. Carslaw of Glasgow, contributed papers to the London Mathematical Society on the multiform functions that satisfy certain differential equations subject to various boundary conditions; and they used these functions to solve some standing physical problems. One of these was the following problem in diffraction: "Plane waves of sound, light, or electricity are incident on a thin infinite half-plane bounded by a straight edge: to find the resulting diffraction of the waves." This problem had been mentioned in Lord Rayleigh's article on 'Wave Theory' in the 'Encyclopædia Britannica,' in the following terms:

"The full solution of problems concerning the mode of action of a screen is scarcely to be expected. Even in the simple case of sound, where we know what we have to deal with, the mathematical difficulties are formidable, and we are not able to solve such an apparently elementary question as the transmission of sound past a rigid infinite thin plane screen bounded by a straight edge or perforated by a circular aperture."

The appropriate solution was obtained by the conception of a twofold Riemann space having the ordinary physical space as one of its folds, the half-plane as a branch membrane, and the straight edge as a winding line. The characteristic multiform function that expresses the wave motion was derived by the most beautiful use of pure function-theory.

I doubt if either Cayley or Sylvester would regret to see the sway of the virgin queen thus extended over new dominions. If she no longer has the seclusion of a "Leibnitzian monad," she exemplifies in a higher sense the Leibnitzian doctrine

of preëstablished harmony--the harmony of the world of nature with the world of the intellect. What God hath joined together, let not man put asunder.

Very truly yours, JAMES McMAHON.

ITHACA, N.Y., October 10, 1903.

[An apposite instance! Had we known of it, we should have softened our remark. Nor, in making it, did we forget that several applications have been made of propositions worked out by Cauchy and earlier mathematicians before the "theory of functions" was christened, and for which (though they are now incorporated in it) it can take no more credit than the theory of numbers could to the carpenter's rule of three-four-five, which was known to Pythagoras, but the principle of which may to-day figure in a treatise on that theory. But now, for the first time, we meet with one genuine case of an application of the theory of functions, upon which we may ground some hope for further such triumphs. It is a convincing and striking proof that a line of thought which seems to relate exclusively to impossible states of things may, if resolutely pursued, eventually bring great light upon familiar experiences. Let us give this instance a permanent place in our memory alongside of the fact that Pascal, after his wonderful discovery about conic sections, abandoned that study as an idle pastime having no application to any matter of importance.

All this, however, does not in the least touch the point that our remark was designed to make; for at the time when it could be said that the British were neglecting the theory of functions (which is no longer true), there was no glimmer of reasonable hope that it could ever be of any use. As for the theory of numbers, the first application of it has, we believe, yet to be made, unless, perhaps, Cayley used it for his theory of chemical "trees." But that concerns the *partition* of numbers, a separate branch of mathematics which the English have perfected, we believe.

We are heartily of opinion--but it is no longer a matter of opinion--that the younger generation of physicists are going to reap a rich harvest from their studies of the higher mathematics. In this they are only following a time-honored custom, for almost all the great physicists, from Galileo down, have been strong mathematicians. At the same time, there are instances enough--like the beautiful researches of Le Bon on phosphorescence and peculiar radiations--to show that, even in these days, the consciousness of a decidedly deficient capacity for mathematics need discourage no young man, nor young woman, from devoting himself or herself to physical investigations.--ED. NATION.]

77 (5 November 1903) 360: FRANCIS ELLINGWOOD ABBOT

TO THE EDITOR OF THE NATION:

SIR: Dr. Francis Ellingwood Abbot was found, in Cambridge, on October

23, lying face downward on the grave of his wife, who had died just ten years ago, he having lately completed the book on philosophy to which he had devoted his whole

life. One of the benefits of an acquaintance with Dr. Abbot was that it gave a new conception of the saying, "The pure in heart shall see God." The unsophisticated purity of his love of and apprehension of truth, oblivious of the tide of opinion, was a quality without which the Introduction to his 'Scientific Theism,' wherein he put his finger unerringly (as the present writer thinks) upon the one great blunder of all modern philosophy, could not have been written. The perfect clearness and simplicity of his argument will blind many a mind to it that could thread its way through the most abstruse tortuosities of law. But Dr. Abbot was like that "best philosopher" of whom Wordsworth speaks--

"On whom those truths do rest

Which we are toiling all our lives to find."

He had that spiritual insight into philosophy that Wordsworth attributes to the child. In each writing of a philosophical nature that he produced, he brought out some undeniable and important point that had been almost entirely overlooked by philosophers; so that philosophy is sure to be materially advanced by the great work which he left finished, but which is not yet published.

Dr. Abbot was an intensely self-conscious man, with a perfectly accurate appreciation of himself, and with a noble variety of self-consciousness that bent to no compromise with himself or with others, for all his gentle and loving nature. The world does not like men so extremely consistent as he was, but it seems impossible at this day to review any long-past episode of Dr. Abbot's life without seeing that his course was the right one, the only right one. He was fond of writing verses. Here are two stanzas of seven in his 'Scientific Theism':

"Art Thou the Truth? To Thee, then, loved and craved and sought of yore, I consecrate my manhood o'er and o'er, As erst my youth.

"Art Thou the Strong? To Thee, then, though the air be thick with night, I trust the seeming-unprotected Right, And leave the wrong."

C. S. P.

October 27, 1903.

1904

78 (11 February 1904) 110: NOTES

CSP, identification: MS L 159.240. See also: Fisch, *First Supplement*.

Four books on electricity are before us. The first of these, which is intended for the general reader, is incomplete, although already filling three volumes, being the first part and the first division (or a portion of it) of the second part of R. Millineux Walmsley's 'Modern Practical Electricity.' It is an English book, referring to instruments used in England, and is printed there, though issued by W. T. Keener & Co. of Chicago. It carries no date; nor is there any intimation that it is a second edition of a ten-year-old book. Nor is there any statement as to the number of volumes still to come. On a hasty examination, one might suppose it to be complete. It really has a good deal of merit as being such an account as any intelligent person can understand of those effects of electricity which may come into the experience of non-electricians. We cannot recommend it until we know when it is to be completed. The 'Elementary Treatise on Electricity and Magnetism,' of G. Carey Foster and Alfred W. Porter, is also a second edition; but as it is published by Longmans, Green & Co., we may be sure there is no occasion to cry *Caveat emptor*. It has been extensively revised, yet is hardly so good as an entirely new book might have been. It is a text-book that avoids the calculus.

'Elements of Electromagnetic Theory,' by S. J. Barnett (Macmillan), is a profound and meritorious mathematical work. The author has "tried to present in systematic and definite form a simple, rigorous, and thoroughly modern introduction to the fundamental principles of the subject." He does not seem to us to have succeeded in making the subject quite as perspicuous as he might have made it, nor everywhere to be clearly rigorous; yet he has produced a valuable work. Mr. G. D. Aspinall Parr's 'Electrical Engineering Measuring Instruments' (Van Nostrand Co.) is a good technical book concerning instruments generally in use in England.

78 (11 February 1904) 113-115: FAHIE'S GALILEO

Galileo: His Life and Works.

By J. J. Fahie. With portraits and illustrations. James Pott & Co. 8vo, pp. 451.

CSP, identification: Haskell, *Index to* The Nation. See also: Burks, *Bibliography*; *List of Articles*; MSS L 159.247, L 159.250.

To one whose chief enjoyment of books is in reading them, who hates an *édition de luxe*, or any beauty of type or paper that may adorn a parlor table but incommodes a reader, and for whom, in the case of illustrations in which the artistic element is not the predominant consideration, nothing every way more satisfactory has been invented than good process reproductions of photographs, the dress of this

volume will seem worthy of its subject, with its tasteful cover, light weight, paper really better than most of the old linen paper, legible type, graceful and free from every kind of affectation, and with black ink. It is from the Edinburgh Press. In those respects in which Mr. Fahie's 'History of Wireless Telegraphy' made us fear this book might be weak, it turns out to be particularly strong; while in some of those respects in which our expectations ran high, we find ourselves a little disappointed. We are not disappointed, however, in finding very scrupulous care and intelligence in getting the events of Galileo's life accurately recorded in every detail. It is safe to say that the history of the persecution here at last takes substantially its definitive shape. For the first time we feel confident of really understanding that history.

The author acknowledges great aid from Professor Favaro, whose stupendous labors in Galile-ology are so famous; and it would seem, from the notes, that Favaro may perhaps have read and minutely annotated Mr. Fahie's MS. There is certainly no one volume of Favaro's own that gives the unitary conception of Galileo's life that this does. It is necessarily more or less a compilation from Favaro; but there are places where the author successfully maintains somewhat different views. It is on the account of the persecution that he has chiefly laid out his resources, and shows really high literary qualities. He is eminently sane, not flying into a passion with the dead, but painting the crime calmly in its true colors, unglozed by any superficial excuses. He looks upon Galileo, not as if his duty consisted in making him a fine subject for a melodrama, but as what might be called, in theatrical parlance, a "practicable" man, like a door or window that will really open and shut. These are what we call the qualities of literary skill in the book. The reviewer took it up late in the evening, and laid it down at half-past six in the morning.

Unfortunately, so much pains has been expended by the author on this great event and on getting all the little facts of the biography right, that not quite so much energy seems to have been left for accuracy in other directions. One cannot say it is a heinous offence to call Simplicius (the Greek commentator, we mean, not Galileo's personage) a Sicilian; but still it puts a bad mark against the account of accuracy. In a sketch of the history of the Magnet down to and including Gilbert, no mention is made of Petrus Peregrinus, from whom Gilbert stole his best experiments. These are the merest trifles; yet even smaller faults are not altogether without importance when they relate to the subjects of Galileo's discoveries. In noticing the Italian's invention of the telescope, late in August, 1609, near eleven months after Lipperley in Holland received a patent for a similar invention, it might have been worth mention that on February 14, 1610, a letter, of which the original happens to be extant, was written to remind Thomas Harriotts of a former promise to send one of the "perspective cylinders" he was then making to an acquaintance. Galileo's *bilancetta*, which was simply an account of a balance for weighing an object under water, in order to find its specific gravity, was published in 1644. It is here said (no doubt with good reason) that it was invented in 1586, but it would have been well to explain just how that date has been ascertained, and to state that in that very year Simon Steven published the very same invention in Leyden. Galileo was in all probablity [[sic]] right in surmising that the method had been known to Archimedes. We are told that Galileo discovered the principle of virtual velocities. No doubt; just as there were three kings of Chickeraboo. But a good half of the virtue of that principle consists, not in perceiving it in a few simple cases, but in generalizing it into the universal key to dynamics. To say that Galileo knew the parallelogram of forces is not in perfect strictness true; but if that is to be said, it becomes downright misleading to say that "he failed to grasp the fact that that acceleration which in the case of motion under gravity he so closely examined, might be made a means of measuring the magnitude of the force producing the motion." Galileo had perfectly clear conceptions of velocity and acceleration, and of their parallelograms. He had, for example, a perfectly clear conception of what is called "*g*," and never would have been guilty of trying to express it in pounds' weight. He had a tolerable notion of pressure, and, of course, some idea of a force applied to a part of the surface of a solid, etc. But he seems to have had no idea at all of a moving or effective force. He never spoke of gravity as a force; and if he had ever heard anybody so speak, he would probably, at first, have accused him of talking metaphysics. Consequently, his notion of the parallelogram of forces must have been obscure. The truth is, that Mr. Fahie, though he makes a praiseworthy effort to do so, never properly appreciates Galileo's wonderful command of dynamical conceptions, wherein lay his chief greatness, and which formed the warp of his intellectual life. The inevitable consequence is that his portraiture is, with all its merits, hardly more than mediocre. It is true that he quotes what Lagrange says (calling him an Italian, as he might so call Napoleon); but if he had fully understood what it amounted to, he would have erased the ninth and tenth words of his book: "Galileo Galilei, one of the earliest and perhaps *one of* the greatest of experimental philosophers of the modern world," etc.

The principal facts now established as to Galileo's dealings with the Inquisition are these: From 1612 and earlier the storm had been gathering. The Jesuits and the Aristotelians were bent upon crushing him, because he put them into the ridiculous position of refusing to see what was before their eyes. But all the cultivated people, the Grand Duke, Cardinal Maffeo Barberini (afterward Pope Urban VIII.), Cardinal Federigo Borromeo, and the like warmly supported him. On the 5th of February, 1615, an unsigned denunciation of him was lodged with the Inquisition. Early in December he voluntarily set out for Rome. On the 19th of February the Qualifiers for the Holy Office were called upon for an opinion substantially as to the Copernican proposition. On the 24th they reported it to be heretical. On the 25th, Cardinal Bellarmine was directed "to summon before him the said Galileo and admonish him to abandon the said opinion; and in case of refusal the Commissary [*i.e.*, the most Rev. Michelangelo Seghizzi] is to intimate to him, before a notary and witnesses, a command altogether to abstain from teaching or defending the said opinion, and even from discussing it; and if he do not acquiesce therein, he is to be imprisoned." Note that if he consented to "abandon" the said opinion there was to be no such intimation; and if there was an intimation, it must be executed before a notary and witnesses to be of effect. But Galileo at once agreed to abandon the theory. On March 3 a decree of the Inquisition was

published, of which the preamble states that "Galileo Galilei, mathematician, had, in terms of the order of the Holy Congregation, been admonished to abandon the opinion he has hitherto held, and had acquiesced therein." On May 26th Cardinal Bellarmine delivered to Galileo a formal written statement to the effect that "Signor Galileo Galilei has not abjured [but only promised to *abandon*] . . . any opinion or doctrine held by him, . . . but only the declaration made by the Holy Father . . . has been communicated to him . . . that the opinion attributed to Copernicus . . . is contrary to the Holy Scriptures, and therefore cannot be defended or held." This obliged Galileo to abandon his researches and writings; and his health suffered seriously, although he was allowed to use the proposition as a "hypothesis."

Events led to his writing 'Il Saggiatore,' and in October, 1622, it was sent to a member of the Accademia dei Lincei, and was passed from hand to hand, and various corrections were suggested and accepted with a view to rendering it acceptable to the Inquisition. In February, 1623, the Papal imprimatur was attached to it, and later in the year it was published. Meantime, on August 8, Galileo's adherent and admirer, Cardinal Maffeo Barberini, had been elected Pope, taking the name of Urban VIII. In April, 1624, Galileo went to Rome and had six long interviews with this new Pope, endeavoring to have the prohibition of 1616 removed, but to no purpose. The Pope wrote officially to the Grand Duke and expressed his sense of what the world owed to Galileo's discoveries, as well as his great affection for him. Galileo returned to Florence, where he drew up a reply to an attack on the Copernican system. This was handed about in manuscript. The Pope, having been shown some passages, expressed his high commendation, and on another occasion remarked to a cardinal that the Copernican system had never been condemned as heretical, but only as rash. Such things convinced Galileo that he might find means to express himself; and in 1626, he began to write his famous 'Dialogo intorno ai due Massimi Sistemi del Mondo, Tolemaico e Copernicano,' which fully occupied him until May, 1630, when he repaired to Rome in order to find under what conditions it could be published. He had audience with the Pope, who assented to the publication under three conditions: first, the title must not be misleading (Galileo had proposed to call it 'Dialogues on the Tides'); secondly, the subject must be treated from a purely hypothetical standpoint; and thirdly, it must be wound up by the argument that since God is all-powerful, no facts can be a *necessary* proof of any independent facts. Certainly, no logician to-day can withhold his assent to that; and certain it is that this argument was absolutely vital to a good Catholic then, and so remains to this day.

There was nothing for Galileo to do but to make the required alterations of the title, the introduction, and the conclusion. The last was a most difficult task, for he held the Pope's opinion in supreme contempt, and yet it would not do so to treat him. He ought to have seen that this situation forced upon him, what good controversial rhetoric required, the genuine putting of himself in the Pope's attitude of mind, so as really to feel what the reasons were which weighed with the Pope. Otherwise, in attempting to restate that argument, he would give a travesty of it. If he found it impossible to conform to that condition, then the one course open to him

was to betake himself to Venice, which republic was ready to receive him with open arms, and to protect him at the risk of an interdict, if necessary.

In the Dialogue, all the arguments (including the Pope's) against Copernicus are put into the mouth of a character called "Simplicius." There was a certain appropriateness in this, since the best arguments of the Aristotelians were drawn from the commentary on the 'De Cœlo' by the real Simplicius. Yet it was bad rhetoric. The manuscript was submitted to the papal censor, who, after causing it to be thoroughly revised by his assistant, all passages being altered that were at all objectionable (as Galileo had desired), finally very carefully examined it himself, and attached his imprimatur for its publication in Rome, with the understanding that an introductory and a concluding passage should be inserted by Galileo, such as the Pope had required. The affair having come to this stage late in June, Galileo returned to Florence.

The prevalence of the plague and a particular death hindered the publication in Rome, and in August Galileo decided to have the book printed in Florence. On communicating with the Roman censor, that dignitary said he must first see the complete book. But the plague had rendered the mails so uncertain that Galileo proposed to send instead only the new preface and conclusion, suggesting that some person in Florence be deputized to reexamine the body of the work. This proposition was acceded to. The counsellor of the Inquisition in Florence went through the body of it with the minutest attention, and declared there was nothing in it that could give the slightest umbrage to anybody. The Roman censor neglected to attend to the introductory and concluding portions sent to him, until July 1631, when the Pope personally ordered him to approve those parts at once, with permission to alter the wording in any way Galileo might desire, the substance remaining as it was. After another complete reëxamination in Florence, the final imprimatur was attached, and the book was published toward the end of February, 1632.

During the summer the Jesuits laid their plans deep to bring Galileo to ruin, and, in all that followed, the Pope (having, no doubt, come to believe that Galileo's statement of his argument was ironical and satirical) manifested an intense personal vindictiveness, which was never relaxed as long as Galileo lived, and whose effects were only a little mollified under such circumstances that not even his passion and the inherent littleness of his soul could hide from him the general contempt that he was in danger of bringing upon himself. In August, 1632, further sale of the Dialogue was forbidden, and a papal commission was appointed to examine the book. This commission reported during the next month, and in this report for the first time appears the statement that, in 1616, Galileo had been enjoined from ever holding, teaching, or defending the Copernican doctrine. It appears that the commission, upon looking at the records of the proceedings of 1616, came upon an unsigned *compte rendu* or protocol in the handwriting of the commissary-general of the Holy Office, who then acted as secretary, which failed to say that Galileo had submitted to the admonition of Cardinal Bellarmine; and which stated that the next step had been taken, namely, that said commissary had, before witnesses, enjoined Galileo, under pain of further proceedings, never to hold, teach, or defend the opinion. But nothing is said about a notary, nor are any other witnesses but Cardinal Bellarmine named. He had formally denied that any such thing took place, and was now dead. It has been supposed that this minute was a forgery. Favaro says that it cannot be so; but not being signed, it was without legal value. Nor does the minute, such as it is, represent the injunction to have been executed according to the form prescribed by the Holy Congregation; so that that injunction, if it had been so delivered, would be extra-legal. This document came as a complete surprise to all parties, and Galileo believed it to be a forgery. Pope Urban VIII. himself, who had taken part in the proceedings of 1616, had repeatedly discussed with Galileo the doctrine which that minute represented Galileo to have been forbidden to discuss. There is no doubt, therefore, that the proceedings of 1632 and 1633, basing themselves, as they did, on that minute, were contrary to the law of the Church.

On October 1, Galileo was summoned to appear during that month before the commissary-general of the Inquisition, in Rome. There were various delays, and he did not arrive at the Tuscan embassy in Rome until the 13th of February, where he begged leave to remain; and this was granted for the time being, under restrictions. On April 12 he appeared before the Inquisition. From that day until the end of the month he was held prisoner in the walls of the Inquisition, but was allowed his servant, and was well treated. Owing to the intense malignity displayed by the Pope, Galileo's friends, in fear for his life, advised him simply to admit everything, and to submit to everything. This he did at his second examination of April 28, and on his third appearance of May 10. On June 16 the Pope held a meeting of the Congregation, at which it was decided to make Galileo confess his evil intention under threat of torture, and if that failed, to proceed further--that is, to burn him alive; for that is what it undoubtedly would have come to. Catholics may take such comfort as they can that this did not happen. On June 21 Galileo appeared once more, and, being threatened with torture, replied, "I am here to obey," and acquiesced in everything. He was then imprisoned. On June 22 he was made publicly to confess, and recant upon his knees. On the 6th of July he was allowed to retire to Siena, to the house of Archbishop Piccolomini, where he arrived on the 9th. He remained, however, wherever he was, a prisoner of the Inquisition all the rest of his life.

78 (3 March 1904) 171: NOTES

Attributed to Peirce by Fisch in *Second Supplement* (a galley proof of this note, and of the following note, are in the Peirce papers). This note is unassigned in Haskell's *Index to* The Nation, vol. 1.

Mr. Harold Hilton's 'Mathematical Crystallography and the Theory of Groups of Movements' (Oxford: Clarendon Press; New York: H. Frowde)--"movement," by the way, is an ill-chosen word, since there is no continuity in the transformations considered, and reflexion in a plane is one of them--is a book supplementary to the treatises on crystallography. It is divided into two parts. Part I. is mainly occupied with proving mathematically that the thirty-two classes of crystals with which the reader will be familiar are, upon certain hypotheses sufficiently probable, the only classes possible. What are called "forms" of crystals are not, in this part, seriously considered. On the other hand Part II. may be regarded as an attempt to explain, by further hypotheses, the geometrical nature of crystal-forms, and to show that they are 230 in number. It is an account of recent strictly geometrical studies of possible regular displacements of regular arrangements of molecules in space. No dynamical considerations are introduced; but there is little room for doubt that the results here developed must be the foundation for any future dynamical theory of crystals. The whole is presented with the utmost clearness, and, being very easy to read for a mathematical book, must be studied by every student of crystallography who can make any pretension to thoroughness.

78 (3 March 1904) 171: NOTES

CSP, identification: see immediately preceding note. This piece is unassigned in Haskell's *Index to* The Nation, vol. 1.

Mr. John Edward Campbell's 'Introductory Treatise on Lie's Theory of Finite Continuous Transformation Groups' (H. Frowde) is the first book presenting in our language an account of perhaps the most important mathematical theory that has had birth in our time; Burnside's 'Theory of Groups' covering the subject only in a general way. Although the omission by Mr. Campbell of the important matter of possible types of group-structure is to be regretted, the student will find this volume by far the best introduction to its subject in any language; for the presentation is fairly abreast of the day, and the theory has been developed in sufficient detail with very considerable ability. There is a convenient index.

78 (10 March 1904) 191-192: NOTES

Probably by Peirce. Mention is made here of A. D. Risteen, one of Peirce's friends, and of the Office of Weights and Measures, which Peirce supervised for a time while he was in government service.

'The Mechanical Engineer's Reference Book,' by Henry Harrison Suplee (J. B. Lippincott Co.), is a new book intended for American use. It measures 7 x 4 1/4 x 1 1/4 inches, and contains 837 well-printed pages. Its contents have been made up with excellent judgment, and we should incline to believe it the most practically useful of its species. But a new compendium, unless extraordinary pains have been taken with it, and especially with the proofreading, is liable to vex its user with occasional errors. Here, for example, on page 67, one hundred U.S. gallons are said to equal 478.53 litres. Risteen's 'Metric System' (Hartford Steam Boiler Inspection and Insurance Co.), which has the best tables, makes the figure to be 378.679, which is substantially correct. This number has been, as it should have been, deduced from the rule of procedure of the Office of Weights and Measures. The definition of

the gallon as 231 cubic inches merely shows what the gallon was intended to be. Mr. Suplee's decimal supposes it to be exact, which must not be assumed.

78 (17 March 1904) 211: NOTES

CSP, identification: MS 1477 (draft). See also: Burks, *Bibliography*.

Frederick James Eugene Woodbridge (1867-1940), though a Canadian by birth, achieved fame in the United States as the co-founder of the *Journal of Philosophy*, along with Catell. He was educated at Amherst College, graduating in 1889. Woodbridge also had a distinguished teaching career, which included posts at the University of Minnesota and Columbia University.

Prof. Frederick J. E. Woodbridge's 'The Philosophy of Hobbes in Extracts and Notes collected from his Writings' (Minneapolis: The H. W. Wilson Co.) is, we presume, well adapted to the use of hearers of a particular course of lectures, though hardly sufficient by itself, perhaps, for an understanding of Hobbes. It contains some 370 pages from the 'Leviathan' and from the 'Elements of Philosophy concerning Body' (the *De Corpore*). As a rule, we have not been particularly favorable to such volumes of extracts from philosophical writings that are easily obtained in their entirety, and which would seem to be worth reading in full by anybody interested in philosophy. If he has not the time to do so this season, let him have the book within reach, and some day he will read it. About Spinoza a considerable class of persons have a burning curiosity that can quickly be appeased, so that a book of Extracts from him is all very well. But for such smooth reading as Locke, Reid, and especially Hobbes, with his strong style and interesting cast of thought, we really do think it a pity to encourage fragmentary reading. It is not as if there were not editions of his works to be had at moderate prices--beautiful editions, too, while the presswork of this volume is no better than indifferent. The 'Leviathan' may be had, well-printed *in extenso*, for less than the price of these extracts. On the other hand, the extracts from other writings of Hobbes here printed as notes do add decidedly to the value of this edition; and the portrait is well reproduced.

78 (17 March 1904) 215-216: THE METRIC FALLACY

The Metric Fallacy,

by Frederick A. Halsey;

and **The Metric Failure in the Textile Industry**,

by Samuel S. Dale. D. Van Nostrand Co. 8vo, pp. 231.

CSP, identification: Haskell, *Index to* The Nation. See also: Burks, Bibliography; List of Articles; MSS L 159.255-257, L 159.259, L 159.261; MSS 1463, 1478 (drafts).

Frederick Arthur Halsey (1856-1935) played a major role in United States governmental decision making regarding weights and measures. Having been trained as a mechanical engineer at Cornell University, Halsey practiced his trade for several years, becoming quite well known for his outspoken opposition of the metric system of measurements. In 1902, Halsey

read an anti-metric paper before the American Society of Mechanical Engineers, which led to the taking of a referendum ballot among the membership on the issue of adoption of the metric system. The result was an overwhelming condemnation of the system. It is even said that this event led to the defeat of a bill in Congress providing for the adoption of the metric system, after having been favorably reported by the committee on coinage, weights, and measures. Halsey was the first recipient of the gold medal of the American Society of Mechanical Engineers in 1923 in recognition of his contribution to industrial economics.

The adoption of the metric system would have real advantages. It would also have real disadvantages. Are the former so assured and so great as unmistakably to overbalance the latter?

The serious advantages of the metric system, itself, are two. The first arises from its decimal subdivisions, and this, so far as it goes, is a sound argument for the full adoption of it. Yet we are apt to overrate it, because we think of the conveniences of our decimal money, without considering how much more frequently sums of money have to be added by ordinary people than weights and measures have to be arithmetically operated upon. The second advantage of the metric system lies in that system's furnishing a universal commercial language for weights and measures. We believe this to be the greater advantage of the two; but it affords no argument for any modification of our laws, since everybody is already free to express himself in metric units. The law of 1866 (due to Messrs. Kasson and Julius E. Hilgard) protects him in doing so, and he is sure to be understood. Other advantages are claimed for the system, but any impartial person will easily satisfy himself that they amount to little. Thus, is is averred that the simple and elegant relations between the different kinds of units of the metric system would be important. No doubt they would for a small number of scientific men, not a hundred thousand all told. But these all use the system already. They seem to be very desirous that the unlearned, too, should be able with equal readiness to interconvert mass, length, and capacity; but really this cannot be a vital advantage even for the scientific class; and when it is divided by the ratio of the whole population to the number of this class, it evidently becomes insignificant. For the great body of the people, the relations between the old units of mass, length, and capacity are about as handy. They are not generally very well known, for the reason that they are not generally very important.

It is further pretended that the adoption of the metric system would lighten the tasks of schoolchildren so much as to afford an argument of national importance for its adoption. The idea presumably is that they would no longer have to learn the tables of weights and measures. But, as matters now are in this country, children need to be required to learn but a small part of those tables; and that little they would still need to know just the same. For, not to dwell on the fact that the books that speak of the old measures, as well as the things made according to them, would still endure, Mr. Halsey conclusively shows in the volume before us that the old units would persist, and with them the need of knowing them. Finally, it is said that another advantage of the change would be that manufactured articles would be made of the same sizes the world over; that such things as bolts, screw-threads, machinery, textile fabrics, etc., would have such uniformity that our manufacturers could begin to compete in the markets of Continental Europe.

Surprising as it may be, there actually are intelligent and educated men so utterly ignorant of the condition of their own country's business and of its relations with other countries, and who are so ignorant of their own ignorance, that they are not ashamed to talk more or less like this before the faces of those who are acquainted with such matters. Much more worthy of attention is the counterclaim that our abandonment in our manufactures of a unit of the international importance of the English inch would bring disaster upon the country. This is, indeed, the greater of the two main arguments against Congress proceeding any further than it has already done in the direction of favoring the metric system. The other argument is drawn from the intrinsic inconvenience of a decimal system, especially when employed, not merely as a part of lnaguage [[sic]], a mode of expressing quantities, but as regulating doings and makings.

That the number ten is an unfortunate base of numeration is generally agreed by those who have inquired into it. Indeed, it is so decidedly so that several men otherwise remarkably intelligent have actually proposed that it should be given up, and that a power of two (they mostly prefer sixteen) should be used in place of it. This is sheer insanity. Yet it is worth notice as showing that our retention of ten is pure conservatism, and as consequently affording a warning against the follies to which unmeasured rationalism in regard to weights and measures, like all unquantitative discussion about quantities, may lead. The effect of the inconvenience of this base of numeration has been that while it has mainly governed our expression of quantities, yet our practical dealings with these have broken away from our system of expression and have been regulated by other factors of subdivision. There are two principal cases. In the first place, when our subdivisions are used additively, as, for example, in weighing anything by placing weights in one pan of a balance, everybody knows the great convenience of successive bisections. We virtually put to the balance a series of questions answerable by yes or no; and the most expeditious way of reaching the desired result is by so putting the questions as always to bisect the possibilities. In this case, subdivision by ten causes a loss of time and energy amounting to 2.35 per cent. The other principal case is where it is desired to have within certain limits so many sizes of some article, say screwbolts, that the ratio of each size to the size below it shall never exceed a certain value. If there be an unnecessary variety of sizes, not only will more capital be locked up in keeping them on hand, which aggregates something, but, what may rise to any degree of importance, in an emergency the particular size needed may not be found in the store.

Take a typical case of continual happenings. A refrigerator car of strawberries from California broke down in the desert for want of a bolt. But that bolt was of the Sellers system of sizes, which prevails the world over, except in England, and even there is much used. Here, there is no other system, and therefore the size wanted was sure to be procured in the nearest store, and was found, and the car went on after half a day's delay. Suppose, however, that the metric system had been in use, with the Armengaud system of bolts, now struggling for existence in France against the Sellers and Whitworth systems. That system has forty-one different sizes for the interval covered by thirty-one sizes in the Sellers system. The chance of replacing the bolt in time to save the precious cargo would have been much less. Besides these two principal inconveniences of decimal subdivisions, there are others of an obscure nature whose effects are certain to be felt by every mechanic.

Of the main disadvantage which the change would involve, we must now endeavor to give our readers some slight idea. Although manufactures engage but a quarter of our gainfully occupied population, and do not therefore constitute our highest material interest, yet their annual products amount probably, by this time, to full equality with those of Great Britain, Germany, and France, put together, even after subtracting from the nominal value of ours 25 per cent. for the illusory magnification produced by our tariff. Our population, it will be remembered, is only half of theirs. We owe this proud position to several causes, of which the first --to generalize as much as possible--may be said to be American systematization. The most prominent effect of this has been the supremacy of American machinery, which is absolute. The greatest factor in the attainment of this supremacy, greater perhaps than the American genius for simplification or than American ingenuity, was, we believe, the thorough systematization of American machinery early brought about by the labors of Eli Perkins, by which the dimensions of the different parts were brought into simple relations to the inch, so that those parts could readily be replaced--a feature only tardily copied by Europe. The result has been to impart to the English inch an international character embodied in real manufactured things and in the tools with which to make those things. This internationality does not appear in ordinary expressions of quantity, but has a real existence, and already rivals the real internationality of the centimetre. Ultimately, if we sustain the inch, one or other of two results must be reached: either the real internationality of the inch must gain a clear ascendency, or else the metric countries must be forever stunted in the growth of their manufactures.

Now we are assured by those who are the best qualified to judge of the matter that, were any law to prevent our manufacturers from making things to inch measure, or which should prevent obligatory contracts to do so, the position of our machinery abroad would be irrecoverably surrendered--to England, if she had not already taken a like step; but in any case, would be gone. The understanding under which our machines had been sold, that parts should be replaced, would be dishonored, and we disgraced. Our own country would be flooded with English machinery, unless it were kept out by heavy duties the effect of which would be to bring all our manufactures to the verge of ruin, or further. A large proportion of all the machinery now in the country would have to be thrown away. As for the cost of the change, something may be guessed of it from the fact that the cost of the designs and tools to manufacture a single machine will sometimes amount to nearly a hundred thousand dollars. All this is the best testimony we have.

We know ourselves that the metric system has never been effectually introduced into any country without the aid of penal provisions of a severity entirely unknown to our people; and there has been no people among which they have been more necessary than they would be among ours. Were such measures enacted

here, in a few years they would be repealed amid a storm of popular indignation, almost doubling the havoc and confusion, and bringing well-deserved ignominy upon the heads of the doctrinaires whose conceit and pretension had drawn such disaster upon the country; and our universities, where their leaders are, would suffer in consequence.

How are we to estimate the probable amount of this disadvantage in dollars? The only sane way of doing so will be to be guided mainly by a critical consideration of the testimony of those of our great manufacturers who are in the habit of using measurements with real precision, say with errors not greatly exceeding one hundred-thousandth part of the quantity measured. These are the people who make our machinery, etc. For it is certain that their great capital of seven hundred millions cannot be seriously impaired without the injury being felt, first, by all other manufacturers and soon by every unit of our population. It is no mere theoretical question upon which physicists or other scientists are, as such, competent to lay down the law, but is a practical question of tremendous moment, which practical men best comprehend.

The bill at present before the committee of the House contains two provisions. One of these is that "on and after January 1, 1907, the weights and measures of the metric system shall be the legal standard of weights and measures of and in the United States." We doubt much whether this provision, in the absence of the penal clause usually attached to it in introducing the metric system, amounts to a row of pins. As matters now are, if a court had before it a contract in which a Rhine-inch was mentioned and was defined as 1.03 United States inches, would not that court hold the contract valid? If so, after that enactment, why should not a contract which mentioned an English inch, defining it as 2.54 centimetres, be equally enforced? The contract would express itself in terms of the standards that would be legal "of and in the United States." The other provision of the bill is, that immediately "all the Departments of the Government of the United States, in the transaction of all business requiring the use of weight and measurement, except in completing the survey of the public lands, shall employ and use only the weights and measures of the metric system." In examining all the printed matter put forth by the committee, we have been struck by its avoidance of any explanation of the phrase "employ and use." Does it mean use in practice or use in expression? If the former, and the bill becomes a law, no chart that expresses depths of water in fathoms can legally be used; if one of our ships should lose a bolt, it can never be supplied, but the vessel must be condemned; the vast collection of designs in the archives of the Navy Department can never be consulted, with a thousand other consequences equally ridiculous. Common sense protests that that cannot be the meaning. Then, the meaning of the phrase "employ and use" must be to use in expression. When an inch is meant, it must be called 2.54 centimetres. That would be an innocent provision. But the ambiguity was deliberately intended by those who have been behind the bill. They expect in this way to divide the struggle into two parts, getting the bill passed by quietly representing to those who hesitate to vote for it that it amounts to little more than a requirement that quantities shall be expressed metrically; while, after the bill once becomes a law, they

will insist that the inch must really be abandoned, to the endless embarrassment of business in the Departments. They hope to dictate afterward their own interpretation. This should not be allowed. The phrase should be precisely defined in the bill.

The volume before us contains two distinct works. Each contributes something definite, pertinent, and irrefragable to the controversy. In the first, Mr. Halsey shows that the old units are still used a good deal in all the metric countries--nominally, even in France; and really, elsewhere. We regret, however, that the author builds too much on this fact, and that, generally speaking, he too much dilutes weighty arguments with others that are almost trivial, if not sometimes downright fallacious. This fault is avoided by Mr. Dale, who, on the positive side, brings forward facts less open to general observation and very instructive, showing that the English inch is "employed and used" in France and throughout Europe, both in practice and in expression in the textile industry, side by side with the centimetre.

78 (24 March 1904) 231: NOTES

Attributed to Peirce by Fisch in Second Supplement. See also: MS L 159.93.

'Physical Chemistry in the Service of the Sciences,' by Jacobus H. van't Hoff, English version by Alexander Smith (University of Chicago Press), is a small book with a striking portrait of Van't Hoff, of whose genius it is hardly worthy. We read in Mr. Smith's preface that "the lectures were delivered in English," yet the author speaks of the "version here presented" and of "diese englische Ausgabe"; so that we infer that the English of the original needed correction. It is, at least, interesting to see these matters dealt with by this important chemist and physicist.

78(24 March 1904) 237: The Reminiscences of an Astronomer.

By Simon Newcomb. Boston: Houghton, Mifflin & Co. 1903. 8vo, pp. 426.

CSP, identification: Haskell, Index to The Nation. See also: Burks, Bibliography; List of Articles; MSS L 159.249-250, L 159.254-256; MS 1479 (draft).

Nobody will take up Professor Newcomb's blithe 'Reminiscences' in the expectation of social or political genre-pictures or revelations. The theatre of experience of a devoted researcher into an exact science is necessarily extremely narrow. It is doubtful whether he will have known even the chiefs in his own line so intimately as some less engrossed man or woman may have done. A student of science may find interesting information in the volume, as well as a cheery example when his task is discouraging, and a cheering voice when a great effort is to be made. The general reader will, for the most part, go to the book to learn something of that curious human variety, the great savant. Newcomb is quite the most distinguished man of science in this country to-day, as well as one of the most eminent in the

whole world. His name will remain upon the page of scientific history, and eventually take its place high in the second rank, distinctly above Leverrier's or even Hansen's, because of the breadth of his work. He is one of the eight associés étrangers of the Paris Academy of Sciences. This is universally acknowledged to be the greatest public honor that can be conferred upon a non-French man of science. Newcomb is the first citizen of the United States to receive it (if we are right in thinking that Louis Agassiz never completed his citizenship). It has never yet been bestowed upon a native citizen of the United States, although Franklin and Rumford received it.

The unscientific as well as the scientific reader will find these memoirs entertaining. Such anecdotal books always make agreeable reading; and Newcomb's powers of telling a story and of painting a situation are much beyond the mediocre, while his light, pleasant style is quite remarkable. The two best chapters are the one that gives an account of the author's own work, and the one called "Scientific Washington." The brief notice of the Johns Hopkins University may also be singled out as better even than the rest; and it does justice to the singular faculty of Dr. Gilman, that university's only true begetter. The picture of the old Nautical Almanac Office in Cambridge from 1857 to 1861 is interesting, because that office bred a true school of mathematicians and philosophers, students mostly of

Benjamin Peirce. As for scientific Cambridge as a whole, Newcomb was at that time not sufficiently developed, and as a consequence had not sufficient opportunities, fully to comprehend it. But perhaps the gem of the book is the account of his boyhood, a boyhood which, one can see, taught him much that was most valuable just because it did not teach him what he burned to know. It will afford the reader a lesson in human nature, too, by showing how apparently small a difference in his innate make-up would have converted this illustrious man, whose eminence is as little accidental as a man's can be, into an utter nobody, or even into an obscure little quack doctor of Nova Scotia. Young fellows will do well to ask themselves what was the ingredient of his character that saved him. Everywhere, the volume is pleasant reading. The author has shown a perfectly distinct apprehension of who might be interested in his reminiscences, and why, and has written it for them.

78 (14 April 1904) 298: Lectures on the Logic of Arithmetic.

By M. E. Boole. Oxford: Clarendon Press; New York: Henry Frowde. 1903. 12mo, pp. 144.

Elements of the Theory of Integers.

By Joseph Bowden. The Macmillan Company. 1903. 12mo, pp. 258.

CSP, identification: Haskell, Index to The Nation. See also: Burks, Bibliography; List of Articles; MSS L 159.251, L 159.255.

Mrs. Boole's is not a work like Dedekind's 'Was sind und was sollen die Zahlen.' The lectures are supposed to be addressed to children. "The earlier chapters are suited to little children, the later ones for children of fourteen or fifteen." There are twenty-three lectures or chapters, and "not more than one chapter is intended for use in any one term." The book can hardly be said to relate to the teaching of arithmetic, for, as the authoress says, it "is not intended to interfere with ordinary methods of teaching arithmetic." It aims to take advantage of a class having been formed in arithmetic, to teach the children a little logic. The logic, not too definite for the infant mind, is wholesome--occasionally quite refreshingly so, as when the writer says: "The sentimental people who assert that everything in arithmetic can be 'proved' to children have, usually, no idea of what rigid proof means." If she would delete "usually," every exact logician would agree with her. But we wish Mrs. Boole would treat the grown people for whom she writes, in this volume and in others, a little more as she says children should be taught. "If a teacher has anything to say to children as a statement [presumably she means as an assertion or as a proposition or in the indicative mood], he should say it, not exactly as a dogma which they are bound to believe, but as a working hypothesis which they are to assume as a basis for the present." This may be going a little too far, even in reference to assertions made to grown folk; but the tendency of it is good. If we are to believe everything in Pére Gratry's 'Logique,' let it be when we come to see the truth of it.

George Boole was one of the great vehicles of truth of all time. Like all the great philosophers, he possessed the power of working his way to the truth with ideas that could not, at his time, be rendered entirely distinct, for he lived before the true logic of mathematics had been called into existence. He not only had this power, common to Aristotle, Descartes, Kant, Leibniz, and the rest, but he did what only one or two of the great philosophers was able to accomplish--he positively proved the truth of his idea. Gratry is no more to be compared with Boole than the lustre of Vega or Capella is to be compared with that of Jupiter. With some aperçus of high truth, his mind was too much in the attitude of prayer and of preaching to see the necessity of proving what he advanced. The infusion of Gratryism into Mrs. Boole's mind does the children no harm. We only fear that it may obstruct the reception of her wholesome ideas by teachers; as when she says, for example, "Arithmetic seems to some people dry and unbeautiful; but that is because they have not soaked it in that solvent which is called sympathy."

She hardly discusses the fundamental question whether or not it be desirable to cultivate the minds of young children in the direction of deductive logic. That is a matter to be carefully considered. The reviewer believes that she is right in thinking that it is desirable. This kind of logic--the logic of "Mamma says it is wrong"--we believe to be the very kind of thought for children. But it is too momentous a question in education to be hastily decided. Once grant, however, that logical conceptions are to be developed so early, and Mrs. Boole's methods of developing them are certainly exceedingly skilful and quite admirably adapted to the minds of children.

Prof. Bowden gives an independent development of his subject. That is a merit. Any person unacquainted with the logic of arithmetic could gain enough from the book to pay for the trouble of going through it carefully and critically. Further than that we cannot praise it. Its first pargraph [[sic]] is as follows:

"The concept of number, in its simplest and original sense, is a fundamental concept. It is incapable of definition--that is, it cannot be exprest in terms of ideas simpler than itself."

This is not so; and if it were so, there would be no use in such a book. There is no possible account of the logic of number that is not based on the logic of relations, whether consciously or not; and number does not express a simple relation, whether the ordinal or the cardinal numbers are regarded as primitive. This has been made perfectly clear in more than one of the books with the titles of which the footnotes of this volume are ornamented.

On page 3 we meet with this: "Axiom. Any number is equal to itself." A poor sense of logic must a man have to entitle this an axiom, when on the page before he had said, "To [the idea of sameness between two numbers] we give the special name equality."

Whoever wishes to understand the logic of Integers should begin with Dedekind's little book, of which a translation is published by the Open Court Co. There is a good deal more to be read besides if one's appetite holds out.

78 (28 April 1904) 328-330: THE NATIONAL ACADEMY MEETING

CSP, identification: Haskell, Index to The Nation. See also: Burks, Bibliography; List of Articles; MS L 159.257; MS 1480 (draft).

WASHINGTON, April 23, 1904.

The National Academy of Sciences has just concluded an unusually full and lively session here, beginning Tuesday, April 19. The focus of scientific interest, which for so many years burnt in the question of the origin of species, is now decidedly shifted to that of the constitution of matter, and to the partly logical discussions of the fundamental principles of physics. The papers of Tuesday belonged mostly to this field, where nothing is more interesting than the marvels connected with radium, polonium, actinium, uranium, and thorium, on the one hand, and those connected with helium, neon, argon, and xenon, on the other. Concerning the former series, Professor Barker gave some new information, in a paper whose special object seemed to be to introduce the excellent term "autoluminescence." What is called "phosphorescence" (not a very scientific term, according to our present conceptions) is mostly either an accompaniment of chemical change or a prolonged fluorescence; and the term "fluorescence," originally applied to cases in which light is absorbed while in place of it light of another wave-length, mostly blue, is emitted, is now sometimes extended to cases in which the light emitted has the same color as that absorbed. But the term "luminescence" has come in to denote the general property of emitting light with hardly any heat and without apparent chemical change. In 1896, Becquerel discovered that salts of uranium have a property which was afterwards found to consist in their separating the molecules of nitrogen of the air into their ions, or electrified atoms; and this property is called radio-activity. In 1898 the salts of thorium were found to possess the same property.

Mme. Curie ascertained, upon investigation, that different salts of uranium possess this property (which can be accurately measured by its electrical effects) in strict proportion to the amount of uranium they contain. Yet she found that pitch-blende, the ore of uranium, is much more radio-active than pure uranium itself. It must, therefore, contain some other radio-active substance; and by chemical analysis she partially separated from it in 1898 a new element more radio-active than uranium, which she called polonium. A little later she found in the same mineral another new element, vastly more radio-active, which she called radium; and an assistant of hers has found in the same mineral still another radio-active element, which he calls actinium. These five radio-active elements stand in Mendeléeff's [[sic]]

table of the elements at the foot of as many columns.

In March, 1899, Mme. Curie announced that, in addition to its radio-activity, or power of ionizing the air, radium possesses a different property, that of shining eternally without any excitation; and it is this property of luminescence without excitation to which Professor Barker proposes to give the name of autoluminescence. There is no proportionality between radio-activity and autoluminescence. The former depends entirely upon the amount of the radio-active element that is present; but "autoluminescence" is one of those physical properties which seem to require the presence of a small amount of some second substance. Professor Barker, for example, showed one specimen of a salt of radium whose radio-activity is 240 times that of uranium; and it shone so brightly as to be visible all over the darkened, but not quite dark, hall. Yet another specimen of the same salt, so pure that its radio-activity is eighteen hundred thousand times that of uranium, will show no light in absolute darkness.

No other substance than radium was known to be autoluminescent until November last, when Becquerel announced that uranium, also, was slightly possessed of this property. A tube of radium in which careful examination by the spectroscope had discovered nothing else but air, was found, after some months, to contain a quantity of helium; and substances emitting helium have been repeatedly found to be radio-active. Thus, the waters of the Bath springs, which, after suffering the derision of generations, have come again into esteem, were first found to contain helium, and then to be radio-active. There is every reason to guess that any radioactive substance would have an effect upon the health; as in some instances is well known to be the case. Water both from an artesian well and from a lake has been found to be radio-active, and some men of authority believe that all bodies are more or less radio-active, and that the new gases of which helium heads the list are products of radio-activity. Professor Barker exhibited the first photographs printed from their negatives by the light of ionized nitrogen, and very perfect landscapes they were. He also showed a photograph of M. and Mme. Curie and their daughter Adéle, taken by the direct light of radium. It was a little fogged, proving the intensity of the effect to be too great for practical photography.

Dr. Barker's paper was followed by an account, by Dr. E. L. Nichols, of a research by him and Dr. Ernest Merritt into fluorescence spectra. The spectrum in all cases presented a single maximum of light, shading off equally upon its two sides, so as to remaind Professor Webster of the spectrum of an incadescent black

body, and to suggest an inquiry into the effect of temperature upon the position of the maximum. Three papers presented by Prof. John Trowbridge gave excessively condensed accounts of as many extensive researches made in his laboratory upon the spectra of gases; one by himself that has occupied seven years, one by Dr. Theodore Lyman (son of the biologist) that has occupied five years, and the third, of great merit and difficulty, by the young Californian physicist, Dr. H. W. Morse. It is very much to be regretted that only nine or ten hours are devoted at the Academy meetings to the reading of papers, so that only fifteen or twenty minutes, or half an hour at most, can be devoted to single reports of the most elaborate investigations; and numbers have to be gabbled off at a faster rate than the mind can follow them, although without the numbers no real understanding of physical and physiological researches can be had. Professor Trowbridge has employed a storage battery of twenty thousand cells, giving a spark of any length up to seven feet. His work was doubtless undertaken in the hope that facts would be brought out that might shed a light upon the constitution of matter, since the complexity of the spectrum of a gas (making due allowance for series of connected lines) cannot well be greater than that of the molecule that produces it. He finds that an electric current always acts to raise the vacuum, and thus tends to extinguish, and will ultimately extinguish, the light it produces. The hydrogen lines have a special tendency to disappear, and several phenomena go to show that hydrogen itself is an insulator, and that its apparent conductivity must be due to humidity. Professor Trowbridge declared himself to be quite assured by his own experiments, as well as by others, that the presence or absence of a line in the spectrum affords no indication whatever of a specially high temperature--a result very unfavorable to Lockyer's theory of "inorganic evolution," at least in its details. Dr. Lyman has been working upon the extreme ultra-violet part of the spectra of hydrogen and other gases, to which one millimetre's thickness of atmospheric air is absolutely opaque. Hitherto, one experimenter alone has ever succeeded in photographing these lines (the only way of observing light to which the eye is entirely impervious, even if the retina be sensitive to it). But Dr. Lyman has accurately measured the wave-lengths of the extremest lines which seem to exist, to 1,050 Ångström units--a feat that had been deemed impossible. Dr. Morse, with a most ingenious instrument, has photographed spectra of a Wehnelt interrupter. As an incidental result, he obtained in one photograph a flame-spectrum, an arc-spectrum, and a spark-spectrum; so that it would appear that these three kinds of spectrum may be produced by gas of the same temperature.

The only other papers of Tuesday were two, of characteristic ingenuity, by Professor Woodward, in one of which he proposed a peculiar form of pendulum for the absolute measurement of gravity, while the other gave a calculation from which it appeared that the effect of doubling the pressure of the atmosphere would be to diminish the radius of the earth by about 2 metres.

On Wednesday, the Academy received the sensation of the session in a report by Professor Chittenden of Yale on the results of his experiments upon the effects of reducing the amount of food, especially proteids, or nitrogenous food, to less than half what men ordinarily take. He began by rehearsing all the various dietary standards now ordinarily adopted, of which the best known is that of Voigt, who prescribes 118 grammes of proteids daily. Some go somewhat below this figure, others rise far above it. A Swedish standard calls for 189 grammes of proteids in daily food having a heat of combustion of 4,728 large calories. Atwater's American rule is 125 grammes of proteids with 2,500 calories for a man performing severe labor. But Mr. Fletcher has subsisted for many years upon half the proteids of Voigt's standard, with half the fuel value, and has remained all the time in most admirable condition. Now it seemed to Professor Chittenden that the various dietary standards merely show what men are in the habit of eating, and do not at all prove that they might not enjoy greater vigor if they ate less. In order to test the question, he experimented for six months upon twenty-seven men, of whom six were professors and instructors in Yale, thirteen were soldiers under military discipline, and eight were athletic students of Yale. Nothing was absolutely prescribed; there was no weighing out of the food except for the soldiers; but the men were desired in the course of a fortnight gradually to reduce the amount eaten, especially in the morning, so as to bring it down to about half of what it had been. It was, as Professor Chittenden expressed it, not prohibition, but temperance.

He began with himself in November, 1902, when he was a small man eating about Voigt's standard. In a few weeks, he reduced the amount by one-half; and after nine months his body weight had begun to be stationary. He then found that during six months his kidneys carried away 5.82 grammes of nitrogen a day, in place of the 16 grammes that they would have excreted under Voigt's standard. (It will be understood that the nitrogen so carried away is precisely the nitrogen that has been absorbed, the metabolic nitrogen, and is therefore the most important fact.) His health and efficiency seemed to be improved. Similar results were detailed for the other men of the first group. The thirteen soldiers were at first casting off on the average daily 16 grammes of metabolic nitrogen. After reducing their food in the course of a month, during the next five months the amount of 16 grammes was reduced to an average of 7.80 grammes. At the end of that time they had greatly gained in strength and courage, without any complaints of insufficient food. There was no change in their reaction-time nor in the blood, unless there had been a slight increase in the number of red corpuscles. The fullest possible data for each man were given in detail in the report. All the men of the third group were enthusiastic as to the good effect of the reduced ration.

Professor Chittenden entitled his paper "a preliminary report," and confined himself strictly to the facts observed and to the scientific conclusions from them, without indulging in any opinions as to what might be the ultimate consequences of the meagre diet. But the facts spoke for themselves, and undoubtedly produced a great effect upon the minds of all his nonmedical auditors. Dr. Bowditch thought that they showed that the optimum of diet was nearer the minimum of nitrogen equilibrium than the maximum. He thought the digestive organs might under such a diet undergo a partial atrophy, and he would like to know whether the temperature of the skin would not be lowered, since otherwise it was difficult to understand how the heat of the body could be maintained when the fuel value of the food was so much reduced. Two other medical members of the Academy indulged their

professional propensity to enunciate opinions in the customary highly impressive medical tone, one of them drawing a lesson altogether destructive of Professor Chittenden's facts from the feebleness of the Hindus, who subsist upon rice, while the other pronounced an equally magisterial dissent, grounded somehow upon the extraordinary strength, activity, and endurance of the Japanese, who subsist upon rice. There were other comments which showed mainly how surprising Dr. Chittenden's facts appeared to be to many members.

A paper by Dr. Horatio C. Wood, jr., read by his father, related to the drug Canadian hemp (Apocinum cannabinum), whose effects resemble those of digitalis. Professor Osborn, in the name of Dr. W. D. Matthew, gave an interesting account of the arduous studies that had been found requisite in order properly to set up the skeleton of a great sauropodous dinosaur, a reptile quadruped sixty-five feet long.

The papers of Thursday were of various sorts. The most remarkable was a discussion by Gen. Abbot of the disposition of rainfall in the basin of the Chagres River, with a view to estimating the force of the objections that have been raised to the plan of establishing artificial lakes in connection with the Panama Canal. For this purpose it becomes necessary to ascertain how much water percolates through the ground. By remarkably ingenious and solid reasoning, based upon the average monthly rainfall, evaporation, and discharge of the streams, with other data, the monthly state of the ground water was deduced, and it was shown that no danger is to be apprehended from the projected lakes. Professor Osborn gave some account of the recent palæontological discoveries of the American Museum of Natural History's exploring parties, which amount to a complete palæontological survey of the West. From this has resulted the overthrow of Clarence King's theory of a vast lake having formerly existed in that region, in which the animals whose remains are found perished. Moreover, it appears that, throughout the tertiary period, Europe, Asia, and North America formed a single zoological region. Professor Osborn argued not only that North and South America were connected by a bridge in the pleistocene, but that there was strong evidence of an earlier bridge during the cretaceous period. (Professor Agassiz, however, thought the evidence of this quite insufficient.) A fully developed armadillo had been found in Wyoming, and the edentates seemed to be as old in North as in South America. An animal resembling a crocodile had been exhumed in Montana. Three, and possibly four, collateral lines of horses had been clearly made out in the miocene, with very perfect specimens. Another paper by Professor Osborn related to the classification of reptiles. He argued that there were two grand divisions of these animals, which had been separated since the Permian age. One of these was characterized by having only one skull-arch, while the other had two. From the latter are descended all our present reptiles and also the birds. From the former, which was much more conservative in its evolution, were, nevertheless, descended the mammals and man, as well as the tortoises. This paper provoked no little discussion, but the value of the work was acknowledged on all hands.

Prof. A. T. Zahm, introduced by Mr. Alexander Graham Bell, gave an account of experiments for determining the law of the friction of the air at speeds below

40 feet per second. The experiments were well conducted, and led to a very valuable and simple formula, with which they accord remarkably well. Mr. Graham Bell gave an interesting account of his breed of multinippled sheep on his estate of Beinn Bhreagh. Mr. C. S. Peirce read a paper on the simplest possible branch of mathematics. A paper by Professor Newcomb on the application of new statistical methods to the question of the causes influencing sex, was announced, but was not read. On the whole, the session illustrated the progress of science in this country, for more than half a dozen of the papers were memoirs of high importance; which would hardly have happened ten years ago.

78 (28 April 1904) 335-336: COMTE'S PHILOSOPHY

The Philosophy of Auguste Comte.

By L. Lévy-Bruhl. Authorized translation [by Kathleen de Beaumont-Klein]. With an Introduction by Frederic Harrison. G. P. Putnam's Sons. 1903. 8vo, pp. 363.

CSP, identification: Haskell, Index to The Nation. See also: Burks, Bibliography; List of Articles; MSS L 159.243, L 159.247.

Throughout the greater part of the sixth decade of the nineteenth century, there seemed to be some prospect--to many of those who were then laboring to render philosophy, and especially logic, exact and scientific, it appeared as a danger--that the philosophy of Comte, or the first section of it, might become as dominant as nominalism has been since the overthrow of scholasticism. This was feared, not so much because that philosophy was a tissue of contradictions from beginning to end, as because, while teaching the relative, temporary, partially false character of all human knowledge, expressly including the Positive Philosophy itself, nevertheless the very essence and soul of it lay in a recommendation not to pursue certain lines of inquiry, and in a disposition to bring brute force to bear to prevent those inquiries from being pursued. That was the very stamp of Comte. That was his catholicism. Nor were these prohibitions the result of long examination anxiously attentive to all that could be urged on both sides. They sprang, on the contrary, from just such one-sided thought and partisan blindness as produced the revocation of the edict of Nantes and other events of French history. While such a terrible danger was hanging over human science, those who felt it to be such were naturally unwilling to say anything that might be taken for an approval of Comtism. But now that that system is utterly exploded, and that it seems impossible that--in this country or in England, at least, where the sacredness of individualism is felt--there ever should arise any real danger to inquiry from inquiry itself, it is time to apply ourselves to learning what there is to be learned from the many pregnant suggestions of that extraordinary thinker.

France, during the nineteenth century, produced no philosophical ideas of greater value than Comte's law of the three stages of thought and his classification of the sciences. There was an obscure Dr. Charles Burdin, who died, we believe, during the fifties, from whom, it has been very positively asserted by reputable writers, Comte appropriated both these doctrines. It is impossible to believe that Comte was a conscious plagiarist; for even if his morals had been less strict than they were in literary matters, of all the victims of the plagiarist the easiest to delude is himself. But it is almost equally difficult to admit that such writers as those who repeat the charge should have done so without weighing their words.

To say that a broad philosophical conception is altogether new, is almost equivalent to a condemnation of it. That anybody has given it its definitive form can hardly ever be said. Comte's conceptions of the "theological," the "metaphysical," and the "positive," were very hazy, indeed. What he called a "theological" account of a phenomenon was a view which attached to the phenomenon one of the familiar ideas of human life--an idea in Hegel's naïve stage. A "metaphysical" account was one which regarded an abstract concept as being illuminative apart from its predictive value--roughly corresponding to Hegel's second stage. What Comte meant by a "positive" account was one which avoided doing what it has quite commonly been the triumph of science to do, as in the kinetical theory of gases, the oscillatory theory of light, the ionic theory of electrolytes, and the like--namely, to pass beyond the general formulation of facts to likening them to diagrammatical ideas, and thus to explaining them by theories which probably contain some admixture of error. Neither Comte nor Hegel sees anything to commend in this. Poincaré and his like are Comtists in this respect, while Bolzmann and the older physicists generally are of a contrary faith. But that there are three stages in the comprehension of phenomena is now generally admitted, whether this be a mere logical division, or represent a subjective tendency to divide by three, or whether it be an objective law, as Comte seems to think it is.

Comte holds, and it is generally too hastily admitted, that his classification of the sciences is, and can only be, the immediate offspring of his law of the three states. Dr. Arnott had already formulated essentially the same classification, with the same ladder-like relationship between the different sciences, without having any notion of three stages of any kind. There is quite another conception which differentiates the Arnott-Comte classification from all others. It is that the others are simply classifications of the different conceivable branches of knowledge. They are mostly classifications of the objects that can be known, and, moreover, almost all relate to what is possible rather than to actual fact. Every one of them is concerned with science either in the sense of {eposthmh} (scientia), or in Coleridge's sense of "systematized knowledge." This is true, for example, of Whewell, of whom one might have expected something different. But for Arnott and Comte a science is an historical development, the collection of those phenomena that are connected with the fact that a social group have been devoting their main energies to the pursuit of inquiries so closely allied that the persons who compose it understand one another, their conceptions and their sentiments, as it is impossible that others should. As soon as the word science is thus made to mean a congeries of actual phenomena, the sciences become susceptible, for the first time, of what we call a "natural" classification--that is, a classification which displays, in a useful way, the principal general relations which we have learned

from observation concerning the more important resemblances and differences of the objects classified--in short, what, in a reformed sense of

Comte's word, may be termed a positive classification. Now the fact which it seemed both to Arnott and to Comte to be most desirable that such a classification should exhibit, was the well-known fact that one science will very often depend upon another very largely for its axioms while contributing little to that other unless it be material for study. All classifications of the sciences that have been made since Comte's (some fifty in number) have been methodeutically useful pretty nearly in so far as they have recognized that principle.

Not only the 'Cours de Philosophie Positive,' but also the 'Politique Positive' and other of Comte's works, are brimful of thoughts that are warm (as they say in Hide and Seek) to living truths, although these are invariably most inaccurately analyzed and formulated. For example, nothing can be truer than that a hypothesis is good for nothing unless it is, as Comte says, "vérifiable," provided this means that the hypothesis is of such a nature that, if true, it will lead to correct anticipations as to the characters of percepts and diminish the number of surprises; but Comte's definition, that a verifiable hypothesis is one whose substance is of such a nature as to be capable of being itself directly perceived, makes his maxim an arbitary and indefensible limitation of useful knowledge. The past, for example, is of its nature incapable of being directly perceived, and therefore, according to Comte (too literally taken), we ought not to believe at all that there ever was any past, and, as he proposes to give up the use of the word "cause" altogether, so he ought to have given up the preterit definite and indefinite of all verbs. Then his law of the three stages would relate in the main to nothing real, but to mere phantoms of the brain, and would fall under the interdict of the high priest of Positivism. He never attempts to define his ideas without similar failure.

Since M. Lévy-Bruhl's book, though its author is not a Positivist, carries the approval of Mr. Harrison, the leader of the remnant of the school in England (none of them very great minds, one infers from their writings), the rest of the world ought to be content with it. It is truly a useful compend, in which great attention has been paid to the accurate representation of Comte's opinions. We doubt its causing anybody whose philosophical opinions are important to think better of Comte than he did before, since the impression that it produces is that of a piece of special pleading, one-sided and insincere. We cannot deliberately suppose that it really is insincere, but we have in sundry places been tempted to think so. Upon the reviewer, who had not looked into Comte's writings for many years, and who may, perhaps, serve as an average sample of M. Lévy-Bruhl's readers, the effect of the book has been markedly to lower the reviewer's estimate of Comte and quite sensibly to raise his estimate of Spencer. It is a well-constructed work, and is written in a very agreeable style. The translation is also agreeable despite an occasional Gallicism, such as the oft-recurring construction "substitute to" (substituer à). The print and paper are agreeable, too; but the proof-reading is not excellent, and, where French occurs, is bad.

78 (19 May 1904) 393: NOTES

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

--There are great advantages in learning any mathematical theory which is subsequently often to be applied, from a logical syllabus expounded in lectures. The propositions should be numbered in the syllabus in one series, from beginning to the end; and propositions very frequently employed in the proofs should have, besides, brief names. Then, in the case of a corollarial proof--that is, one not requiring the introduction of any subsidiary lines or quantities--mere references to the numbers of the premises, sometimes with the number of times each is to be applied, will generally suffice. In case this is not enough, brief indications of how those premises whose application is not quite obvious are to be applied may be added. Theorematic proofs--that is, such as depend upon some ingenious addition to the conditions of the proposition to be proved--will require this addition to be stated; after which the proof becomes corollarial, and should be treated like any other corollarial proof. The student having become perfectly familiar with the arrangement and general contents of such a syllabus, by working through it, will ever after find it an invaluable work of reference, in which any result may be found directly, together with the logic of it in a nutshell. Dr. A. Clement Jones's 'Notes on Analytical Geometry: An Appendix' (Oxford: Clarendon Press; New York: Henry Frowde) approaches to being such a syllabus in a duodecimo of 172 pages, of which 30 are occupied with examples and hints for their solution, with a perspicuous one-page index. The work is confined to plane curves of the first two orders, together with unicursal cubics, the whole treated from the metrical point of view, with Cartesian coordinates and occasional references to polar coordinates. The most serious omission is the problem of two conics without contact, with single contact of 2, 3, or 4 points, and with double contact, which is often wanted and is not very readily worked out. A better book of the same sort might be made; but, as it is, Dr. Jones's 'Appendix' will prove a lifelong blessing to many a student.

78 (26 May 1904) 411: NOTES

CSP, identification: MS 1481 (draft). See also: Burks, Bibliography.

Whoever likes to have some tolerable conception of the practical problems connected with things that he daily deals with, will be interested in a fairly readable little book entitled 'Electric Traction' (Whitaker & Co.), by John Hall Ryder, the chief electrical engineer of the London tramways. It discusses such details as controllers, conduit systems, accumulators, etc., from the point of view of a man in a high responsible position. The work is, to be sure, calculated for the meridian of Greenwich; but that only adds to its interest, especially as the author never loses sight of American practice, which indeed would be quite impossible in the field of electrical engineering. In special parts of that field Germany and France are in advance of us, although it is admitted that we lead on the whole. How backward England is in some respects is shown by Mr. Ryder's considering only direct-current dynamos, though the alternating system is used on the Manhattan Elevated, and though the majority of recent advances have been in that direction. On the other hand, he seems to have the best of the argument in condemning our conduits midway betwen the rails.

78 (26 May 1904) 411: NOTES

CSP, identification: MS 1481 (draft). See also: Burks, Bibliography.

The new edition of Hawkins and Wallis's 'The Dynamo' (Macmillan) is, on its practical side, a new work. The first edition, which preceded this by just ten years, and which was in its day an admirable compendium, is now as much out of date as Johnson's Dictionary. The new edition, though double the size of the first, is relatively hardly as full, in view of the art's advance. It is easily the best book on the subject, but does not enter into the minutest details.

79 (7 July 1904) 15-16: TURNER'S HISTORY OF PHILOSOPHY

History of Philosophy.

By William Turner, S.T.D. Boston: Ginn & Co. 1903. 8vo, pp. 674.

CSP, identification: Haskell, Index to The Nation. See also: Burks, Bibliography; List of Articles; MS L 159.266; MS 1482 (draft).

Some apology may be due to our readers for giving more than a line or two to a book which professes merely to be a one-volume text-book of the whole history of philosophy. But this is no ordinary third-hand compilation. The thorough reader of philosophy will find himself here confronted by a peer whose judgments are his own (barring in a sense those that touch dogma) in a larger proportion than is usual, the principal exceptions being that, in ancient and early mediæval history, imaginations of German higher critics are too easily assented to. As one goes through the volume, one becomes interested in considering the views of the author, and finds them mostly acceptable. Stöckl's various writings, the manuals that have emanated from Stonyhurst, and sundry other books and articles had already produced upon us the impression that Leonine Thomism was a decidedly favorable standpoint from which to survey the course of philosophy; and the present volume bears out that impression. There is a reason why it should be so: the adherent of any modern school is a nominalist--that is, he believes in only a single fundamental mode of being, and his power of conceiving other modes has become atrophied from disuse. But a true Aristotelian, of whatever stripe, must recognize a germinal mode of being, a positive, substantial possiblity, or potentiality, over and above actual existence, or exsistence. He is thus in a condition to understand both nominalistic and realistic conceptions of the universe. The most favorable standpoint, on this principle, should be that of the Scotist, who is habituated to thinking of three modes of being--matter, or the positively possible; form, or that whose being consists in its general governing of what in any way is; and hecceities, or positive elements of individual existence. Unfortunately no considerable Scotistic school of thought is now extant. Only a pragmatist, here and there, has a sort of affinity to Scotus.

Apart from all this, it has long been coming home to the philosophical world that a more thorough appreciaton of the realistic theory is one of the prerequisites to philosophy's ever developing into a stable science. It is a chief merit of this little book that it furnishes a more intelligible and truer account of the development of scholastic realism than can be found in many a more pretentious history of philosophy. The system of St. Thomas Aquinas, beyond dispute one of the greatest that have ever been formed, is here expounded with accuracy and in some detail. The modification of that doctrine by Scotus--departing from it, roughly speaking, about as much as Aristotelianism departs from Platonism--is less sympathetically treated. The reasons for the modification are not explained, nor is the essential characteristic of it clearly set forth. What is said of it, however, is true enough; and that is more than the Scotist is accustomed to find in compendiums or even in books devoted to explaining the doctrine he follows.

Father Turner's account of the rise of scholasticism is particularly comprehensible and in the main points just. During the three centuries and more that elapsed between the 'De Divisione Naturæ' of Erigena and the granting of the first distinctly recorded privilege to the University of Paris by Philip Augustus, A. D. 1200, we hear of some sixty teachers of philosophy; and of most of them we barely learn their existence. About a third of them, including all the most important, receive separate treatment by Father Turner, thus affording a tolerably definite idea of one side--the internal side--of the line of generation of scholasticism proper, the scholasticism of the University of Paris. The account given of the philosophy of the fourteenth and two following centuries does not satisfy us nearly so well. It is difficult to conceive that a person who had ever made any serious study of Ockham could have given so colorless a protraiture of that strong and singular thinker, whose subsequent influence has been so mighty that a close examination of his thought was called for. The decay of scholasticism is largely attributed to nominalism; and no doubt there is some truth in that. For not only does nominalism render any religious philosophy impossible or absurd, but the empty formalism, oppressive triviality, and soporific verbosity of the mediæval nominalists--especially the later ones, though Ockham himself is hard to beat in the last particular--was calculated to disgust every lively mind. Yet, after all, the Scotists retained the upper hand in the most important centres to the last; it was not the Ockhams but the Dunses that most excited the resentment of the humanists; and it was, in part, because of this resentment that the modern world became nominalistic. Yet it should not be forgotten that the study of the Stoic and Epicurean philosophies was an important factor in this.

Besides nominalism, Father Turner holds the chief cause of the decay of scholasticism to have been Averroism, especially the proposition that what is true in philosophy may not be true in theology. Is it not singular how most men regard this proposition as simply abominable, while another proposition, differing from it chiefly in being more general, is by many if not most men considered as the capstone of good sense--we mean the proposition that what is true in theory may be false in practice? The meaning of this is, of course, that a correct deduction from a theory may, in consequence of the impossiblity of any general theory taking into account all the factors which affect experience, be found quite contradicted by experiment. Nobody will be more alive to this than modern mathematicians. What, then, is it that Averroes adds to this that makes him proposition so abominable? It is that if a deductive conclusion is so abhorrent to one's most intimate conscience that to accept it would be to break up one's whole system of moral habitues, it is better to adhere to natural or quasi-natural sentiment, and to suppose that some undetected error affects the philosopheme, although it appear evidently to follow from axioms. Averroes advanced his proposition on the occasion of such a contradiction presenting itself in this own experience. He was in a situation similar to that of one of those religious speculators whom we sometimes read of in the newspapers, who reason it out that it is their duty to burn their children alive or commit some other enormity. Is it really so abominable, in such a case, to do as Averroes did rather than to follow apparent reason to the destruction of all that one has been accustomed to hold sacred? Father Turner says that the proposition of Averroes is contrary to the basic principle of scholasticism. Undoubtedly it is so, in so far as the general proposition that what is true in theory may be false in practice is contrary to the basic principles of the theory. But that is not to say that the acceptance of the proposition is unfavorable to further and closer study of the theory. Would it not be a truer account of the decadence of scholasticism to say that any purely deductive theory, like those of scholasticism, must eventually exhaust its interest owing to all the important consequences having been already made out? Pure mathematics is saved from exhaustion only by starting new sets of hypotheses as the former ones become uninteresting.

However, there was an entirely different order of causes tending to disintegrate scholasticism which Father Turner perhaps considered beyond the scope of this manual. Namely, there are certain incongruities necessarily accompanying a priesthood, sacred things--such as masses and intentions of masses--being bought and sold like railway securities; a court, like profane courts, a hotbed of vice and iniquity, etc., etc., which seem natural enough to a barbarian like the European of the ninth or tenth century. Now scholasticism itself, together with other agencies, had cultivated men's minds up to the point where such sordid and vile things, interwoven with the most holy, inevitably produced upon the lay mind an intense disgust which, where there was a certain degree of intellectual strength, made a readiness for unbelief, if not unbelief itself. Then, just as we all know that crude forms of punishment may answer well for very young children, but would have disastrous effects at a later age, so the barbaric manner in which the Church always treated heresy prevented its expression, and so not only rendered unbelief

invulnerable, but added to it a cynical hypocrisy which disinclined minds to any philosophy like scholastic realism. For this hypocrisy said to itself, with a shrug of the shoulders, "I believe what I see, and nothing else." There is enough of it eating the bread and butter of the Catholic Church to-day to illustrate what we mean (not without force) to anybody who is at all acquainted with that communion. Combine causes such as these, and the æsthetic fashion of mind that the Constantinopolitans imported into the West, with the inevitable exhaustion of a deductive treatment of theology, and causes for the dissolution of scholasticism seem only too abundant.

We regret that the limits of our columns forbid our considering Father Turner's interpretations and criticisms of modern philosophy, which are always interesting, and under a good teacher will prove stimulating to the thought of the student. This effect will only be heightened by there being occasional little exceptions to be taken. We will illustrate these by two instances, one of criticism and one of interpretation. Namely, Father Turner calls Locke "superficial." Now, it is hardly conceivable that anybody, in the light of modern psychology, should deny what Father Turner probably means. But how will the neophyte understand the word "superficial"'? Superficiality is the opposite of penetration. It is the effect of not having thought much about the propositions one enunciates. One of Locke's most remarkable traits, shown on every page of his masterpiece, is that every section brings the fruit of enough thought to furnish forth a considerable essay. Not defective penetration, but the failure to take into consideration other circumstances than those which he has studied--that is the characteristic fault of Locke.

The other instance to which we shall refer is that Father Taylor says that Descartes, in his reply to Gassendi, "protested that the [Cogito] ergo sum is not an inference." This was Dugald Stewart's notion; but it is not tenable. Descartes often said that the Cogito ergo sum was not a syllogism or an enthymeme, nor founded on the syllogistic theory of reasoning. Yet even that hardly gibes with his version of it in the 'Principia.' It is true that he calls it "tanquam rem per se notam simplici mentis intuitu." But what he means is that when one considers that one thinks, one at once perceives thereby that one exists. He thus makes the knowledge of one's existence an effect of the knowledge that one thinks. As Cousin said, "Le donc je suis n'indique-t-il pas un lien logique? Commen Descartes emploie toujours ce mot quand il raisonne, n'est-il pas naturel de croire que ce même mot a ici le même sens que partout ailleurs, et ce rapport des termes ne marque-t-il point celui des procédés intellectuels? Si le donc n'a pas ici un sens logique, pourquoi Descartes ne l'a-t-il pas dit?"

We have found so much fault with the book that our readers may not understand why we like it so much. It is because it is neither a machine-made compilation nor a vague essay, but is the work of a real student of the history of philosophy. We may add that its statements are formally distinct and explicit, as befits a book for beginners and a text-book. There is a thirteen-page index of proper names, and of such subjects as Absolutism, Academies, Accadian traditions, Æsthetics, Agnosticism, Air currents of the Stoics, Antinomies, Arabian philosophy, Arianism, Astronomy, Atomists.

79 (21 July 1904) 63: **Spinoza's Political and Ethical Philosophy.**

By Robert A. Duff. Glasgow: James MacLehose & Sons; New York: The Macmillan Co. 8vo. pp. 516.

CSP, identification: Haskell, Index to The Nation. See also: Burks, Bibliography; List of Articles; MS 1483 (draft).

Baruch de Spinoza died on the twenty-third of February, 1677; and when he shall have been dead for two centuries and a half, it is safe to say that a higher rank in philosophy will be commonly accorded to him than ever before. The vicissitudes of his fame, and not of his fame only, but of the understanding of his doctrine's prevalent color, have been unparalleled by those of any other modern philosopher, and are more surprising--yes, and more discreditable to readers of metaphysics--than those which the estimation and interpretation of Duns Scotus, Aristotle, Plato, and Epicurus have undergone, since different civilizations must be expected to apprehend such all-permeating conceptions very differently. When Spinoza's principal works appeared, as they first did shortly after his demise, in one collection, the judgment of Leibniz was that they merely traced out a little further some consequences of Cartesianism; but they soon got that brand "atheistical" scorched upon them that has not entirely worn off to this day. The original meaning of it was that they displayed that incipient tendency toward regarding the books of the Bible with the eye of historical criticism which had no little popular vogue about 1700; but before long, as in so many instances, the word was retained while the meaning of it was entirely changed, as one can see by turning to Bayle's 'Dictionnaire,' where Spinoza is set down, without misgiving, as a materialistic pantheist. When Malebranche called the doctrine an "épouvantabel et ridicule chimère," the phrase reflected a curiously jumbled misapprehension.

About the middle of the century Wolfius honored Spinoza with a refutation, and a definitive judgment seemed to have been reached on all hands that his philosophy merited notice only for its oddity. Then, as the century drew toward its close, the sort of people who gape in admiration of Jakob Boehme, first in Holland and afterward in Germany, took up, as a help or substitute for that, a mystical and sentimental Spinoza fad. Herder caught the infection; and later, Moses Mendelssohn; and still later, Goethe and Schiller. Of course, all these literary folk looked upon the geometrical form of the 'Ethica' with simple awe. So, indeed, or almost so, did all readers of Spinoza until recently, although it is the only thing in his books that is ridiculous, the only thing about the man that is not venerable. The celebrated philosopher Friedrich Heinrich Jacobi made a more serious study of the 'Ethica'; and he and, more particularly, Hegel brought in that conception of its metaphysics which exaggerates the rôle that idealism plays in it; albeit its idealism is, in truth, very remarkable, considering that Berkeley was not born until several years after Spinoza died.

The importance that the Hegelians attributed to the strange thinker led to his works being studied more generally, more carefully, and more impartially than ever before. They were many times republished in the original Dutch and Latin--upon

which latter Spinoza so set his own stamp--not to speak of translations following one another like waves on a beach. We would that the 1895 edition of the edition of Van Vloten and Land could give place to a third issue which, retaining the commodious form, should correct misprints and should confront each page of the Dutch with an English, or even German, translation. Meantime, commentaries have appeared in such numbers that the mere perusal of the principal of them has become a great task. and their study a specialty. But the principal question is the very reverse of a minute one. In regard to another philosopher, say Kant, we compare the three versions of the deduction of the categories to see how they gibe, we ask whether the refutation of Berkeley accords with the first edition of the 'Critik,' how far the Critic of the Judgment is in harmony with that of Pure Reason, and the like. But in reference to Spinoza, it is the general attitude of his mind that is in question; and the general lesson we derive from the leading discussions is that the commentators have been apt to restrict their studies too much to the one book that is so formal, that they consider Spinoza too exclusively as a metaphysician, and that they have not paid sufficient attention to his extraordinary approaches toward pragmatism. Such had been the conviction of the present reviewer before he took up this volume of Mr. Duff's, who presses the same opinions much further than the reviewer had conceived them to be warranted.

Mr. Duff surveys the works of Spinoza as a whole; and his remarkable acquaintance with them enables him at each point to cite chapter and verse in support of his interpretations. Confining himself strictly to interpretation without criticism, and putting aside Spinoza's metaphysics, so far as it is possible to do so, he forces us to acknowledge what we venture to think will be a novel idea to most of our readers, that Spinoza regarded philosophy from an intensely practical point of view. Mr. Duff makes no reference to pragmatism. For aught there is in his volume, he may never have read a page of James, or Schiller, or any other pragmatist. Of course, he could not say that Spinoza ever enunciated the principle of pragmatism, which is that even the abstractest of our conceptions has absolutely no meaning otherwise than in so far as it has a conceivable bearing upon human conduct. But he brings before us a Spinoza so far on the road to that opinion that we cannot help guessing that if, instead of dying at the age of forty-four years and three months (all but one day), he had lived to the age at which men commonly come to philosophical maturity, he might very likely have conferred upon philosophy the inestimable advantage of a formulation that vindicates so many judgments of common sense and of anthropomorphism. Already, as Mr. Duff points out, Spinoza had thoroughly recognized, as a fundamental truth, that the substance of what one believes does not consist in any mere sensuous representation, but in how one would be disposed to behave. How long, then, could it be before he would come to ask himself, "If that is what belief is, how can a belief relate to anything but behavior?"

Spinoza, according to Mr. Duff's presentation of him, was the last man in the world to care for abstract speculation. He was animated with the desire to do his practical part in making men better. How men were practically to be made better was his problem. In order to solve this problem, it was necessary to begin by analyzing it, and this drove him perforce to metaphysics. His real study, however,

was ethics; and he understood by ethics an infinitely more practical science than many writers upon the subject do in the twentieth century.

Of certain faults in Mr. Duff's work we shall say nothing, for, as long as metaphysics is avoided, they are of no consequence. The book is by no means a mere study of the history of philosophy. The author expects his reader to be interested, not solely in the fact that Spinoza thought so and so, but in the substance of his cogitations as well. The reviewer, individually, will frankly confess that, in going through the volume, he has had a difficulty in repressing an occasional movement of impatience at this. On the other hand, this is about the only Spinoza book that is not handicapped by the weight of technicalities. Its style is as comfortable as one's favorite easy-chair can be, and receives every aid that beautiful printing and delicious paper can render. It is the only book we know of that considers Spinoza from the most comprehensive single point of view.

79 (28 July 1904) 84-85: Notes on the Composition of

Scientific Papers.

By T. Clifford Allbutt. Macmillan Co. 1904. 8vo, pp. 154.

CSP, identification: Haskell, Index to The Nation. See also: Burks, Bibliography; List of Articles; MS L 159.262; MS 1484 (draft).

Dr. Allbutt adds to the characteristics of an elderly physician those of a don of a small college in an English university. Thus, he is particular to let us know that he has "read no grammars, nor the handbooks of literary artists." In other countries an author usually desires his readers to know that he has not entirely neglected the literature of the subject upon which he writes. But, considering how nearly identical the greater part of the matter of these "Notes" is with the time-honored recommendations that are found in all the textbooks of rhetoric, we cannot suppose that Dr. Allbutt expects us to think he has worked them all out by original reflection. The volume contains not a few remarks that argue a higher kind of discrimination; but a good many of these have sounded in our ears like reminiscences of observations that were certainly made by one or another of the older French writers on style, from Pascal to Sainte-Beuve; and, notwithstanding his modest-arrogant disclaimer, we should not wonder if all that La Bruyère, Fénelon, Voltaire, Marmontel, Vauvenargues, and Buffon ever wrote about style were perfectly familiar to Dr. Allbutt. Yes, and the utterances of modern critics as well. For when he tells us that 'Le Capitaine Fracasse' (which, though he does not name it individually, is included among its author's writings of which he does speak without limitation) "will not endure," for reasons which would equally apply to the conversations in Alexandre Dumas's novels and 'Impressions de Voyage,' this sounds a little as if it might have been said by one of the later critics in the Revue des Deux Mondes. At any rate, it is sufficiently striking. Dr. Allbutt evidently approves of French rhetoric. It is something like that that he chiefly aims to enforce, though he sometimes fails to strike the nail squarely on the head. Thus, in order to illustrate Sainte-Beuve's manner of opening a "Causerie du Lundi," he quotes a sentence (in translation) which would have been an illustration of this author's sometime failing to devise the kind of opening he preferred for his causerie, were it not that (though Dr. Allbutt does not remark it) it really comes from one of the 'Portraits Comteporains,' where a somewhat different style was chosen by Sainte-Beuve.

The "scientific papers" of the title-page appear in the body of the book as "scientific essays," by which are meant these required from candidates for the degree of M.B. or of M.D. by the University of Cambridge--a motive entirely different from that of any genuine scientific writing. Now, rhetoric ought to be the doctrine of the adaptation of the forms of expression of a writing to the accomplishment of its purpose. Hence, the rhetoric which is specially appropriate to a thesis for a degree (whose soundest maxim is that the forms of expression must be such as the examiner, Dr. Allbutt or whoever it may be, will approve) is not specially appropriate to any writing having a really scientific purpose. The almost incredible comicalities of incorrect writing with which this volume is no less replete than are ordinary books of rhetoric, are examples of what writers of all kinds should shun. We doubt if they could have been culled out of American country newspapers--unless from the columns of jokes. Bad style rather than good style of writing has always been characteristic of the medical profession; it naturally would be so, for more than one reason. Sir Thomas Browne is often set up as one of the glories of English literature, and the mannerisms of the majority of doctors have been less agreeable than his; moreover, of the really great stylists among them a few only carry a distinctly professional stamp. But it is very surprising to find how atrocious must be the faults of the theses which call for such a book as this to correct them.

Dr. Allbutt's own style is one of those that may be perfectly delightful to some persons and at the same time nauseating to others. He is more captious than correct, and more meticulous than engaging. He is very fond of employing technical terms of logic, but almost always applies them wrongly. He insists, for instance, that the word "theory" can properly be used only in a sense which would make it nearly synonymous with "theorem." Yet in one place he seems to confess (what is at all events true) that such a limitation is not in accordance with usage. From his dicta it would follow that the "atomic theory" ought not to be so called. The word "scientific," on the other hand, is used by him with extraordinary latitude. Thus, we read that "scientific writers are apt to suppose that restatement in bigger words is explanation." Evidently, the "scientific writers" he has in mind are the writers of the "scientific essays" he talks of--undisciplined candidates for medical degrees.

It is singular that while he holds that very few, if any, pairs of English words are synonyms, yet he thinks that all such half-English expressions as raison d 'être, trout ensemble, cortège, par excellence, have their precise English equivalents. The extreme improbablity of the proposition passes unnoticed. But who cannot see that all this is nothing but his personal taste, good or bad, hunting in quite a wrong direction after justification? In many cases his judgments are good, while the reasons he gives for them are bad. But in this, as in most respects, the book is very much like any ordinary book of rhetoric. Had he dropped all pretension to being himself very scientific or to having anything to say specially germane to the communication

of scientific discoveries, and had he acknowledgd that his book differed from a common text-book on style chiefly in not covering the ground systematically, we might have thought it a nice little thing in its way.

79 (25 August 1904) 162: NOTES

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

--Mr. Roberdeau Buchanan, who did the eclipse work of the American Ephemeris through more than a saros and a quarter, has published an octavo book of 247 pages and eleven plates (29 figures), explaining the computations in the utmost detail ('The Mathematical Theory of Eclipses,' Philadelphia: Lippincott). Mr. Buchanan is a master in the craft of computation, in the grade that belongs to Nautical Almanac work, though not, presumably, in the more delicate operations of the geodetical computer. The beginner who desires to initiate himself into the computing craft cannot do better than to go through the computations of an eclipse under the guidance of Mr. Buchanan. That of a total solar eclipse so as to determine the limits of totality is by no means the simplest thing in the world, and some pretentious treatises contain very mistaken statements about these phenomena. In Mr. Buchanan's treatise all is made so extra plain that we meet with such remarks as this: "There is a principle in the theory of differences that I have never seen in any of the ordinary works on interpolation, and that is, the symbol {&unk; triangle} is distributive." This means that if the Joneses have both more boys and more girls than the Browns, then the excess of the Jones children over the Brown children is the sum of the excess of the boys and the excess of the girls. The paragraph devoted to the exposition of this original idea concludes: "This principle I have found of very great use in certain methods of computation for shortening the work." We need not say that the works on interpolation which do not mention this presuppose a familiarity with the simplest elements of finite differences. Mr. Buchanan refers his reader in all cases to Chauvenet for the analytical developments, so that the first three of the nine lines of the title of his book very accurately describe just what the book omits. We will only add that, were we to go into technical details, we should have sundry strictures to make. Nevertheless, the work is excellent on the whole.

79 (8 September 1904) 203-204: The Collected Mathematical Papers of James Joseph Sylvester.

Volume I. Cambridge (Eng.) University Press; New York: Macmillan.

CSP, identification: Haskell, Index to The Nation. See also: Burks, Bibliography; List of Articles; MS 1485 (draft).

We receive with delight this first instalment, a beautiful and comfortable volume closely matching in outward appearance Forsyth's 'Theory of Functions.' It contains Sylvester's work from 1837 to 1853. At this moment, when the chill of

senility begins to be perceptible over the very formalistic mathematics that has been and still is in vogue, the virile genius of Sylvester needs to be more fully appreciated. Doubtless there are many memoirs more significant than his and of broader conceptions; but we doubt if there be any whose thought has the peculiar mathematical quality in a higher degree. There are more flawless gems of mathematical workmanship, there are papers of more perfect polish in their execution; but we are strongly inclined to think that there are none quite so instructive in the heuretic art, partly for the very reason that these have not been so finished as to conceal the brush-marks, partly because of the personal originality and singularity with which they are stamped, and partly because Sylvester's garrulity led him almost constantly to tell how he came by his ideas. It would do well worth the while of a student of methodeutical logic to take up the theory of invariants, just for the sake of comparing the ways of thinking of Cayley and Sylvester as exhibited nowhere so well as in this volume, with those of Clebsch, Gordan, etc.

Sylvester's habit of throwing his whole being--or only sparing to poetry and sentiment their strictly necessary aliment--for long year after year into the development of a single system of ideas, while recording his progress every two or three months (every month, in his most active years), renders this collection instructive beyond measure. Nor is the interest exclusively mathematical. Logical remarks of value are constantly occurring, and other philosophical suggestions are not rare. In one place we read, "Universal geometry brings home to the mind with an irresistible conviction the truth of the Kantian doctrine of locality." Verily, metaphysics is the Paris of the intellect: no sooner do the most scrupulously severe reasoners find their feet on this ground than they give the loosest reins of license to their logic. Universal geometry can testify concerning no other Space than its own, which is a space, not of three, but of an indefinite number of dimensions; and nothing is more striking in this generalized geometry than that it is decidedly easier for the human mind to comprehend a space of four dimensions than one of three. Give a higher geometer sixty days to accustom himself to a four-dimensional space, and he would be ever so much more at home there than he ever can be in this perverse world. Meantime, the dynamics of rotations asserts downright that the rotational part of motion, at least, is not relative; and as for the body alpha, the epistemological difficulties of this disguise of the reality of space are too serious; and if the fixed stars, or the whole universe, be identified with the body alpha, the difficulties become downright absurdities. The only body alpha that epistemology can admit is the body of space itself. Meantime, even if one were to prove that three-dimensional Euclidean space is native to the mind, that would be no argument in favor of Kant's position that it is merely an affair of the mind. On the contrary, the proper presumption would be that, in view of the unity of the universe, if such space is native to the mind, probably it is native to the outer world of reality also.

The volume is not without glimpses of human nature. Lagrange's so-called demonstration of the principle of virtual velocities "is contrary alike to sense and honesty"--yes, that is the color of it, "albeit sanctioned by the powerful oral authority of an ex-Cambridge professor." One wonders how its case would have gone without that orally powerful sanction. As for Lagrange's proof, it appears to us that

cavillers mistake the purpose of it. At any rate, it does convince all reasonable living doubters, for the reason that every man's experience has given him an instinctive and virtual knowledge that work always has to be paid for, which the proof of Lagrange either tacitly takes for granted, or, as we interpret it, supposes to have been expressly admitted for the case of one pair of blocks and tackle. But we must confess to not having looked into the immortal book for many a year.

In every way this publication is a precious benefaction to mathematical students, especially to those who have been treading too exclusively the boulevard of dominant ideas. The four earliest papers relate to the mathematics of physics. Then, for five years, from 1839 to 1844, Sylvester was occupied with elimination and multiple roots. Between 1844 and 1847 the editor, Mr. H. F. Baker, has found nothing. Has he searched Adrain's Diary? Three papers of 1847 relate to the integer equation--

$x^3 + y^3 + z^3 = Dxyz$

The two following years are blank; but from 1850 to the end of the volume

(1853) papers follow one another at an average rate of one every month. Here we find the Essay on Canonical Forms, well-known discoveries in Determinants, the "law of inertia," and the great memoir on syzygetic relations and Sturm's theorem, which last was the first of Sylvester's papers to be ushered in with a poetical motto. It is those lines, "How charming is divine philosophy," in which the Lord Chancellor so successfully disguised his detestation and disgust of all the philosophy of his day.

We shall endeavor to keep our readers apprised of the appearance of future volumes, and we hope to find fewer clerical errors in the next. A chronology of Sylvester's changes of place, travels, and, if possible, of his forming of mathematical acquaintances, would be of advantage.

79 (15 September 1904) 219-220: LOGICAL LIGHTS

Ch. Renouvier, Membre de l'Institut: Les Derniers Entretiens.

Recueillis par Louis Prat. Paris: Armand Colin. 1904. Pp. 107.

Studies in Logical Theory.

By John Dewey. With the Coöperation of Members and Fellows of the Department of Philosophy. (The Decennial Publications, Second Series, Volume XI.) University of Chicago Press. 1903. 8vo.[[sic]] Pp. 378.

CSP, identification: Haskell, Index to The Nation. See also: Burks, Bibliography; List of Articles; MSS L 159.249, L 159.264; MS 1486 (draft).

The metaphysical and logical philosophy of Charles Bernard Renouvier, who died on the first day of last September, aged eighty-eight years and eight months, is the most highly esteemed of any by the average Frenchman of to-day; or, as Renouvier himself perhaps more accurately put it, "il est entendu que Renouvier est très fort, mais on ne le lit pas." Considering that the proper date of it is 1848, or earlier, its style and method being those of that period, and its determinative elements having been fixed not long after that date, its reasonings must be praised for their strictness, in comparison with those of metaphysicians generally. Indeed, had their author, instead of coming from the arena of political journalism to take up philosophy when the Coup d'État had shut his mouth to the socialism he had been talking, only received a sound training in any successful branch of scientific research, his native vigor of intellect would have shaped those reasonings to rise above the level of other metaphysical argumentations, and would have caused them to prove something--or, at any rate, to go towards proving something. He himself maintained that the fundamentals of his system were as perfectly demonstrated as the theorems of mathematics, if not more so. When, at last, he came to perceive that a senile ossification of his tissues had advanced so far as to leave him now but a few days more to live--a week or a fortnight, he imagined--with a tremendous effort he gathered all his forces in order to pour into the ear of his devoted disciple and bosom friend, M. Louis Prat, some last philosophical injunctions that to him seemed precious above rubies.

Beginning appropriately (though he may not have noticed the coincidence) on the feast of St. Augustine, August 28--it fell upon a Friday--he spoke continuously from one o'clock till three, and then after half an hour's intermission, for near two hours longer, M. Prat taking notes stenographic or other. On the morrow, a very long fore-noon discourse was supplemented by a shorter one before sunset. Sunday brought another lecture; Monday two--one in the afternoon, the other from nine to eleven in the evening. At 8:45 the next morning he expired. So undeniable was his earnestness. Apostle and martyr of the gospel of work, he was determined to expend his energy to its last grain in doing his duty. Another philosopher in his place might have thought that, since his doctrine was capable of demonstration, it must eventually be acknowledged, no matter with what contumely it was received at first, and that his business should be confined to presenting once for all a demonstration of it that any vaporous supplement could only mar; decency, indeed, forbidding that a priest of divine philosophy should put her into a position of mendicance for a lodging. But (singular scepticism!) this continuator of the great optimist, Leibniz, had no confidence in his own doctrine's ever coming to be generally received, for all its scientific and demonstrative truth. He had reckoned up the chances and found them adverse.

He hated to die; and in these talks--not "entretiens," by the way, since there were no interlocutors, nor any subject agreed upon at the outset--we cannot detect any marked falling off of intellectual powers as compared with the 'Essais de Critique Générale,' the 'Science de la Morale,' and 'La Nouvelle Monadologie,' to say nothing of last year's 'Le Personnalisme.' Being in such possession of his faculties, and in no great bodily pain as long as he kept still, it would have seemed unnatural if he had relished the idea of death. He said: "Je m'en vais. Il me semble que je glisse sur une pente, et je dois, par moments, faire un effort pour me retenir. C'est étrange! ce glissement dans l'inconnu a comme une espèce d'attrait pour moi." He was interested in his own interesting personality; and the little volume is far better worth reading for its human elements than for any utility to a scientific philosophy. It gives two portraits.

The volume of which Professor Dewey is the father forms a part of the University of Chicago's exhibit of an impressive decade's work, and is a worthy part of it, being the monument of what he has done in his own department. Here are eleven essays, four by himself, defining his conception of the business of the logician, seven by the students whom he has helped to form and set upon their own intellectual legs. It affords conclusive proof of the service he has rendered to these accomplished thinkers and, no doubt, to others; and they in their turn will render to another generation services of the same nature. Whatever there was to be gained by contact with a sincere student of philosophy, as such, they have manifestly gained. Are there any further services that logic could be expected to perform? Are any logical questions now being agitated in the different sciences? Is there any such question as to the constitution of matter, the value of mechanical hypotheses, now open in physics? Are there any methods as to more or less statistical methods of philological and historical criticism? If there are such questions, has past experience gone to show that there was any help to be had from broader sweeps of study than specialists can make? Is it worth while to examine at all into the questions here asked; and if it be, is it best to carry to them vague impressions, or the exactest conceptions that studies specially directed to them have been able to evoke?

There are specialists who are disposed to think any inquiries from the outside into their methods are impertinent. They say, with perfect justice, that they understand fully their own business, and wish to be let alone. Unquestionably, they must be right. There is, however, another class of specialists whose aims are of such a nature that they can sometimes make good use of ideas which have grown up in other studies. Such specialists, when they have created, say, physical chemistry, the new astronomy, physiological psychology, stylometry, etc., have sometimes gained a certain measure of esteem even from those of straiter sects. It has often happened that general studies of logic have resulted in such applications of one science to another. Analytical geometry was first conferred upon the human race as an illustrative example of the 'Discours de la Méthode.' The group of writers whom, abandoning all attempt at finding a descriptive, designation, we may roughly call the English school of logicians, meaning, for example, Boole, De Morgan, Whewell, J. S. Mill, Jevons, Venn, Pearson, MacColl, etc., while pursuing studies often purely theoretical, are nevertheless taking a road which may be expected to lead to results of high value for the positive sciences. Those whom we may as roughly call the German school of logicians, meaning such writers as Christoph Sigwart, Wundt, Schuppe, Benno Erdmann, Julius Bergmann, Giogau, Husserl, etc., are engaged upon problems which must be acknowledged to underlie the others, but attack them in a manner which the exact logicians regard as entirely irrelevant, because they make truth, which is a matter of fact, to be a matter of a way of thinking or even of linguistic expression. The Chicago school or group are manifestly in radical opposition to the exact logicians, and are not making any studies which anybody in his senses can expect, directly or indirectly, in any considerable degree, to influence twentieth-century science.

Prof. Dewey regards himself as radically opposed to the German school, and explains how he is so. We must confess that had he not put so much emphasis upon

it, we should hardly have deemed the point of difference so important; but we suppose he must know what his own affiliations are and are not. He seems to regard what he calls "logic" as a natural history of thought. If such a natural history can be worked out, it will undoubtedly form valuable knowledge; and with all our heart we wish the Chicago school godspeed in their enterprise of discovery. But their task will call for such extreme subtlety, precision, and definiteness of thought that we hope their new science will not disdain to take a lesson, if not from any of the older logicians of the country, nor from that American thinker who first essayed to use his great powers of observation to establish a natural history of mental products--we mean Dr. James Rush--at least from the well-established natural history of Nature, chemistry, botany and zoology; the lesson, to wit, that a natural history can hope to begin a successful course of discovery only from the day when it abandons altogether the trivial language of practical life, and sets up a thoroughly new glossary of words exclusively its own, thereby not confusing our meagre philosophical vocabulary with the burden of added meanings to old words. If calling the new natural history by the name of "logic" (a suspicious beginning) is to be a way of prejudging the question of whether or not there be a logic which is more than a mere natural history, inasmuch as it would pronounce one proceeding of thought to be sound and valid and another to be otherwise, then we should regard this appropriation of that name to be itself fresh confirmation of our opinion of the urgent need of such a normative science at this day.

79 (29 September 1904) 264-265: Outlines of Psychology.

By Josiah Royce. The Macmillan Co. 8vo, pp. 392.

CSP, identification: Haskell, Index to The Nation. See also: Burks, Bibliography; List of Articles; MS 1487 (draft).

An unpretending little volume, designed for a short text-book for students of sixteen and indefinitely upwards, the antipode of a cram book is this. It will not distend the mental stomach with bran, nor even impart much glow of repletion. Yet it will always interest every reader and often charm him. It will seduce a young person into close observation and close reasoning; and it will only be long after it has been laid aside that the learner will come to appreciate all the benefit he has derived from it. As an effective text-book it is necessarily an outline treatise, stating each point in explicit terms. Yet it is at the same time an essay in the sense of being animated throughout by a central idea, that of the essential unity of conduct and of cognition. We must not be understood to imply that it is a one-sided book, far less a book of one idea. It is a picture of the mind whose lights and shades are due to an evident centre of illumination.

The first result of this is that all the explanatory theories in it are such as physiological psychology propounds, albeit less than the usual proportion of space is given up to physiology. The strictness with which the author confines elucidation to that direction is marked, and, to our thinking, excessive. No doubt, that method is as thoroughly proved as can be; but that does not in itself in the least disprove

the method of explaining one state of mind as caused by another state of mind. It may be that Professor Royce would admit this, but that he deems it needless and confusing to insist upon ideas to which common sense clings so tenaciously. On the other hand, it may very well be that he is an automatist; for there is nothing in his idealism to forbid his being so. It will be as an automatist that his readers will mostly understand him.

The author's unification of conduct and cognition cuts deep into the theories of psychology. An instance of this, that will interest everybody on account of its practical bearings, is his proposal of a substitute for that division of mentality into Feeling, Volition, and Cognition, which was brought into vogue by Kant, and which has been so generally employed ever since, in spite of the derision that has been poured upon it by almost all the writers who have availed themselves of it. The fact that no other division has been proposed that has proved convenient in half so many relations, would seem to show that, crude as it is, there must be "something in it"--that there must be some truth of which it is an ungainly and ill-fitting vesture. In place of Cognition, Professor Royce puts the phenomena of forming habits, whether of cognition or of conduct, giving to this the name of "Docility." Another thinker more than twenty years ago used the same word, "Docility," for one of three departments of mind, and with the same signification, except for a detail that we shall mention presently. We believe that this feature, at any rate, of Professor Royce's division will soon commend itself to all psychologists.

For what is habit-taking? We can conceive, and do observe, three kinds of direct effects of causation. There are, first, transitory effects which come into existence with the causal action, disappearing the instant it ceases. Such are the accelerations of motion produced by forces. We do not say that power is expended in producing such effects, since by expenditure we mean a transfer of something, not an anihilation. An effect upon which power is expended may either remain after the cause is removed without any tendency to change, when we may say that the subject upon which the change is wrought is inert in respect to that effect; or else, when the power is expended, the effect may at once begin to undo itself, the subject of the change showing a tendency towards change of a definite kind, when we may say that it is elective with reference to such changes. Thus, the law of inertia may be expressed by saying that all bodies are inert in respect to their velocity. That is, if an expenditure of exertion imparts a certain velocity to a body, it will retain that velocity unchanged after the cause ceases to act. The law of the conservation of energy may be expressed by saying that all effects of physical force consist in bodies being inert in respect to velocity, but elective in respect to certain changes of relative position. Thus, if a missile were thrown up into a vacuum, then, as soon as the energy of its upward motion was expended, the body would begin to return to earth, while, its horizontal displacement being a change of position in respect to which it is not elective, the energy of horizontal velocity remains in that form. To say that a nerve-track is capable of habit is to say that it is inert in respect to changes wrought in it by the passage of a nervous discharge along it. A physicist will hardly hesitate to believe that this is due to particles of the nerve being displaced by the passage in ways to which they are inert. If so, the phenomenon closely resembles that of a stream of water wearing itself a channel--a faculty on which every skilled ditcher relies.

Simpler things take habits--that is, behave as they have before behaved. On a cold winter morning the sheet of ice on the countryman's ewer that offers considerable resistance to his first poking a stick to the bottom, will offer hardly any to a second poke, and none at all to a third. Any new behavior of any object whatever must be due to some new condition of that object; and ordinarily this will persist and cause the new behavior to be repeated. Thus, that which distinguishes the watercourse, and still more the nerve, is not that it acts as it has acted, but on the contrary is these two festures: first, that the number of particles whose displacement will facilitate the current is very great, so that many repetitions of the action continue to become easier and easier, and thus the habit is slow in its development, instead of being fully formed from the beginning; and, secondly, that in the case of the nerve the new positions of the displaced particles appear to have at first little stability, so that new habits are apt to be easily destroyed. The earlier user of the term Docility included under it not only the tendency to the formation of habits, but also the liability of habits to get broken up, which is certainly a most important characteristic of the intellectual man. It is probably his physiological point of view which prevents Professor Royce from once mentioning the consequences of surprise at the unexpected and the counterexpected, where an apparent rupture in Nature's habit produces a real rupture of our associations.

For the Feeling and Volition of the old triad, Prof. Royce would substitute respectively "Sensitiveness" and "Initiative"; and here we cannot think his proposals happy. "Initiative," according to his own analysis of it, consists simply in motiveless persistency in repeating an action. But this differs from Docility only as acting from a habit differs from forming a habit. Now, acting from a habit is just like acting from any other disposition, so that "Initiative" is nothing but Docility plus Volition. The author's unification of conduct and cognition should, however, have led him to regard volition and sensation, or the action of sensitiveness, as simply two species of one genus of mentality, that of reaction. For there can be little doubt that sensations react upon the stimulating causes as we know that volitions react upon the willer.

If we once accept docility and reaction as two categories of mentality, we shall forthwith be obliged to recognize a third. For, as is generally admitted, all formations of habit take place entirely outside of consciousness. We can only know that we have formed a habit by some experiment, although it may be an involuntary experiment or may be an experiment in the imagination. What we call pleasures and pains are nothing but sensations of any kind that are significant to us of our having favorable or unfavorable dispositions, of one or another of several kinds; and therefore when a sensation acquires a pleasurable or painful cast, or both, which it had not before, that is a sign of our having formed a habit. But, then, this is a case of becoming aware of a habit by experiment. Sensation and volition take place upon the very edge of consciousness. We have no direct knowledge of them as processes; we know them only be comparing what went before with what comes after.

This is generally acknowledged by psychologists in reference to volition. If there were any kind of sensation which we might expect to take in the act, it would

be the case of the emergence of an idea into consciousness by a suggestion addressed to an association. But we cannot catch even this in the process of emerging. To a man standing between the rails of a track on which a locomotive is approaching, it is successive sudden enlargements that he perceives. The sense of continuous change is an affair of quasi-inference. Thus we have no immediate consciousness either of facts of docility or of facts of reaction. Since, then, we are conscious, we must admit immediate consciousness, or feeling, as a third category of mentality.

What we have said must be taken as a small sample of the stimuli to reflection that are richly strewn upon these pages. We regret that we should have left ourselves no space to illustrate the skill with which the reader of them will be taught to draw practical corollaries from the truths of the new science.

79 (17 November 1904) 396: NOTES

CSP, identification: MS L 159.265. See also: Fisch, First Supplement.

Longmans, Green & Co. publish 'An Attempt towards a Chemical Conception of the Ether,' by D. Mendeléeff, translated from the Russian by George Kamensky. There is nothing new in the idea that the ether may be a chemical body. Mendeléeff's turn of mind would naturally lead him to favor this view; but it cannot be denied that a review of the history of scientific hypotheses will show that it has been conceptions of this character--the realistic character--in favor of which experiment has usually decided. It is an interesting fact, too, that a man of Mendeléeff's surpassing sagacity should be so decided as he appears to be that, if the ether is a chemical body, it is an unmixed element of the helium-argon group. It has long been as good as known that coronium, whose spectrum was so magnificent in the eclipse of August 7, 1869, is a chemical element, considerably lighter than hydrogen. Mendeléeff says its atomic weight "will be not greater than 0.4, and probably less." This would make its density one-fifth that of hydrogen or one-seventieth of the air's. He makes it similar to neon, which, with little reason, he separates slightly from helium, argon, krypton, and xenon--that is, he separates it as much as sodium is separated from lithium, potassium, rubidium, and cæsium.

79 (17 November 1904) 396: NOTES

CSP, identification: MS L 159.265. See also: Burks, Bibliography.

J. Clark Murray's 'Introduction to Psychology' (Boston: Little, Brown & Co.) is simply our old favorite, the same author's well-known and approved 'Handbook,' in a new dress. The author thinks that so many parts have been so completely rewritten that "it would have been misleading to describe it as a new edition of the old work." Many will hold quite the reverse opinion. We do not remark much retraction, and the enlargements amount, we should judge, to some 6 per cent. of the contents; certainly not to the double of that. The changes appear to be everywhere

judicious, and in half a dozen cases, at most, important; but the original dress of the book was far more gracful and more likely to prove engaging to young persons. The new 'Introduction' is, however, welcome.

79 (17 November 1904) 396: NOTES

CSP, identification: same as preceding note.

Prof. Florian Cajori's 'Introduction to the Modern Theory of Equations' (Macmillan) demands mention as the only small treatise in the language embracing the whole subject and showing how best to treat higher algebraic equations. The account of the Galvis theory is avowedly taken from Weber's algebra, but is for the first time brought within the comprehension of every student of middling capacity, by numerous examples. If the student goes through these and makes sure of perfectly familiarizing himself with every aspect of one point before going on to the next, and not fatiguing himself, we see no reason for his finding this doctrine, so useful as it often is, and so sure of enlarging the mind, at all beyond his grasp. We regret that the limits of the roots have not been more liberally treated.

79 (17 November 1904) 402-403: Experimental Psychology and its Bearing upon Culture.

By George Malcolm Stratton. The Macmillan Co. 8vo, pp. 331.

CSP, identification: Haskell, Index to The Nation. See also: Burks, Bibliography; List of Articles; MS 1488 (draft).

George Malcolm Stratton was a pioneer in experimental psychology dealing with sense perception. He was graduated A.B. from the University of California in 1888 and A.M. from Yale in 1890. For one year, Stratton held a post as instructor of philosophy at the University of California and then studied for three years under Wilhelm Wundt, graduating Ph.D. in 1896 from the University of Leipzig. In 1905, Stratton was appointed professor of experimental psychology at Johns Hopkins University.

This book, scientifically of no great importance, is charmingly written, and is as thoroughly sound as it is popular. We are here introduced to the science of experimental psychology nearly as it appears to the researcher himself. We remark in the new science the failure of the extravagant hopes of forty years ago, without at all acknowledging that its present state bids us despair of greater things yet to come with profounder methods of reasoning, and without depreciating the few truths of real importance that have been ascertained. Perhaps Professor Stratton makes his subject appear less scientific than it really is by being a bit too fond of discussions. We will not say that he sets up absolute men of straw in order to afford his readers the instructive exercise of knocking them down; but it does seem to us that he has now and then withheld decisive silencers of sundry flimsy arguments for the sake of considering them from several points of view. For example, the question whether mental phenomena are susceptible of measurement, to which a whole

chapter is devoted, could have been settled at once by the remark that the logico-mathematicians have demonstrated (as a moment's attention to Clifford's 'Analytical Metrics' would show) that number never can signify anything but relative order of succession.

Suppose there were no solid bodies in the world, but everything was liquid: that would not involve any alteration of the properties of pure space. Or suppose that the law of the displacement of bodies we called "rigid" were so different from what it is that while all parts of them that lay along straight lines should always remain in straight lines, yet the lengths, breadths, and thicknesses should be different according to their positions in space. To suppose that is not to suppose that pure space would be anywise different from the space of actual fact; the bodies in it would alone have different properties from those they actually have; and our present designations of lengths would have whatever truth to pure space they now have. But we should then, no doubt, make all our measurements with such "rigid" standards as we could then procure; and such measurement (so long as it was self-consistent) would be just as true to pure space as ours is. The truth is, that the relation of more and less is one thing, and a system of quantity quite another, and that quantity does not belong to space in itself, or to any continuum in itself, but belongs to one continuum (say our yard-stick) in its changing relations to another. Measurement has all the truth that it belongs to its nature to have if it represents the order of succession of parts, the "connectivity," as mathematicians say.

For example, when Ptolemy marked six grades of brightness among stars and numbered them successively, it was a mathematical necessity that those numbers and the fractional divisions of their intervals should be capable of being employed in useful computations (by least squares, for instance), since they, at all events, expressed the betweennesses of the different amounts of light received from the stars, and Ptolemy's values necessarily must be some mathematical function of the amount of light. Those star-magnitudes had come to be traditional among astronomers, who had inserted intermediate "tenths of magnitudes" when photometry was first successfully applied to the light of the stars by Seidel. It then turned out that not only was there in fact such a law of connection between the values of the star-magnitudes and the quantities of light (as there necessarily must be), but, furthermore, that this law was no other than Fechner's psychophysical law. Ptolemy had made the differences between successive magnitudes such as appeared to him equal (though these differences were so large that he subdivided them, ultimately); and that mode of estimating the quantity of light-sensation was in sufficient accord with the principle that a given ratio of physical brightness between two stars gives the same difference of psychological brightness whether on a very clear night, when the stars shine bright, or on a night when they are not nearly as bright. The "photometric magnitudes" of the stars now universally employed by astronomers do not essentially differ from the magnitudes of Ptolemy, except for a small modification made for the sake of facility of computation.

Since we are always able, subject to more or less uncertainty, to arrange feelings in orders of succession, it follows that they are capable of being measured, with probable errors smaller or greater, such as belong to all measurements.

In like manner, the question of the nature of "subconscious" thought, which occupies two chapters, and concerning which much inconsequential talk is retailed, could at once have been put upon a scientific footing by first establishing certain incontrovertible logical principles resting on mathematics, which show exactly what ought to be our attitude in view of the given facts--an attitude substantially the very same that Professor Stratton adopts, but which his discursive treatment may lead readers to deem less scientific than it really is.

The author naturally makes those parts of his science the most interesting in which he has himself made important advances; as, for example, in regard to the enjoyment of sensations. The contrast between the graceful form of a Grecian vase and the horrid crooked path of the eye as it contemplates that form is curious indeed. But to a catalogue of the volume's interesting points there would scarce be an end.

79 (1 December 1904) 432-434: THE NATIONAL ACADEMY IN NEW YORK

CSP, identification: Haskell, Index to The Nation. See also: Burks, Bibliography; List of Articles; MS L 159.265; MSS 1488a, 1488a (s) (drafts).

NEW YORK, November 23, 1904.

The meeting of the National Academy of Sciences last week was not as well attended as one might suppose that a session at Columbia University would be. The hall is one in which few speakers can make themselves heard even in the front row. The programme was not long; a large proportion of the papers were "read by title," and nearly half the rest were by men not in the Academy. Such as were read were hurried through--naturally enough, under the circumstances--although some of them would have been of unusual interest, could they have been listened to with ease.

The scientific session was opened by Dr. Louis Agricola Bauer, one of the savants of the Coast and Geodetic Survey, and one of the very few masters of all that is now known concerning the earth's magnetism. He was introduced to the Academy by Professor Woodward. In a memoir of the good old solid sort that has given the Survey its renown, he explained an analysis of the forces which cause the secular variation of the earth's magnetism. The results, which seem to be irrefragably proved, are of a kind to stimulate curiosity not a little. How the earth ever came to be magnetized, we do not know. It seems that although the earth is in a certain measure an electromagnet, yet it is mainly a permanent magnet; and in the remote past something must have occurred to render it so. Dr. Bauer expressed a confident hope that the series of investigations which he is conducting with the support of the Carnegie Insititution will afford evidence concerning the when and how of that event. Certainly, much the larger part of the forces causing the slow westward drift of the magnetic lines was shown by him to act within the earth's surface. Several unexpected features of these forces were pointed out; but the most startling of them is that for the last three hundred years, at least--that is, for as

far back as the history reaches--the earth's magnetism has been steadily diminishing at such a rate that, if something does not check the drain (and no cause for such check is known), we shall wake up some fine morning in A. D. 3500, or thereabouts, to find that the magnetism of the earth has all been spent and gone. Dr. Bauer seemed to the reporter rather concerned lest the newspapers should give a sensational turn to this result. He declared tht he did not believe it would ever really come to that; but one does not quite see what a geophysicist has to do with any emotional aspect of his results, one way or the other. We know that the earth is cooling. It is possible that something may occur to restore its heat, just as it is possible that something may occur to restore its magnetism. But its cooling is positive fact, while any restoration of its heat is pure fancy, with which the physicist, as such, has nothing to do. It is not easy to see how the case of magnetism is essentially different.

A paper of great elegance, presented with admirable perspicuity so as to engage the interest of every hearer, and, though involving no new principle of science, yet important in more than one way and especially as a precious augmentation of the resources of physical experiment, was by the well-known Professor Pupin, who was introduced by Professor Woodward. The object was to show how to produce impulses at equal intervals of any desired length, in the neighborhood (say) of a hundred-millionth of a second. The method is founded upon the use of that sort of electrical conductor of Professor Pupin's invention in which there are many small coils of wire, each terminating at either end upon plates, a plate of one coil being separated a little from a parallel plate of the next coil, so as to bring all the coils into a linear series interrupted between every two coils. Such a conductior may be constructed so that a current will pass through it in any desired interval of time (within limits); and the rate of propagation is absolutely uniform from coil to coil, as Professor Pupin demonstrated. Supposing, then, a chain of a hundred coils is passed over by the current in a millionth of a second, the interval between the passages to consecutive coils must be one hundred-millionth of a second, precisely. Now Professor Pupin proposes to attach secondary coils to each of the coils of the chain, in each of which an impulse will be produced by the entrance of the current into its primary coil, and these impulses can be utilized in any way desired. The mere ascertainment that one could thus, actually produce distinct impulses at certain excessively short intervals, even if no limit were reached, would be of interest; and it might happen that there should be found to be an absolute limit, which would be a matter of still greater interest. At present, some metaphysicians, like Kant, believe that there are no instants in time unless some instants are marked by some event, while the majority of those who have considered the subject believe that every interval of time consists of an infinite multitude of instants as distinct as so many grains of sand, although no two are consecutive. But some of the most accurate thinkers in the world, such as Mr. F. W. Frankland of Wellington, New Zealand, son of the great chemist, are of opinion that in a finite lapse of time the multitude of instants is finite. Those thinkers may be right; their arguments have not been absolutely refuted. There is, therefore, a possibility that there is some limit to the subdivision of time.

Prof. C. S. Hastings of Yale gave measures and calculations showing for the first time the dispersive power of the human eye. Helmholtz assumed that it was about the same as that of water, but Professor Hastings showed that it was intermediate between the dispersivities of flint and crown glass, and about that of oil of turpentine.

Two interesting biological papers were, the one by Dr. T. H. Morgan, who was introduced by Prof. E. B. Wilson, the other by Professor Wilson himself. It was not the only interesting paper by Professor Wilson, but these two, given consecutively, dealt in part with the same subject, and reached conclusions which, in expression at any rate, were somewhat conflicting. Dr. Morgan's paper related to organic polarity. He showed that, from individuals of all branches of the animal kingdom, it is possible to cut off pieces and have new parts grow out to replace the loss. Thus, taking a certain species of worm, if the individual was cut in two anywhere behind a certain point, a new tail would be grown upon the part containing the head, while if the section was made forward of that point, a new head would be grown upon the tail-piece. Not only that, but, after the former section, the piece of tail cut off would grow a reversed tail upon its forward end, and, after the latter section, the head-piece would grow a sort of reversed head upon its posterior wound. He showed that something similar followed when a limb of a suitable animal was servered in two places and the middle piece was replaced in a reversed position. That is, the foot, or whatever the terminal member was, would be reproduced equally from either end of a piece of the limb. Dr. Morgan had further made longitudinal sections of certain animals with results conforming to the same rule. But no attempt can here be made to explain the formulation of certain German biologists as to the propriety of which the two learned gentlemen were in disagreement. It did not seem to be a very deep separation of opinion.

Professor Wilson further read a paper by one of his students, Mr. Yatsu, about centrosomes. Everybody who interests himself in any branch of natural science has heard of the famed experiments in which Professor Wilson produced these evidences of the power of cell division by subjecting the cells to osmotic pressure. An objection was raised to the conclusiveness of that experiment on ground that the centrosomes which Professor Wilson made to appear might have existed already unseen in the cell. But since that would be the case only if the egg had been fertilized, Professor Wilson has ever since, he told the Academy, been engaged in a quest for a female of the species used containing a ripe egg. His luck, however, was bad, and that of his student, Yatsu, was good; so that it was to the latter that the lot fell. The cell contained the usual sole centrosome beside the nucleus, and, at Professor Wilson's suggestion, was most skilfully cut in halves by Mr. Yatsu so as to leave centrosome and nucleus in one half. This cell, then, could not have been fertilized in the usual way; but, upon being subjected to higher osmotic pressure, there soon appeared many little stellated bodies in it recognizable as centrosomes, and thus the discovery of Professor Wilson is freed from its last blemish. Professor Wilson's style of presenting his papers is simple and elegant in a very high degree. His voice, of penetrating depth, is so soft as not to be heard in the resound, while his enunciation is so perfect that he can be heard when nobody else can be heard. The only complaint that carping envy could conjure up would be that he is so cool; and this last infirmity disappeared when he told of the result of Yatsu's performance.

Mr. C. S. Peirce of the Academy occupied a good deal of time in sketching the contents of a memoir upon Topical Geometry. Topical Geometry is that kind of geometry which considers the motions, not of rigid bodies as elementary geometry and, indeed, metrics generally do, nor yet those of the shadows and rays of light of projective geometry, or graphics (which are such that if they are straight or flat at any one instant they are so at all instants), but of fluid objects which can bend and twist and, without being elastic, can be contracted or expanded in whole or in part in any desired way, the only restriction being that they shall not be ruptured or welded unless at specially designated instants in determinately described ways. Mr. Peirce remarked that this condition of preserving the connection of parts belongs to vacuous space itself, while it is demonstrable that these properties of space which are investigated by metrics and by graphics have nothing corresponding to them in vacuous space itself. Accordingly, Topics, or topical geometry, is alone the science of space itself, and all graphics, and à fortiori all metrics, can be regarded as a special problem of topical geometry. The most important part of the little known-about Topics is due to Listing, who first distinctly conceived the chorisy, cyclosy, periphraxy, and apeiry--that is, the numbers respectively of separate pieces, of rings, of sacs, and of solid regions that cannot shrink to nothing--and who first gave the census-theorem to which these four numbers ("Listing's numbers") are subject.

Mr. Peirce, in addition to revising Listing's work, has added the conception of topical singularities, or places within places, from which former places bodies can, while remianing [[sic]] in the latter places, move away in fewer or more numerous ways than from any other places in their neighborhood. He has enumerated all the singularities that can exist of three-dimensional spaces, and has given rules from which all problems of map-coloring can, for the first time, be demonstrated and readily solved. He further professes to demonstrate that not a single one of the proper theorems of Topics (that is, none that is more than a property of a lattice) can be demonstrated without virtually assuming that space is not only continuous in such a sense that rational numbers do not suffice for discriminating every point from all others, but that it is continuous in such a sense that every description of any kind is either inapplicable to any non-singular point, or else is such that, exclusive of any collection of such points that may be indicated to which the description applies, there is another and greater describable collection of non-singular points to which it equally applies; so that it is correct to say, with Herz, the correspondent of Kant, there there are no points upon a line until they are in some way marked; and indeed there is no multitude of points that could be marked without leaving room for a greater multitude to be marked. In order to establish this proposition, Mr. Peirce gives a completed doctrine of multitude which solves demonstratively the vexed question whether two collections can each be greater than the other. The memoir was said to solve a number of other problems.

Professor Crittenden gave a sort of appendix to that memoir which so deeply impressed the Academy at the Washington meeting. It was merely to the effect that a small amount of urate might be formed by low proteid metabolism. This slightly

detracted from the force of his former argument, illustrating the tediousness of the process of settling questions in physiology by courses of experimentation. The methods of treating such problems by comparing opinions and narrating cases among physicians of great experience not only has the advantage of bringing men into notice, but, after fewer hours of debate than the years the experimentation would have consumed the whole question is finally settled in the mind of every person who leaves the hall, just as it had been when he came in.

Prof. W. K. Brooks gave a paper on the pelagiac tunicates, convincing everybody that the two kinds of barrel-shaped animals with muscles round the barrel were not widely remote, the completeness of the hoops in one case against their incompleteness in the other not being an invariable distinction, and two other supposed essential differences being only a difference in the degrees of development of certain parts. Dr. Franz Boas, the distinguished anthropologist, treated of psychic associations in primitive culture, making some interesting remarks that nobody who heard them is likely to forget, although they may have seemed matters of common knowledge. Professor Woodward communicated a brief account of a paper by Mr. C. E. Mendenhall, son of the Academician of that name, concerning the determination of the absolute value of gravity by means of a pendulum in the form of a ring suspended from sixteen different points. The value of the method could not be judged without long and minute study. Professor Woodward described a pendulum of his own invention, in the form of a horizontal bar suspended by long steel ribbons without knife edges. Since this apparatus presents no problems that have not been completely worked out, it is certain that it would be an excellent way of determining gravity. The chief difficulty would be to ascertain the temperature; for unless the suspending ribbons are very long, the advantages of this form will not be secured. Professor Chandler performed before the Academy a determination of the oxygen in the air of the Subway by Hempel's method. It is a very elegant method, and was very beautifully executed by a student of Professor Chandler.

79 (8 December 1904) 460: NOTES

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

The seventy-two laboratory exercises contained in Prof. J. C. Olsen's 'Textbook of Quantitative Chemical Analysis' (D. Van Nostrand Co.) appear to us to be judiciously chosen and admirably described, and altogether to be calculated to make a skilful analysist of the student. So much of the other matter as is naturally wanted along with the exercises, to finish information about quantitative analysis that is indispensable to the young man entering upon it and acquiring his first skill, is also good. But whether, over and above that, it was worth while in a single volume to penetrate further into the vast mass of details, or to undertake more than to direct the student to the different books and papers, with hints as to the use of them, is a question not easy to answer to one's own satisfaction.

1905

80 (5 January 1905) 18-19: The Preparation of the Child for Science.

By M. E. Boole. Oxford: Clarendon Press; New York: H. Frowde. 1904.

CSP, identification: Haskell, Index to The Nation. See also: Burks, Bibliography; List of Articles; MS 1489a (s) (draft).

Mrs. Boole's little books, taken as a whole, have a certain unity, but their value does not lie there; and what is true of the whole is true of any one. The present volume is decidedly the best of the series thus far. Information and salutary wisdom are to be drawn from it everywhere. The very dedication informs us of a fact interesting for the history of science in England, that Sir George Everest, on his return from India about 1829, inflamed the minds of Babbage and John F. W. Herschel with "certain ideas about the nature of man's relation to Unknown Truth which underlay all science in ancient Asia, and which he had learned from Brahman teachers." It would be curious to reperuse the books that Babbage, in 1830, and Herschel, in 1831, published about the general nature of science (a subject that had long been untouched in England) in the light of this information. Certainly, they two and Everest's son-in-law, Boole, are, as mathematicians, marked by their great predilection for what are called "symbolical" methods (that is, reasoning about operations as if they were things), to which English mathematicians generally, both before that day and since, have shown a marked aversion. The preface contains brief notices of ten writers whose thought pursued paths off the main lines of intellectual traffic. Two of them, Boole and Babbage, are famous; one, Nicolas Antoine Boulanger, once was so; Père Gratry is still read; and a fifth, Ramchundra, received aid from the British Government in his mathematical researches. The others, Thomas Wedgwood (who made a study of Genius), James Hinton (author of 'Life in Nature,' etc.), Dr. Charles Winslow (author of 'Force and Nature'), "the late Dr. Wiltshire," and Benjamin Betts, never attracted much attention, but would seem to be worth some acquaintance.

The purpose of this little volume is to offer "suggestions as to means by which the scientific condition of mind can be induced" in children. The desirability of doing this is a topic distinctly excluded. In the first chapter, the scientific mind is portrayed, slightly, but with a rare fidelity to nature. "Scientific culture is the result of a steady, life-long habit of friendly and intimate, though reverent, intercourse with the Eternally Infinite Unknown." This might have been better expressed; yet, taken as it is, of many a man of science (especially of a passing generation) who might think the likeness execrable it is more true than he himself knows. "The typically scientific mind," says the authoress at the beginning of the chapter, "may be described as one which stands in a definite relation to As-Yet-Unknown Truth, and especially to that portion of As-Yet-Unknown which is just below the horizon of knowledge"; and she goes on to explain of what nature this relation is, laying much stress "upon the rhythmic alternation of attitude" of such a mind toward phenomena.

Rudiments of all the scientific features begin to appear in the mind of every child, in one more strongly, in another less so. It is possible, however, to extirpate them. "That delicate sensitiveness to the touch of the illogical, to the limits of knowledge, and to the Presence of the As-Yet-Unknown . . . is often destroyed in the human brain by rough-and-ready processes, adopted sometimes for the purpose of fixing the opinions of young people, sometimes for that of enabling them to pass examinations successfully in subjects which they do not understand." When it first dawned upon "the advanced section of educationalists" that the rules of Latin grammar are not sufficient aliment for the mind, the first step was to substitute facts of natural science regarded as dead truth, just as the rules of grammar were regarded. Next, when it was forced upon the attention of the advanced that the scientific truth of one generation does not altogether accord with that of the next, "they substituted up-to-dateness, instead of endeavoring to induce the habit of true scientific method." Mrs. Boole herself embraces an "eternal truth" of mental pulsation, which she otherwise phrases as "alternation of opposites." Perhaps the scientific mind may alternate as to the truth of this doctrine.

We continue culling specimens of the volume's contents. The authoress protests that the spirit of inquisitive destructiveness brings more poison than pabulum to the scientific character, and, being naturally excessive in the child, ought to be restrained rather than stimulated. She thinks that "a good deal might be done by teaching children, when they see a flower, not to touch it till they have learned all they can of its poise and mode of growth, so as to be able, after dissecting it, to reconstruct in their minds an accurate picture of how it looked before they disturbed it." One of the points that must receive sedulous care from the earliest lessons of the child is that the line of demarcation shall be clearly preserved between what he has experienced and what he has learned at second hand. To this end, children must be drilled in the power of reproducing exactly what it is that one or another person has said; and from this point of view no study is more wholesome that that of Latin. "Give no more time to science than you can afford to let the children spend in the really scientific manner."

Chapter II. is about the unconscious mind, and particularly about those intervals when the current of thought almost or altogether comes to a standstill, so that the ideas that float in it have time to settle and to compact themselves. "We are sterile for lack of repose far more than from lack of work." It is curious and painful to observe how many things have been proposed by true educationalists simply for the purpose of ministering to the action of the unconscious mind, and afterwards perverted, by persons possessed with the teaching mania, to the purpose of stuffing into children's minds some idea which is in the teacher's mind." Let children alone to their own thoughts or absence of thoughts during a good part of their time. Let them bother a carpenter, and pick up what skill they can; and on no account pay the man for the loss of time they occasion, for then he will feel obliged to show them how to work.

Chapter III., on hygienic sequence in development, has much to say of the same color. Chapter IV., on mathematical imagination, deals with a subject of the utmost importance in education. The last chapter, on ethical and logical preparation,

is perhaps the very best and most practical in the book; but we believe our readers now have some idea of what to anticipate from the reading of this and of the whole book.

80 (26 January 1905) 71-72: ROYCE'S SPENCER

Herbert Spencer: An Estimate and Review.

By Josiah Royce. Together with a Chapter of Personal Reminiscences, by James Collier. Fox, Duffield & Co. 1904.

CSP, identification: Haskell, Index to The Nation. See also: Burks, Bibliography; List of Articles; MS 1490a(s) (draft).

Exactly one-half this volume is occupied by Professor Royce's estimate of Spencer. Such a review by so very eminent a philosopher of one of his elder contemporaries should be of great and permanent interest. It will not, however, add to its author's reputation. Five different meanings of the term "evolutionist" are given, and yet a very important meaning is omitted--that in which Aristotle was an evolutionist, since he certainly based his central conception on the idea of a plant coming up from seed, or upon something of the sort. Aristotle is represented by Professor Royce as one of the two great anti-evolutionists of Greece, in some sense hard to understand from the few sentences he devotes to this matter. But doubtless many of Professor Royce's readers will agree with Zeller that, from the passages which the latter cites in Aristotle, the continued evolution of higher perfection "erhellt." To some of us it appears to be chiefest of the differences between the historical Aristotle and the imaginary Aristotle of the scholastics that the former makes the form to grow out of the matter, and continually to increase in perfection in the passage through the vegetable and animal kingdoms to man. However, Professor Royce has earned so much credit for accuracy that we cannot doubt that in this difficult matter he has chapter and verse ready for citation.

Having sketched the general history of evolution in bold and strong lines, he reviews the origin and significance of Spencer's own view of evolution. This Englishman's extraordinary innocence regarding every sort of nexus between his own philosophy and that of any remote period or foreign country receives due notice, as well as his failure to regard philosphical thought as itself an evolutionary process in which his own thinking had an organic place. His unemotional, direct, plain, and simple mental build is very well described, and brings up to our minds the picture of a common American balloon-frame house with the conventional gable and ell, with its own reasons fro being as it is and not the slightest suspicion of any reason for being otherwise. A great deal of attention is bestowed upon Spencer's invalidism, which is attributed in large part to eye-strain, in accordance with the general theory of Dr. G. M. Gould.

Professor Royce next goes on to give his own restatement of Spencer's principle in these four propositions: (1) that if the parts of any large body are as nearly alike in any specific respect as they then can be, this homogeneity will be unstable; (2)

that the differentiating mass, as it ages, will react by its various structure upon the play of the external forces which impinge upon it; (3) that, as the body slowly integrates, the energies within and about it tend to assume an orderly character; and (4) that "evolution is the consolidation of a mass of matter, attended by a loss of some of the energy that this mass contained; while, as this consolidation takes place, both the matter concerned and the energy which it still retains pass from a state in which there is little firmness of structure, little orderliness of arrangement, little sharpness of contour, and much inner resemblance of part and part, to a state in which there is great firmness of structure, much orderliness of arrangement, much sharpness of contour, and much inner variety of part and part." Evolution being thus completed, the reverse process of dissolution begins.

Professor Royce passes to a crticism of Spencer in something less than four thousand words, or, say, two pages of the Nation. Here we remark the fairness and catholicity which might confidently have been anticipated. He finds Spencer's limitations to be "as obvious as it is unfair to make one's judgment of him dependent upon them." "The real question is, How far did he help people to understand evolution?" He ought not to be condemned because he undertook to conceive of evolution in mechanical terms. He would have been false to his just philosophical purpose had he conceived it otherwise."

The Spencerian will derive great comfort from the different attitudes of the idealist and the logical scientist toward his master. The fault which each finds with Spencer is a virtue in the eyes of the other. The latter objects that cosmology, because of its immense variety, cannot possibly be deduced as a consequence of a fixed law, such as that of the "persistence of force," which will not of itself suffice even to explain a steam-engine. To do this the second law of thermodynamics has to be invoked; and this law, as Maxwell first showed and as is now universally acknowledged, merely provides that nothing shall interfere with certain chance distributions; for an intelligent demon opening a door for molecules that happened to be moving with particularly high velocities in one way as well as for those moving with particularly low velocities in the other way, would produce the effect which this "law" denies. It thus has a character opposed to that of ordinary definite laws, since these provide that mere chance is not to have its way. The physicists further object that, so far as Spencer explains any phenomena of nature, he virtually bases his explanation on a principle quite independent of that of the "persistence of force," and, moreover, that many of his deductions are too vague to have any value as explanations, although they may be valuable as general descriptions of the course of nature. On the other hand, they admit that he did well in putting the emphasis he did upon the distinction between simple and compound evolution; the former describing histories such as that of a planet, and the latter, histories such as that of a plant or of a race of plants. These objections are familiar to all who have any acquaintance with the world of physical research. They are worth recalling, however, because their contrast with the objections of Professor Royce brings out the distinctive character of the idealistic views; and we may presume that Professor Royce intended to mark this contrast. He is very explicit in bracketing the two laws of thermodynamics as of precisely equal rank, the one determing [[sic]] the quantity, the other the direction, of change, and is equally explicit in praising Spencer for reducing all the trasformations of the physical universe to this single invariant type. Nor has he one word of fault to find with his deductions as being too vague. A reader who should know no other writing of Royce than this would think him substantially a Spencerian like Youmans; for only objection he makes is that simple and compound evolution ought not to be described as a single process. But the question whether Spencer does as a matter of fact describe them as a single process or as two processes would appear, to the Spencerian and to the physicist alike, to be little more than a question of words.

The third quarter of the volume is given to a criticism by Professor Royce of Spencer's educational theories, which, by the way, have no apparent connection with the doctrine of evolution. They are treated with much greater severity than is that doctrine, and the last paragraph of this part reads as follows: "Let us honor him for what he was. But let us be glad that he is not the trainer of our children."

The volume is brought to a close by some personal reminiscences of Spencer by Mr. James Collier, who was for nine years his secretary, and for ten his amanuensis. It is as good a personal portraiture as any we call to mind; not speaking, of course, of large books. It begins by saying that Spencer was no recluse, and telling where he might often be seen in London. The places mentioned do not include any at which he would be drawn into serious discussions; and though, besides the places mentioned, he could be found, for many years, almost every evening at the Athenæum, upon the committee of which he served, yet he did not join the conversation circle there, but played a certain number of games of billiards and went home to bed. It was only his sworn adherents who could see much of him. It was that vast work which so absorbed him that sometimes, having of his own motion brought about an interview, when the occasion came he found he must not talk. Yet, let an attack be made upon any position he had taken, and instantly upon hearing it read out he would be ready to dictate his reply, for two or three hours, without wishing to make any corrections. On such occasions, his grasp seemed Napoleonic. In short, he had converted himself into an apparatus for performing that one task, and he had no passions or intuitions which in any way deranged his adjustment to that.

That he certainly was a wonderful thinker in his peculiar way appears much more clearly now that his work is done. Mr. Collier says he never read any book of philosophy except Mansell's 'Prolegomena Logica,' and it is a great pity that he ever read that, because it was just that which introduced an element into his 'First Principles' which philosophical students then and always regarded as utterly refuted and out of date, and which did not harmonize with his original work. When one thinks that his 'Psychology' appeared in 1855, five years before Fechner's 'Psychophysik,' and simultaneously with Bain's first book, 'The Senses and the Intellect'--so inferior in originality and value, although it taught us more, because we were better prepared for it--one cannot but rank Spencer very high. He wrote when the ideas of energy were in the air, especially among engineers, with whom he had mingled much. But those conceptions were by no means répandues, as they now are. That he had grasped them in his own way, we need not say. His valuation of

Darwinism was from the first extraordinarily near to that of biologists of to-day. So it was with his estimate of the nebular hypothesis at a time when the objections to it appeared most redoubtable.

He did his work in his day, but the system of Synthetic Philosophy will never becom a classic. It will not be read forever, like Locke's 'Essay concerning Human Understanding,' Berkeley's 'Principles of Human Knowledge,' and Hume's 'Treatise of Human Nature.' In a few years it will have passed into history, along with Cudworth and Ockham--books that one wishes to know about, but to be excused from reading.

80 (2 February 1905) 100: The Becquerel Rays and the Properties of Radium.

By Hon. R. J. Strutt. London: Edward Arnold. 1904. New York: Longmans.

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

Robert John Strutt (1875-1947), fourth Baron Rayleigh, was educated at Eton and Trinity College, Cambridge. From 1908 until 1919, he was professor of physics at Imperial College of Science and Technology. Like his famous father before him, this Lord Rayleigh carried out research in a private laboratory, notably investigating the age of minerals and rocks by measurement of their radioactivity and helium content.

From the son of Lord Rayleigh one anticipates intellectual superiority--not necessarily individual force, but that superiority which comes from sitting often at table during many years with the leading physicists of Europe; and one is not disappointed in the event. The promise of his preface is only "to give as clear and simple an account of the phenomena of radioactivity as the subject admits of, without sacrificing accuracy," binding him to no more than compilation; but in generous fulfilment he gives us a most interesting discussion of all the questions that have been opened by the discernment and skill of Mme. Curie. Indeed, his only fault worth mention is that he has not realized that the majority of those who will attach a value to the volume would have been glad if, somewhere between its covers, somewhat fuller details could have been found. They are mostly quite able to read mathematics, be the motive for doing so sufficient; they would have liked references to the original papers; and they would have been glad to know that they had only to take down this book from their shelves to find, for example, the value of Mme. Curie's determination of the atomic weight of radium, and other minutiæ of that nature. It is a most interesting book, brimful of information and of thought; but it falls just a little short of the kind of perfection that an experienced bookwright would have imparted to it. The lacking matter we have spoken of might have been relegated to the appendices, of which there are three as it is, besides a direction to "see" an unembodied fourth. It will not be long before a new edition of this work is called for--or if there is not such a call it will be only because of this irrepletion. Let us hope there soon will be a second edition, and that the little conveniences we speak of will have been put in before the work is offered again to the public. Probably Mr. Strutt did not wish his volume to dispute the ground with Rutherford's 'Radioactivity.'

Beginning with an account of Crookes's exquisite experiments (for let us not forget that it was Crookes's surpassing genius that started the whole development), Mr. Strutt first treats of the cathode rays, and shows how it was a happy idea of Henri Becquerel's--albeit, most curiously, a totally mistaken one--that brought about the discovery of radioactivity. Mr. Strutt says this was a circumstance unparalleled in the history of science, and the remark (which, we doubt not, expresses not only his own impression, but that of a whole circle of the first physicists of the world) merits our attention as illustrating, in despite of Dr. Karl Pearson, what very great significance those men attach to successful prediction. The case was this: The cathode rays of Crookes's tubes produced a peculiar green phosphorescence in the glass where they struck upon it; and this led Becquerel, after Röntgen had discovered that from the outside of the glass at that point his wonderful X-rays were given off, to surmise that salts of uranium, which likewise phosphoresce with green light, might perhaps emit similar rays. To test his guess, he wrapped a photographic plate in black paper, and, having placed some uranium nitrate upon the paper, awaited results. Sure enough, after a few days, on developing the plate, he found a perfectly distinct impression of the crystals pictured there. Now the thing that Mr. Strutt, in common with physicists generally, finds so extraordinary and downright unparalleled is that, notwithstanding this successful issue of a quasi-prediction, it nevertheless turned out, as he says, (1) that the green fluorescence of the glass of Crookes's tubes has nothing to do with the production of Röntgen rays; (2) that the green fluorescence of uranium salts has no connection with their effect on photographic plates, and (3) that those uranium rays which affected the plates are of a radically different nature from the Röntgen rays which duplicate the effect. It was, indeed, a remarkable case, conveying the important lesson, not that prediction or quasi-prediction is not a vitally important factor of physical research, far less that it is unscientific or even anti-scientific, as Professor Pearson contends, but that one or two fulfilments of predictions do not suffice to prove that the hypothesis upon which they are based is so much as a recognizable likeness of the real truth. The experience of Becquerel was, it is true, not so unprecedented as Mr. Strutt represents it to have been. He goes too far in saying that the Röntgen rays, the uranium invisible rays, and the green phosphorescence "have nothing to do with" one another. It can hardly be doubted that there is some connection between them, although we cannot say what it is, and although the phenomena are certainly not directly allied. Mr. Strutt can surely have no difficulty in calling to mind many and many a chemical induction, virtually predictive, which went on swimmingly for a long time and then broke down so one-horse-shay-ly that the favorable instances seem to us little more than accidental coincidences. Probably, however, time will show that they were not so utterly fortuitous as they at present seem to have been.

Several of Mr. Strutt's positions in the logic of science are questionable; but whenever he has set forth his reasons they appear very strong and very interesting, even if not fully convincing. Against his mode of attacking the substantiality of matter and his argument in favor of electricity as the only subject of spatial motion, it is impossible to hold out. (We speak of his argument, which, however, neither is nor professes to be absolutely demonstrative.) His doctrine of the

transmutation of the elements is peculiar in making the course of development to proceed from elements of high atomic weight to elements of lower atomic weight. Moreover, he makes the transmutations run along the horizontal lines of Mendeléeff's table. The brilliant ingenuity exhibited by Curie, Ramsay, Rutherford, etc., in all these researches, and their astounding marvels of manipulative skill, are now an old story, perhaps; but, as narrated in Mr. Strütt's book, they appear more real and more fabulous than ever.

80 (16 March 1905) 198-199: An Introduction to the Theory of Optics.

By Prof. Arthur Schuster. London: Edward Arnold; New York: Longmans. 1904.

CSP, identification: Haskell, Index to The Nation. See also: Burks, Bibliography; List of Articles; MS 1490b(s) (draft).

Sir Arthur Schuster (1851-1934), though German born, achieved his greatest fame as a mathematical physicist in England. Schuster's work was mainly in the fields of spectroscopy, electricity in gases, terrestrial magnetism, optics, and the mathematical theory of periodicity. He was knighted in 1920.

As the title imports, this text-book, by a physicist of fairly high standing, relates, not to light in general, but only to the theory. The student is supposed to be acquainted already with the phenomena of light, so far as they are described in college books of general physics or "natural philosophy." Nor is this a regular treatise on the theory of light, but only an introduction to that theory of a peculiar sort. The first two-thirds of the volume are elementary; that is, they explain only polarization, interference, diffraction, the theory of optical instruments, and the peculiarities of the different crystalline media--phenomena that result simply from light's consisting of transverse vibrations. The remaining third of the book contains the deeper theory of light, and is written on a novel plan, the idea being to direct students to the original memoirs without repeating their contents, unless Dr. Schuster has some simplification or amplification of the matter of a memoir to bring forward. This spoils the book as a work of reference, but we think that all good physicists will highly approve of thus forcing upon students the habit of reading original memoirs.

There is another feature that cannot pass unnoticed. Probably a large part of our readers are aware that of late years there has been among physicists a split of no ordinary kind. How many approved writers, metaphysical analysts of high pretensions, have explained to us that there necessarily is a science of physics whose essence consist in its assumption that matter moves according to the principle of inertia, which Hoefding, for example (than whom no philosopher, perhaps, is more in vogue just now), calls, in his 'Outlines of Psychology,' "the fundamental assumption with which natural science comes into existence." Yet at this moment a growing moiety of the world of physics assumes this law to be only approximately true, and even that only for velocities not too great. The physicists of the nineteenth century took for the sole aim of their hypotheses the explanation of phenomena as special cases under the general laws of dynamics. The new school, however, proposes to explain dynamics as a special case under the general laws of electricity. The peculiarity of Schuster's position is that he wishes to treat the matter as a party question, and says that the new opinion must be "strenously resisted." Such language will tend to make us all hope the new doctrine may turn out true, as scientific propositions that excited "strenuous resistance" usually have turned out in the past. We note, by the way, that Schuster seems to regard the fact that Kirchhoff (in 1857) remarked that the velocity of electricity was about that of light, as in some measure detracting from the merits of Maxwell. Why, then, does he not mention that the electromagnetic theory of light was originally propounded by Faraday in 1846?

80 (16 March 1905) 218-219: Modern Practical Electricity.

By R. Mullineux Walmsley. Chicago: W. T. Keener & Co.

CSP, identification: Haskell, Index to The Nation. See also: Burks, Bibliography; Fisch and Haskell, Additions to Cohen's Bibliography; MSS 1491, 1491(s) (drafts).

Appearances are against these four thin royal octavo volumes. They display a kind of ornateness by which the stern reviewer, lover of the leaden casket, refuses to be charmed. It is an ill omen, too, that the title-page bears no date and does not testify that this is a second edition, while the worn appearance of a good deal of the type does. Such lack of frankness creates a prejudice, and has caused the book to receive at our hands a more searching examination and comparison with other treatises than would ordinarily be bestowed upon a popular exposition of an established science. However, the final verdict is that an intelligent man of business or profession, who, by way of preparation for twentieth-century contingencies, would like, without too severe study, to understand the scientific basis of the wonderful inventions which have been revolutionizing life, and to learn as much as he easily can of the relation between practice and theory in electrics--so as, for example, to comprehend in some measure why one great company employs continuous current dynamos and others alternators--such a man cannot find a book better suited to his wants than this. That delight with which a work of genius like Maxwell's 'Theory of Heat' thrills him who reads it is not to be looked for from these volumes, but our readers understand what is the sort of cultivation required for the conscious enjoyment of a fine talent well trained; and they will take pleasure in noting the deftness of Dr. Walmsley's expositions. Clearness of statement is a small part of it. There is an accomplishment by which one suggests the right idea at the right moment and in the right form, a finish of expositional tactics, for which the science of electricity affords an interesting field. That art is Dr. Walmsley's; and in sinking those parts of the theory that need not perplex the reader, such as the displacement current, in so arranging the different sections of the book and governing their contents that each difficulty that is to follow shall find the reader in the precise state of preparation needed to meet it, he at one point forced us to exclaim: This is verily the high strategy of perspicuity! Yet it moves so smoothly that the reader will deem it a matter of course unless he goes to the trouble of comparing this work with others.

Extreme devotion to perspicuity has in one place had its inevitable disadvantage--namely, after explaining in considerable detail the arrangement and shape of the lines of force from a charged conductor placed near an uncharged one, and how those lines move when one of the conductors is moved about, until the reader fully possesses it, the author supposes a chain thrown over the uncharged conductor so as to connect it with the earth, and correctly enough says that the ensuing motion of the ends of the lines of force, during this discharge, is an electric current. To have analyzed the phenomenon into two parts would have been a monstrous blunder of exposition. But the author abstains from pointing out that the so carefully described features of the lines of force, with their perpendicularity to and termination at the surfaces of conductors, refer exclusively to a state of equilibrium, and cannot be expected to persist when the lines move with luminal velocity. Doubtless his reason was that, if he did that, he would have to indicate how these features would necessarily be modified under rapid motion, which would bring complications mountains high. Yet, as he has left the passage, we will venture to predict that no small perplexity will be occasioned for the innocent who is at first led to believe that the displacements of the lines of force in moving an insulated conductor about in an electric field accurately represent an electric current.

The book is undoubtedly a second edition; but it must have been radically remodelled in some parts, for not only are all the latest discoveries treated, but early chapters present features for which there could have been no motive except to prepare the reader for a clear understanding of the new developments.

80 (23 March 1905) 228: THE SORROWS OF PHILOSOPHERS

TO THE EDITOR OF THE NATION:

SIR: Now that the subject of healing diseases by mental suggestion is being so much discussed, readers may be interested in a fragment of parallelism connected with the early history of the subject.

James Hinton's philosophy of the Art of Thinking was analogous to the special kind of mathematical philosophy of which a writer in the Nation (January 5 of this year) says that "English mathematicians generally" have shown "a marked aversion" to it, and that Babbage, Herschel, and Boole were "much addicted" to it. In consists, as the writer says, in treating mental "operations as if they were things." In the "fifties" of the last century, Hinton carried on a series of experiments suggested by Hahnemann, with a view to testing the truth of the homœopathic system of medicine. Results followed as predicted by Hahnemann. It occurred to Hinton to repeat the same series of experiments with plain sugar-of-milk; fixing his mind, when administering the pillules, on the symptoms which, according to Hahnemann, should have followed from the administration of a special drug. The new set of experiments proved as successful as the old. Hinton came to the conclusion that the results were due to the imagination of the patient, acted upon by silent suggestion

from the doctor. The story, so far, is told in his biography. The sequel, I believe, has never, till now, been alluded to in print.

If we examine the literature of Christian Science, or any other variety of "mental healing," we find the authors, for the most part, expending their energy on proving the one point, that disease is the product of false thinking, and can be cured by true thinking. They hardly touch on the inquiry, What is the normal order of the process of thinking? It is here that Hinton showed his peculiar genius for treating mental operations "as if they were things." Having come to the conclusion that the action of the doctor's mind on the patient's could affect the health and sensations of the latter as a drug does, he proceeded to deal with the newly discovered remedy, in all simplicity and good faith, on the same principles as an honorable physician deals with a new drug. A physician does not bind himself down beforehand to use one set of remedies exclusively and abjure others; he deals with each case as it arises, according to the best of his present judgment enlightened by past experience. James Hinton would never have consented to commit himself to use, in all cases, imagination and suggestion rather than iodine or sulphur.

He laid great stress on the principle that all remedial agents have reactions as well as direct actions. When a new drug is found capable of allaying unpleasant symptoms, the good physician does not therefore immediately feel himself justified in using it freely to get rid of those symptoms; he uses it with great reserve, in cases of illness, until every effort has been made to find out as much as possible about its nature and properties, and about its effects and after-effects, when taken by persons in ordinary health. Having discovered that the operation of framing mental postulates, hypotheses, opinions, imaginations, etc., can induce or can cure disease, Hinton devoted himself to the study of that operation as an entity in itself, on the same principles as he would have studied any anodyne or alterative drug.

The consequences much resembled those which followed my husband's mathematical analysis of the Laws of Thought. By the mere fact of analyzing the process of forming hypotheses, Hinton shed light on many departments of science of which he had only quite ordinary medical knowledge, such as morphology, embryology, physiology, and sociology; and even on mathematics, of which he knew hardly anything at all till he studied it as a branch of the Art of Thinking; much as George Boole, by his analysis of the Laws of Thought, shed light on obscure questions of mathematics, probabilities, electricity, chemistry, and economics. In each case, some section of the public welcomed enthusiastically such results of the investigation as proved directly useful or attractive (Hinton's little object-lessons on Morphology, published under the title, 'Life in Nature,' are exquisitely graceful and fascinating). But, in each case, the readers missed the true purpose of the writer, which was to present the process of thinking and show its normal course.

As it was my father, Thomas Everest, who had induced Hinton to try Hahnemann's experiments, I was naturally interested in just that part of Hinton's work which to most of his friends seemed uninteresting and unimportant. I sometimes felt that his most fervent disciples hated his philosophy "with the hate of hell," though they "loved passing well" the "beauty" of its results! His fate, in this respect, resembled that of my husband. Both were at times crushed by their failure as regards

the main object of their life-work. In my husband's case, the disappointment showed itself in fits of remorse about accepting medals and degrees granted for the external results of his thinking, by authorities who, he said, would shun him if they realized what his books were really about. In Hinton's case, the grief was more profound and continuous. It overshadowed all his later years and hastened his end. It made him, in a certain sense, ashamed of the artistic beauty and intellectual charm of his work, and at times, even of its very moral excellence, because all these things attracted attention away from the study of the Art of Thinking itself, which he, as well as George Boole, believed to be the true key to the physical and moral regeneration of mankind.

MARY EVEREST BOOLE.

LONDON, March 3, 1905

80 (23 March 1905) 231: NOTES

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

--In 'Les Grands Ecriviains Scientifiques' of Gaston Laurent (Armand Colin & Cie.), along with much that is familiar to all, everybody will find something new to him with which he will be glad to become acquainted, while some extracts there are which will be known to nobody, unless by accident. Notes accompanying the text, along with judicious geographical notices, go to compose a decidedly entertaining volume. We cannot make out exactly why these particular passages from these particular writers should have been selected, but it is easy to see that the reasons have been of the kind that usually prevail in France, where it never would do to allow the youth to suspect that any German could combine the characters of a man of science and a writer of distinction. So that accounts for the omission of Goethe. Nor will a strictly defined patriotism accord any hearing to Belgians, Swiss, Lombards, or Catalans. After appropriating a portion of the volume to foreigners--the English Harvey, Newton, and Darwin, the Italian Galileo, the Dutch Huygens, the German Kepler, and Leibniz (Copernicus being treated as a Pole, and Leibniz being chiefly a writer of French)--the rest is given over to eighteen authors pretty fairly distributed among the different provinces of France; but a volume at least as good as this might have been made up of extracts from strictly French scientific wits not here represented. We note an anecdote or two of Sainte-Beuve. The mathematician Bertrand reports that he always used to insist that the expression "Il est de nos amis" was bad grammar. Again: "Sainte-Beuve repoussait avec indignation cette maxime cynique que beaucoup d'honnêtes gens, comme s'ils se vantaient d'un devoir accompli, se disent fiers de pratiquer: 'Il faut toujours défendre ses amis.' Aimons nos amis, partageons leurs chagrins, réjouissons-nous de leurs succes, mais ne les défendons que quand ils ont raison, ne leur accordons, même en public, que les louanges qu'ils ont méritées. La vérité est, comme la justice, le droit et le profit de tous: à qui, dans certains cas, se vante de l'oublier, il serait bien sévère de ne pas pardonner, il n'est pas tolérable qu'on en fasse un mérite." This is a morality which needs to be combined with delicate judgment.

80 (30 March 1905) 244-256: The Phase Rule and its Applications.

By Alex. Findlay. With an Introduction on the Study of Physical Chemistry, by Sir William Ramsay. Longmans, Green & Co. 1904.

CSP, identification: Haskell, Index to The Nation. See also: Burks, Bibliography; List of Articles; MSS L 159.267-268; MSS 1492, 1492 (s) (drafts).

There are numbers of highly successful men who have never been able to master elementary geometry or algebra. In the case of many of them, the defect is undoubtedly merely due to bad teaching. For others, there may be some peculiarity of metrical ideas which prevents these men from mastering them. But, with a third class, the truth probably is that there is a weakness of the understanding which causes all conceptions of the slightest intricacy to become confused. Now every man ought to endeavor to understand his own capacities; and the subject of the Phase Rule may serve as a touchstone to show him whether he belongs to the third class or not.

But what is this phase rule that has for so long set all the chemists agog? Take a number of materials, each either a pure chemical substance of a homogeneous mixture, but none of them such that any chemical reaction that could take place could produce it from the others, and (calling these the components of your system) put them into a tight cylinder in which a piston works, so that the aggregate can be subjected to varying pressure and temperature. Further, let there be some means of varying the proportions of the components present in the cylinder. Then, since the total quantity is a matter of indifference, there will be, as independently variable conditions, one for each of the components minus one, plus the pressure and the temperature; that is, on the whole, one more than the number of components. Now give your system time to come to a settled state of equilibrium while there is no change of the above conditions. This equilibrium must be stable; that is, if you vary those conditions a little and restore them, the state of the system must be restored. Now count the number of phases, that is, the number of substances or homogeneous mixtures separated from one another by surfaces like the surface between water and air. Then it will be found that the degrees of freedom--that is, the number of conditions (pressure, temperature, and the proportions of components) that can be independently varied without altering the number of phases or destroying the equilibrium--are equal to two more than the number of components less the number of phases. For example, can water, ice, and aqueous vapor exist together? Here there is only one component, and we suppose three phases separated by surfaces. By the rule, the degrees of freedom amount to zero. That is, if the temperature and pressure are both fixed right, such a state of things is possible. But when it exists, neither pressure nor temperature can possibly be varied, the equilibrium being preserved, for the least change will cause either the vapor, the water, or the ice to pass away entirely. Suppose, however, that air be added, so that now there are two components, and, for phases, first, air mixed with a little vapor; second, water containing a little air in solution; third, pure ice. There will now, according to the rule, be one degree of freedom, and the pressure may be changed so long as the temperature is correspondingly changed. Suppose we let the ice melt. There will then be two components and two phases, and consequently by the rule two degrees of freedom. That is, both pressure and temperature may be varied, but only on condition that the proportions of water to air in the vapor and in the solution are correspondingly changed at the same time.

Such is the phase rule. Its mathematical form is identical with that of Euler's topical theorem that the combined number of faces and summits of any polyhedron is two more than the number of its edges; and, like topical geometry, the phase rule has nothing to do with measurement, but only with counting three sets of things--components, phases, and degrees of freedom. Nor do the experiments for verifying it require any measures to be made, but only that more and less should be discriminated. Beyond that, there is no mathematics in the subject; but the chemical applications of the rule have a certain moderate degree of intricacy, not much less than that of elementary geometry and algebra. They will be beyond the powers of those who are unable to master those mathematical subjects owing to general weakness of the understanding, but will presumably (not certainly) be found to be just complicated enough to afford entertainment for other men.

The phase rule was discovered not by a chemist, but by Willard Gibbs, a mathematician, the ablest, in his particular line, of his day; the most unaffectedly modest man (for his strength) that search could find; the sort of man who is sure to be depreciated until, by the action of a sort of phase rule, the ice breaks up and men suddenly discover that it is best to laud him. For ten years and more the phase rule slumbered peacefully in the Proceedings of the Connecticut Academy of Sciences until the chemists had grown up to it, till Roosebaum and others brushed the dust off the volumes and carried them into their laboratories. But Maxwell fully appreciated Gibbs long before the phase rule was discovered.

Dr. Findlay's illustrations from chemistry are as replete with interest as they are abundant, and each one detailed. Upon all that side the work is simply splendid. Its weakness is that the abstract definitions, and the possiblities which the phase rule leaves open, but which have not yet been met with in nature, are not forced upon the reader's attention in all their definiteness sufficiently to enable him to comprehend exactly what the phase rule does for him in each case. In short, the work presents a picture admirably rich in its coloring, but whose outlines are too soft for a scientific purpose--on rare occasions, quite indefinite or even wavering. The practicable remedy for this in a new edition would be an appendix to which the reader could always turn to find there the abstract parts of the subject defined with abstract precision. The reviewer must confess that, though the subject is not new to him, he has here and there in the perusal of the work found his ideas losing their definiteness; and this state of things was not remedied until he had drawn up for himself the sort of appendix that was needed. However, taking

the book as it is, anybody who adds to a turn for the mode of thought a fondness for chemistry, will find the volume replete with matters of singular interest.

Sir William Ramsay's introduction contains a very good sketch of the history of physical chemistry without any such high merit as one would have expected. The language is that of a man who, when he thinks, imagines apparatus and experiments, and who has little training in the use of words. Thus, on page lxi, he finds fault with Julius Thomsen because, in 1854, he believed it possible to measure the force of chemical affinity by measuring the heat evolved in reactions; and his objection is simply that heat is not force, but energy, a quantity of different dimensionality. Now this is hypercritical in the extreme; for though the word energy had been proposed by Young, it was not at all in use, even in England, before Rankine's paper of 1853, and in Denmark was quite unheard of; the word "force" being much more loosely used than it now is.

80 (27 April 1905) 327-328: THE NATIONAL ACADEMY OF SCIENCES

CSP, identification: Haskell, Index to The Nation. See also: Burks, Bibliography; List of Articles; MSS 1493, 1493(s) (drafts).

WASHINGTON, April 24, 1905.

Never before, we believe, since the National Academy of Sciences was created, in 1863, has a spring session brought so meagre a crop of papers as did that of last week. Meeting in Holy Week has not hitherto had such an effect, but this year there were only eight papers. The consolation was that those few were not hurried over as Academy papers usually are, but could be understood and enjoyed in full.

Dr. William Morris Davis, Professor of Geology in Harvard, opened the readings with a deductive sketch of the distinctive effects upon the conformation of the surface of any land-region of several modes of erosion. No other than erosive agencies were considered; and indeed the title of the paper, "The Geographical Cycle in an Arid Clime," would limit the subject still further. But, unfortunately, no lantern had been provided, and the paper probably had to be modified in its presentation. It was very interesting, however, as it was given. In an ordinary climate, the surface of the land would in the ultimate result get washed down to one lowest possible level, technically called the base-level. It usually coincides with the sea-level. Powell showed the importance of recognizing this tendency, since, under normal conditions, there is no other cause that can produce a terrestrial level surface. In arid regions there is no other erosive agency than that of the wind to mould the surface; and wind is far more efficient in such regions than where vegetation arrests it and makes a relative calm close to the ground. Air, of course, does not seek any particular level for itself and its freight, as water does. But its action, as figured by Professor Davis on the blackboard, reminds one of the process of grading a railway-bed by cutting and filling, the depressions being raised with the matter removed from the elevations; so that, like that operation,

it does at last leave a level surface--interrupted, it may be, by bare crags of rock. This level surface may perfectly well be below the sea-level, none other than which can usually be the last surface washing. Nevertheless, the wind may strike upon the base of an obstruction in such a manner as to dig holes. It was a matter of regret that a second paper by Professor Davis, which was announced on the printed list, did not get read.

Professor E. L. Nichols of Cornell gave a paper entitled "The Mechanical Equivalent of Light." Since ordinary text-books do not use this phrase, the reader may naturally ask what meaning can be attached to it. The photometric intensity of any one bright line or narrow band in a spectrum will vary in strict proportion to its heat-energy, and the value of the inverse ratio (the energy per brightness) will be the mechanical equivalent of the light of the wave-length of that band. But, for different parts of the visible spectrum, this inverse ratio will vary immeasurably, becoming infinite at the limits of the visible spectrum. We may add that the curve of variation will be appreciably different for different eyes, though they be normal eyes, and not a little for the same eye at different times of day, conditions of health, etc. Perhaps our many expressions about "everything looking blue" to a man in one mood, "bearing a roseate hue" in another mood, and so forth, though they are all now understood as purely metaphorical, may, some of them, have had their origin, before poetry was recognized as such, in nice observations of positive fact. At any rate, the mechanical equivalent of ordinary light capable of dispersion by a prism will have a different value for each illuminant, for each pattern of lamp or burner in which that illuminant is used, for each state of cleanliness of that lamp, for each eye, and for each state of the eye. But the usage of physicists restricts the phrase to the light of a little lamp introduced by Von Hefner-Alteneck burning "pear oil" (often insufficiently designated as "amyl-acetate") and having a flame of a certain fixed height. The photometric intensity of such a flame is called a hefner. It is about eight-ninths of a standard sperm candle-power. An ardent admirer of the marvellous precision of modern physical measurements was all ears and eyes when Prof. Nichols proposed to set down upon the blackboard the three principal determinations of the mechanical equivalent of light by Thomson, Tumlirz, and Ångström, and was evidently preparing himself to be astounded. When he learned that the three values of this important unit which the three eminent physicists had obtained were in the ratios of 19, 13, and 7, respectively, his surprise was of an unexpected kind. Prof. Nichols gave a number of conclusive reasons for holding Ångström's value to be the only trustworthy determination. What had to be done was to remove all the invisible rays from the Hefner flame and measure the heat of the remaining visible rays. If a young student under examination were unexpectedly required to give an instant answer to the question how this should be done, never having heard of the problem, he would infallibly propose to disperse the light into a spectrum, screen off the invisible parts, and concentrate the rest into a calorimeter. But, easy as this would be to say, the difficulties of doing it, with the consequent probable errors, caused Thomson and Tumlirz to prefer to cut off the dark heat by simply passing the light through water. Now it is true that the water would permit the passage of but a very small proportion of the dark heat; but this small proportion might amount to as much as all the heat of the visible part, or more. Ångström therefore chose the difficult method. Some of the visible rays would be absorbed in the spectroscope, no doubt; but, by a photometric comparison with the bare flame, or otherwise, he could readily correct for that; and his value is alone capable of reconciliation with Langley's bolometer results and various other observations.

Prof. Nichols gave the amounts of energy per unit of photometric intensity in the light of a kerosene lamp, of an ordinary gaslight, and of an acetylene light. Such vague descriptions of course preclude precision, but the three amounts of heat were nearly in the ratios of 9, 8, and 7. He compared the different illuminants from other points of view. If we take the "economy" of a mode of illumination to consist in the proportion of the energy that is visible, then the Welsbach burner is a more economical source of light than the electric arc or than the sun itself. If, however, we define the economy as consisting in the ratio of the watts of light-emission to the consumption of fuel-weight, the Welsbach burner stands very low--hardly above a tallow candle--while acetylene distances every artificial competitor. Professor Nichols remarked that the place in the spectrum to the light of which the human eye is most senstitive is also the point of maximum energy in the solar spectrum, and that the curve of sensitiveness of the eye for light of different wave-lengths and the curve of energy in sunlight are substantially identical; which he thought gave logical support to the idea that our faculty of vision, including its seeing the particular mixture of light as colorless which it does so see, has been developed in conformity to the character of sunlight. Fortunately the idea is in no desperate need of logical support.

The well-known therapeutist, Dr. Horatio C. Wood, spoke upon the effect of alcohol on the circulation. The University of Pennsylvania has always taught that alcohol is an active stimulant to the heart. The Johns Hopkins University teaches that it is a sedative to the heart. In order to settle the question, Dr. Wood had instituted a thorough experimental research. The mammalian heart is so susceptible to a thousand influences that no decisive results can be obtained with it. The numerous experiments that have been made upon it are conflicting in the extreme. A heart of more poise is needed--one of those hearts which can be taken out and hung on a peg, and which will still go calmly on for days undisturbed. Cold-blooded reptilian and batrachian hearts were chiefly employed. The first question submitted to experiment was, What is the effect of alcohol upon the arterial pressure? The apparatus employed was described. It was ingeniously contrived to avoid subjecting the blood-vessels to unnatural strains. The experiments so conducted showed that ordinarily the pressure is not affected at all by alcohol. But arterial pressure, like all pressure, is a reaction between two opposing forces, the strength of the heart's action on the one hand, and the friction in the arteries on the other. Alcohol might increase one of these while correspondingly diminishing the other. In fact, this was found to be the case; for if the constrictive reaction of the arteries was paralyzed by severing their connection with the vasomotor centres, it was found that the heart dilated so much more fully under the influence

of alcohol that the increase in the flow through the carotid would amount to from 50 to 75 per cent., although the frequency of the heartbeat was unaffected. The apparatus was so arranged that every five minutes there was an alternation between supplying the heart with blood mixed with alcohol and with pure blood, while the blood that passed through the carotid in each five-minute half-period was collected. The result was invariably as stated. It follows, therefore, that alcohol has a double effect on the circulation, at once stimulating the heart and paralyzing the vasomotor centres. But if this be the case, the flow of blood must be greatly increased, and consequently there should be a dilatation of the smaller vessels which would be shown by that instrument which detects changes in the volume of a limb. Dr. Wood's communication was set forth in so interesting a way, and his delivery was so admirable, that the audience was vivaciously responsive.

Professor Brooks, of the Johns Hopkins University, explained how he had discovered that the principal axis of symmetry of the mature oyster is already marked in the ovarian egg--in the egg before it is yet an egg, and while it is still attached. In that stage of development it has a kind of stem, in reference to which the nucleus of segmentation is symmetrically situated. It also has a shell at that time, but as soon as it is expelled the sea water enters the shell, and the egg slips out; whereupon, in ninety-nine cases out of a hundred, the egg assumes a spherical form, so that the axis which its stem had marked now appears to be obliterated. However, in one egg out of every hundred a sort of neck remains, which is indentifiable, by its peculiar shape and by the situation of the nucleus, with what was at first the stem. The segmentary spindle and all the segmentations are placed symmetrically to the axis of this neck, and the identity of the axis can be traced throughout the animal's life. It is perhaps the only case in which a principal feature of a mature, individual, and unattached animal is so indubitably determinate from the very first.

Mr. Agassiz, the President of the Academy, gave a fascinatingly interesting account of his last Albatross expedition. It was intended to explore a part of the Pacific Ocean that had been quite unknown. We will not undertake to state all the lines run over, but the following will show some of the principal passages, though not always in chronological order: From Panama to the Gallapagos (on the equator due south of New Orleans); thence to Callao (12 degrees south); thence to Easter Island, and to Manga Rava Islands (on the tropic of Capricorn westward of Pitcairn, due south of Sitka); back to Acapulco (south by west of the city of Mexico). In this quadrilateral, particularly in its southern part, was found a large district of the ocean characterized by a bottom of manganese nodules, with scarcely anything else. Each haul would fetch up two or three bushels of what looked like potatoes, running up to the size of cannon balls. Such a bottom is found nowhere else, although manganese nodules have been brought up sporadically. Elsewhere the bottom is covered with an ooze largely of decaying animal matter, affording plenty of food, but in this district there is very little food. It is a sea-bottom desert. It lies upon a plateau some 2,000 fathoms deep, between which and the coast is a series of deeps where the soundings were most irregular, some of them reaching 4,000 fathoms. It is curious that the explorers took with them a

chart from Keil, where they found laid down, on the evidence of a few soundings, the "Albatross Plateau," a sort of clairvoyance of what was to be. Mr. Agassiz called it a guess. Over this plateau there are no currents. No food is drifted there, and consequently there is no animal life at the surface. There being no life at the surface, no food can drop to the bottom; and that explains its being a desert. In the Humboldt Current, animal life reaches a depth of 300 fathoms. Where there were currents, plenty of food and animal life was everywhere found. The amount of animal life on the surface of the Humboldt Current is immense, but it diminishes very rapidly as the depth increases, because the temperature falls very rapidly. It would be 72 degrees Fahrenheit at the surface, 52 degrees at 30 fathoms, and at 100 fathoms not much above 40 degrees. Many of the so-called deep-sea animals really do not live below 150 fathoms; but they are brought up in the hauls, and, being much damaged, are supposed to have come from great depths. Mr. Agassiz remarked that this relation between the distribution of animals and the currents throws a certain light upon former geological conditions. It was the eastward currents that stocked and peopled the islands of the Pacific; and there were greater currents in geological ages.

Easter Island was visited, and it was most satisfactory to have an account of that enigmatical place from such an observer. The whole periphery of the island is land-walled 12 to 15 feet high, with numerous platforms for the gigantic images. In the entire absence of wood, these were roughly cut from stone with obsidian tools. The stone must have been soft when first taken from the quarry. They are idols with enormous heads and small bodies, not apparently intended to imitate humanity, and infinitely below the work found in Central America. Hieroglyphics abound which the natives can still read. Originally there must have been a population of four or five thousand inhabitants; at present the natives number fifty or sixty. Every indication is that the work ceased most suddenly, as if in consequence of some unexpected physical or psychical catastrophe. The audience was charmed with the lecture. It recalled Louis Agassiz to those who had heard that famous naturalist and lecturer.

On Wednesday the Academy visited the new Bureau of Standards with the Washington Academy of Sciences. It is as yet impossible to form any critical opinion of this institution; but it has certainly been planned upon a generous scale, with the intention of covering every kind of standardization that there is any important wish in the country to have the Government undertake. The appearance of everything--buildings, instruments, and men--is highly creditable; and there are enough accomplished physicists in the country to make its work an object of national pride.

On Wednesday evening the Academy dined with Mr. Agassiz at the New Willard.

80 (4 May 1905) 360-361: Philosophy as Scientia Scientiarum, and A History of Classifications of the Sciences.

By Robert Flint. Imported by Charles Scribner's Sons. 1904.

A Syllabus of Certain Topics of Logic.

By C. S. Peirce. Boston: Alfred Mudge & Son. 1903.

CSP, identification: Haskell, Index to The Nation. See also: Burks, Bibliography; List of Articles; MSS 1493(s), 1513 (drafts).

Judicious and thorough, the historical part of Dr. Flint's volume satisfies well the conditions that are most essential to a satisfactory history of any department of philosophy. Devoted students of the subject, the few there are, will not need to be told this. It is not merely that Dr. Flint's skill in the clear presentation of the history of philosophy is known to them by his 'Vico,' his 'Historical Philosophy in France,' and his 'Philosophy of History in Europe,' but that for now nearly twenty years they have had the larger and major part of this very work at their elbows, undeterred by its obliging them to harbor volumes of the old-fashioned review in which it first appeared, ponderous volumes--in their avoirdupois we mean, and not merely in their tone. A certain tell-tale redolence of the quarterly-reviewer clings to Dr. Flint still, in a habit of pronouncing judgment concerning questions of philosophy and criticism without having submitted much evidence, if any, to the judgment of the reader, to whom alone it really appertains to pass judgment. This harmless addiction is certainly not a mark of great philosophical strength, nor can that quality be claimed for Dr. Flint; but it has been one of the foibles characteristic of Scottish philosophers, and we all know how the stock of one or two or these has been rising of late in the world market of criticism. That man has not learned to read philosophy to serious purpose who, other things being equal, does not more enjoy, in a discussion of a purely theoretical question, to read that from which he deeply dissents than that with which his own opinions mainly concur. Besides, Dr. Flint's habit brings along with it that sturdy Scottish freedom of private opinion which does not quail before the face of any host of authorities--a trait, for all its amusing us at times, which in a historian of philosophy is as valuable to his student, after those of thoroughness, level-headedness, and fairness, as any that occur to us, simply because the writer who possesses it suggests ideas, while he who follows the great authorities suggests nothing to the student who reads their works for himself.

If Dr. Flint asserts many things which, so far as appears, he is unable to prove, they are for the most part propositions which recommend themselves to natural good sense, and thus at any rate suggest interesting questions. He has attempted no classification of his own, and, as long as he has not been moved to do so, we need not lament it. In this and other historical works he has employed his philosophical capacities with such wisdom as to render them more conducive to philosophical progress than those of many a more vigorous thinker, though it be only indirectly, by the pabulum his works afford to the more vital thought of others. We must say, however, of this book that its utility would have been appreciably enhanced if an appendix had exhibited the schemes of those classifications which are described merely in general terms in the text. Moreover, we should have preferred an alphabetical index to the analytical table of contents that is furnished; but why might we not have had both?

As for fairness, perfection in this virtue is as unattainable as in any other. Dr. Flint is a professor of theology, and it is to be presumed that some cause renders it more difficult for divines to be fair than for other men. We quote the passage which seems to be the most suspect in this respect of any in the book:

"We now reach Auguste Comte, than whom, perhaps, no one has done more for philosophy as positive. He owes the high place he holds among philosophers to the power and skill and general truthfulness of his elaboration of the doctrine of the so-called positive sciences as a whole, not to the merits of his treatment of the particular problem of the classification of the sciences. He claimed, but had no right whatever to claim, that he originated the classification which he adopted. If that classification possess any merits, they must be ascribed to Dr. Burdin, who conceived it, and to Saint-Simon, who first received and published it; not to Comte, although he showed how much could be made of it.... The classification cannot be dissevered from the celebrated 'law of the three states.'"

As to this law, M. Lévy-Bruhl remarks that it

"had been anticipated and even already formulated in the eighteenth century by Turgot, then by Condorcet, and by Dr. Burdin. Comte, nevertheless, takes to himself the merit of the discovery. As he is generally most precise in doing full justice to his precursors, we must admit that, according to him, none of them had seen the scientific importance of this law. It certainly is one thing to gather the notion of a law out of a number of facts, and another to understand its capital importance, and to discern in it the fundamental law which governs the whole evolution of humanity."

It will be seen, from this admission of a writer who, though not a Comtist, is extremely favorable to Comte, that no charge of unfairness on the part of Dr. Flint can be sufficiently supported upon his rather harsh phrases. It is by no means beyond a doubt that the general truth of the Comtian classification depends upon that of the law of the three stages.

It may very likely strike the general reader that the classification of the sciences must probably be a very small matter to have a history that can be expanded into a book. How many such classifications have ever been proposed in all, he may ask? Dr. Flint does not tell, nor can the reviewer; but the latter has examined upwards of a hundred that may fairly be said to be independent of one another. Of titles of publications approximately covering the questions, Dr. Ernest Cushing Richardson, in his book called 'Classification,' enumerated about half as many again; but these are not all independent. Their multitude will not appear surprising if one considers that, in the first place, the different writers aim to make their several classifications subserve several widely distinct purposes, and that their theories of the nature of classification in general are very diverse; that, in the second place, the general word "science" means, for some, what scientia and {episthmh} meant for the ancients, while for others it means, with Coleridge, systematized knowledge, and, with a third party yet the whole business of research as an existing activity; that, in the third place, those who entertain substantially the same notion of science in general, may nevertheless differ as to the nature of "a science"; and that in the fourth place, while the majority of those who attempt to classify the sciences have in view all possible sciences, there are not a few who intend only to enumerate those which have hitherto developed somewhat extensive doctrines. Thus there is no lack of substance for the history.

The second title at the head of this notice is that of a brochure distributed last winter, which gives an outline sketch in four pages of a somewhat elaborate inquiry into the relations of the actual living and advancing studies as they are conceived by the researchers themselves. The outline embraces only theoretical sciences of research; but the study on which it is based allots considerably more space to the practical sciences.

Dr. Flint's essay on philosophy as scientia scientiarum sketches his own notion of what philosophy should be. There is little argument in it, and that little rather inconclusive.

80 (11 May 1905) 374: NOTES

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

In a little volume called 'N-Rays' (Longmans, Green & Co.), Mr. J. Garcin has shown his French tact in book-making by simply translating Blondlot's papers just as they stand in the Comptes-rendus, which gives the book a life and interest that no literary make-up could ever impart. The N-rays, so called from the town of Nancy, in whose university M. Blondlot is professor, are simply light-rays; but they can scarcely be described as ordinary light, since (unless there is a misprint of cm. for mm.) they can be detected after passing through a thickness of over two feet of metallic copper. They are ultra-violet rays. An ordinary eye with an ordinary spectroscope can hardly (without fluorescence) perceive any light of lesser wave-length than 400 micro-microns. (A micro-micron is a thousandth of a micron, and a micron is a thousandth of a millimeter.) About eighteen months ago the Smithsonian Institution published a research by Dr. Victor Schumann, showing that by replacing the glass lenses and prism of a spectroscope with lenses and prism of fluor-spar, and by exhausting the air to a high vacuum, the spectrum of hydrogen could be photographed as high (i.e., to as short a wave-length) as 100 micro-microns. Long before that point is reached, air has become absolutely opaque, even in layers a hundredth of an inch thick or less; so that not a scintilla of such light reaches us from the sun. But now these new N-rays, whose wave-lengths range over more than two octaves, from 18 down to 3 micro-microns, pass through air with perfect ease, and are observed by Blondlot with a prism and lenses of metallic aluminium. They are found in sunlight and in various artificial lights. They are absolutely cut off by the thinnest film of pure water, but pass through it readily if a little (even a very little) table salt be dissolved in the water; and they are faintly perceptible to a good eye if it be reinforced by placing behind it an artist's power of seeing without looking. The means of experimentation may be reduced to extreme simplicity, and all that is requisite for it is actually bound up in the volume--the instrument itself, we mean. These rays ought to become the rage.

80 (1 June 1905) 438: NOTES

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

--The third edition of George Kamensky's translation of Mendeléef's 'Principles of Chemistry' (Longmans, Green & Co.), probably from the definitive edition of the original, incorporates a laborious revision by Mendeléeff, for the years intervening since the last edition have been wonderful years for chemistry. Our readers need not, perhaps, be reminded that the book, as an account of the distinctive properties of all the chemical elements, which is its character, is one of the most, if not the very most, marvellous achievement in making a subject of infinite details untiringly interesting, and investing it with a power of taking root in the reader's memory. It is also valuable as expressing with unusual openness all the processes of thought of one of the greatest scientific reasoners that ever lived. It cannot, however, be called a model of judicious and calm logic. Whatever proposition Mendeléeff inclines to, which must be something illuminating his most famous discovery, will be for him "a logical development"; while anything else will be a "hypothesis," regardless of its logical genesis. The phase rule is admitted to the rank of "a theory," which he hopes may in time develop into "the true theory." On many points he is skeptical about the doctrines of the new chemistry, and sometimes his objections have no little force, but they are always exaggerated. Perhaps the most valuable addition to this edition, because the substance of it will not be found elsewhere, is an admirable account of all the metals of the rare earths, contributed to this edition from the pen of B. Brauner, than whom no authority could be higher and probably no other as competent in every way for this task. It shows the "periodic law," as it is called--though it never was anything more than a rule of waviness--suffering under the serious defection of over a dozen elements; and a whole horizontal line (or two, as commonly drawn up) is erased from Mendeléeff's table. Brauner compares the series of elements from cerium to ytterbium to the triads in the eighth column, iron, nickel, cobalt, etc., where the atomic weights progress while the general chemical character remains constant. This is a matter of deep interest for scientific logic on account of the extraordinary success of almost all the predictions that have been based on Menedeléeff's scheme, which have by no means been confined to the existence of scandium, gallium, and germanium. It cannot, of course, break the probative force of successful prediction, but it must cause men to scrutinize instances of it, and see to what circumstances it may have been due. We must add that the "periodic law" retains all the value it ever had for other parts of the table. The three English editions of the book, which have represented three successive Russian editions, have all had different English editors. The first of them was a fairly competent person as translators go, and the others have been careful about getting the numbers right; the last corrects the form of some

names, and Wyruboff, for example, no longer occasionally figures as Wernhoff. But otherwise, the new editor leaves much to be desired, and in several instances renders statements false which in the previous editions had been rightly given.

80 (1 June 1905) 444-445: **Scientific Fact and**

Metaphysical Reality.

By Robert Brandon Arnold. Macmillan. 1904.

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

Perhaps the less one knows of philosophy, the more one will find in this book. In reading it one is now and then reminded that the author is an adherent of Bradley. The volume is about the size of 'Appearance and Reality,' and its page has somewhat the appearance of that famous essay. It further reminds one of Bradley by the many things it pronounces "obvious" which few would discover to be so of themselves. The work has a method, too; or so it informs us. The author is so thoroughly acquainted with the Absolute that we cannot think of questioning what he has to say of Metaphysical Reality, but he is by no means at home with Scientific Fact. His misunderstandings of the many scientific books he seems to have read or dipped into, are surprising.

We have carefully selected for examination a single paragraph, to be treated indulgently, if so we may convey a sufficient idea of what the whole is like. It fills two pages and a half. The subject under consideration is immortality, which may be taken as indicating the relation of the work to "Scientific Fact." The writer begins by telling his reader that "a small cyclonic eddy sweeping along a road"--in short, a whirlwind--may, after the lapse of "a few minutes," "not contain a single material particle which was present in it" before; and he suggests that the identity of a "human personality" ought to be conceived as analogous to that of the whirlwind. To support this, he remarks that "it is decidedly inaccurate to describe" "conscious manifestations" "as dependent on a particular formation of molecules in the brain," since the true "correlation" is between "mental phenomena" and "cerebral changes in form and activities." For the "stream of consciousness observed in introspection should be regarded as the energy aspect of our being and of the universe," provided energy be understood in a generalized sense in which it covers more than mere physical energy--a generalization which is at all events indispensable to save the physicist from falling into contradictions. In short,

"our view may be put thus: all evidence tends to show that the organism is one. Yet we have the clear distinction of body and mind giving rise to different sciences, physiology and psychology. The solution seems to lie in the inevitable inadequacy due to our evolutionary and limited point of view, for which certain aspects of the universe must become apparent before others, and hence all the possible ground of our apprehension is prematurely occupied. Psychology is barely fifty years old, but already is becoming psychophysics. We stated earlier that

mind is matter in certain combinations transcended. It would perhaps be more accurate to say that in proportion as the universe is apprehended from a more comprehensive standpoint, it displays what is to us the mental aspect more plainly."

Since accuracy is here in question, what accuracy is there in saying that "all evidence tends to show that the organism is one"? It is difficult to understand what is meant by saying that psychology is barely fifty years old (which would mean either that Lotze, Bain, or Spencer originated it, for it would be too absurd to attribute its paternity to Volkmann von Volkmar or even to George), while in the same breath saying it is now becoming psychophysics; for surely there has been no work on psychology more purely psychophysical than Wundt's 'Vorlesungen über die Menschen- und Thierseelen,' which appeared in 1863, therefore almost at the birth of the science, according to Arnold. If we remember rightly, Tyndall likened thought to a wave, as a form that is propagated and has a sort of identity, and yet is not matter. It was probably that comparison which has suggested to Mr. Arnold that a person is something like a vortex. But anybody who is competent to draw a simple inference from the testimony of his two eyes can see that a vortex is entirely different from a wave, being a portion of fluid matter, and not a mere form like a wave, which leaves a floating chip behind and does not carry everything along as a whirlwind does. It is not true that its particles are constantly being exchanged for others like the particles of a wave. But such is the representation of 'Scientific Fact' throughout the book.

80 (8 June 1905) 461: NOTES

CSP, identification: Haskell, Index to The Nation. See also: Burks, Bibliography; Fisch and Haskell, Additions to Cohen's Bibliography; MSS 1494, 1497 (drafts).

George Santayana was born Jorge Ruiz de Santayana in Madrid, Spain. At an early age, he came to the United States with his parents, who sought the best education for their son. Santayana was graduated A.B. from Harvard in 1886 and earned his Ph.D. there in 1889. He was known for his poetry as well as his philosophical writings.

--Professor Santayana of Harvard puts forth the first two little volumes of five to be entitled "The Life of Reason, or the Phases of Human Progress" (Charles Scribner's Sons). The first volume is entitled 'Introduction, and Reason in Common Sense,' the second 'Reason in Society.' Reason in Religion, Art, and Science respectively are promised. The publishers' advertisement called the philosophy pragmatistic, and it is true that the first part of the first chapter and some other passages have that aroma; but the author more accurately describes himself when he says that "almost every school can furnish something useful." It is, in fact, an eclectic philosophy, and, like other works of that sort, is likely to have more literary than scientific value. Book A of Aristotle's Metaphysics (the theistic book) has had more influence upon this mosaic of opinions than any other one work. Professor Santayana's style is highly polished, in parts too much so; so that we are bewildered and fatigued by a shimmer of rapidly passing thoughts that are hard to make out through a medium more glittering than lucid. He is so extravagant an admirer of Greek reasonableness that we have sometimes asked ourselves whether he hadn't a deep design of producing in his writing a general effect of Greek; although nobody can be more perfectly aware than Professor Santayana that on a closer inspection every feature of his style is at the very antipodes of Greece--as far from it as one could possibly get. Professor Santayana's volumes are anything but commonplace. They are all that Boston has of most précieux. They are also extremely handy and agreeable to the eyes.

80 (22 June 1905) 503: NOTES

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

--Ida Freund's 'The Study of Chemical Composition' (Cambridge [Eng.] University Press; New York: Macmillan) is a work upon a novel plan. A sub-title informs us that it is an account of the method of that study and its historical development, with illustrative quotations. Another feature of it is that it selects the most instructive examples and sedulously avoids all appearance of giving complete details of anything; showing, for example, the general plan and results of the best determinations of the atomic weights of more than half of the elements, but saying nothing about the atomic weights of the others, nor generally about their determinations of those that are referred to. Its historical parts go back to the phlogistic period and even earlier. We cannot see how the plan could have been carried out with greater skill; and it is an excellent plan, provided the narrow limitations of its utility be recognized. To a professional student of chemistry it will afford amusement, and here and there call his attention to a stray fact or consideration. To a non-professional student it presents the disadvantage that, after he has gone through it, it will not serve as a handbook; but he will be driven to handbooks of which he has made no study, and which very likely falsify their names in not being at hand. Nevertheless, to such a student it offers the signal advantage that, if the study of it be supplemented by a few months of entire absorption in laboratory work, it will furnish him with very nearly such aperçus of this branch of science as the real researcher in it has. The authoress has evidently held this purpose steadily in view, and accordingly directs attention at each epoch of each problem to its logical aspects; and her opening chapter is devoted to the method of the inductive sciences, a subject to which the historical method is more advantageously applicable than to chemistry. Unfortunately, she has made no exact study of scientific logic, and both this chapter and other parts of her book have suffered grievously from this deficiency.

80 (29 June 1905) 527-528: James Watt.

By Andrew Carnegie. Doubleday, Page & Co. 1905. 8vo, pp. 241.

CSP, identification: Haskell, Index to The Nation. See also: Burks, Bibliography; List of Articles; MSS 1495, 1495(s) (drafts.).

Andrew Carnegie (1835-1919) was one of the richest and most influential men of his time. As a boy, he worked in the textile mills near Pittsburgh for one dollar per week and as a young man began to accumulate his fortune by timely investments in petroleum and the railroads. Carnegie built a financial empire around his vast holdings in the steel industry, yet he never let himself abandon the humanistic goals developed in his youth. As an author, he enjoyed much popularity; in his article "How To Get Rich," he expressed the view that native ability and industry are sufficient to insure prosperity without a college education. And in his book Wealth, Carnegie held that surplus wealth is a trust to be administered for the good of the community, and he exemplified this attitude by his extensive benefactions to libraries and other institutions.

Its lively, not to say jerky, style would hardly be a sufficient inducement to read this book. It contains, beyond a scrap or two, no new information about Watt. A particular interest, however, must attach to Mr. Carnegie's commentary on Watt's life, as one sees as soon as one recalls what that life was. Not the testimony of eulogists, but systematic critical comparison, warrants us in placing Watt among the world's great intellects, narrow though the field was to which he confined himself. Even when he was a child, his genius produced its impression, and was recognized by a few perspicacious men for what it was. At the grammar school he was backward in his recitations. He was not good at answering such questions as boys are expected to answer. Yet he must have been making good use of his time, since the early records of his thinking astonish us by bearing the marks of a trained mind--his stores of knowledge seem so well ordered, he strikes the nail so squarely on the head with unfaltering promptness, he is so sure to carry this most attentive scrutiny to the points where logical leakiness was most to be apprehended. The specifications of his patent, drawn up by this almost unschooled young man at an early stage of the invention, continued during near a generation to furnish lawyers with new surprises at their completeness.

When, at twenty years of age, he went to set up his little shop in Glasgow, explicit testimony and unmistakable actions show us that the extraordinary minds then clustered in the University began from their first interviews with the young intrumentmaker [[sic]] to entertain a very thorough respect for his acquirements and intellect. John Robison, the well-known astronomer and physicist (afterwards called to a professorship in Russia), happening, when himself not quite twenty (Watt being then twenty-two), to drop into the shop, confesses, "I had the vanity to think myself a pretty good proficient in my favorite study, and was rather mortified at finding Mr. Watt so much my superior."

When, at the age of twenty-three, he took up his great problem, the judiciously contrived pumping-engine of Newcomen (which Mr. Carnegie does not understand, and misdescribes; but then Newcomen was born in the south of England) was beginning to be inadequate for the deepening mines of Cornwall, although its general principle is that of a modern pumping-engine, and though it had un-

questionably been better adapted to pumping out coal mines than any more complicated contrivance, especially if requiring a considerable pressure of steam, would have been. It had a boiler and a separate piston-cylinder. Steam, being admitted to the vertical cylinder below the piston, raised this, and was first cut off and then chilled in the cylinder; whereupon atmospheric pressure, aided by the weight of the piston, restored the latter to its first position. The power was transmitted to the pump by means of a beam oscillating about its centre. Watt began by making an experimental study, sufficiently thorough and accurate for all the practical purposes of his generation, of the thermal phenomena of the boiling of water. He discovered for himself the heat of vaporization, though he at once found that Black, who was professor in Glasgow, but who printed hardly anything, had taught the whole doctine of "latent heat" for two years. He also discovered that the same amount of heat was required to convert water from a fixed temperature into steam, no matter under what pressure it was boiled, as nearly as he was able to measure this heat. Being thus sufficiently equipped with theoretical data, he proceeded to the work of invention; and within a few months after his rediscovery of the heat of vaporization he had hit upon the idea of his essential improvement of the steam-engine, that of the condenser. That is, instead of chilling the steam in the cylinder, which chilled the metal of the cylinder and thus the new steam sent into it, he opened a passage into an exhausted cold chamber where a jet of water cooled the steam. He did not stop here, however, but perfected the engine in its principal features and in its minutest accessories considerably beyond what could advantageously be put into practice in his day.

Although these inventions were not all made before he began actual work, they were mostly made within four years. For thirty-five years Watt's life was a struggle to get the engines made and running. There was no machinery to make them with, nor any good tools. There were no even barely tolerable journeymen machinists. There were no men fit to be trusted to run the engines; and the best there were were liable at any time to smash things in their drunkenness. For twenty-five of these years, Watt was the second partner in what was for those times a vast concern--the greatest mechanical establishment in the world. He was not naturally a man of business. He detested it. Financial affairs terrified him; and he always dwelt too much on adverse contingencies. He had the Celtic passion, pride, imagination, glow, social sympathies; and it needed another Celt to interpret him to us. The publishers well understood what was wanted in asking Mr. Carnegie to undertake this biography. It is very satisfactory to find that his judgment of the conduct of the firm is distinctly commendatory. Many sagacious observations of general application to the direction of large works are scattered through the volume, which its author evidently endeavors to render practically useful; so that it becomes not only a study of Watt, but an elucidation of Carnegie. In this connection, one will remark how full and clear an idea of the entire contents of the book he carries away from the reading of it. We note that here is one business man more added to the list of admirers of Samuel Smiles.

Happily, no literary person has been permitted to tamper with the text, as several passages convincingly prove. The preface names two highly competent engineers as having revised the technical passages, but here and there a sentence may be found to which they can hardly have lent their deliberate approval. Thus, on page 74, we read that the Newcomen engine "was an atmospheric engine, and in no sense a steam engine." In no sense? On page 49, and in a paragraph devoted to explaining what is meant by the phrase "latent heat," we read: "Heat . . . lies also in water.... The heat lies latent and dead until we raise the temperature of the water to 212 degrees, and it is turned into vapor. Then the powerful force is instantly imbued with life." But, in fact, the living force is not derived from the water, but from the coal; and more heat is "latent" in the steam than in the water.

Mr. Carnegie is quite mistaken, too, in attributing the first discovery of the composition of water to Watt. It having been already known that water was produced when hydrogen was burnt, Cavendish in 1781 ascertained that pure water was the sole product, and that it equalled in weight the sum of the oxygen and hydrogen consumed. He told Priestley of this, who, with a rather amusing notion of his own competence as compared with that of Cavendish, undertook to repeat the experiment with greater precision. In 1783 Priestley communicated the result to Watt, but stating it in terms of the phlogiston theory to which he was wedded. All that Watt did was to set Priestley's logic right by stating the matter as everybody now states it, and as Cavendish had originally conceived it. Mr. Carnegie can find the whole story, as all critics now concede that it should be told, in Thorpe's 'Essays in Historical Chemistry' (Macmillan, 1902).

On page 15 it is said that, "at fifteen, Watt had read twice carefully 'The Elements of Philosophy' (Gravesend)." This was not, however, Gravesande's great 'Introductio ad Philosophiam,' of 1736, which may be said to have given birth to the Scotch philosophy of common sense, but was only (doubtless in translation) his 'Physices Elementa Mathematica, Experimentis Confirmata, sive Introductio ad Philosophiam Newtonianam,' of 1720.

The reader would have been placed in a better situation to appreciate Watt's intellect if his lesser invetions could have been more fully explained, such as the indicator diagram, and especially that approximate parallel motion which he himself considered his chef-d'œuvre of ingenuity, for which see Kempe's 'How to Draw a Straight Line' (Macmillan, 1877).

Although Watt's statement that the total heat of saturated steam is the same at all pressures is so far from being true that no less than three-tenths of the sensible heat of the water must be subtracted from the total heat in order to get a substantially constant remainder, yet it does not follow that Watt's observations were in error. He could not do otherwise than assume that the heat imparted to the water, up to its boiling point, was measured by its temperature, which he naturally would ascertain by a mercurial thermometer, doubtless reading it only to the nearest Fahrenheit degree. If, then, he determined the heat of vaporization by passing the steam into water not very much cooler, it might very well happen that, although every figure he recorded was correct, these figures should have indicated an exact constancy of the total heat at all pressures.

Our readers will be interested in knowing that Watt not only read French and Italian, but, at a time when any knowledge of German was rare, took pleasure in

discussing German poetry, and that he seems also to have had some acquaintance with Kant.

81 (13 July 1905) 33: NOTES

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

Miss Gertrude Bacon, herself well known as an aeronaut, has produced 'Balloons, Airships, and Flying Machines' (Dodd, Mead & Co.). The whole story having been already told a thousand times with every imaginable device of sensation, her problem was to make a brief thousand and first recital not wholly uninteresting, without resort to any other means than those that are strictly germane to the history of airfaring. The tiny volume that has resulted is a little triumph, due to a bright, fresh mind drawing from the headwaters of information ideas that sparkle with genuine interest in the subject, which is allowed to run on in its own natural babble. Just as an ordinary observer who stands up with some effort against the pressure of a high wind, and descries far up in the blue a balloon hurried along in a still more tremendous gale, is apt to forget that, to the balloonist himself, he seems to be in a dead calm--the spectator, indeed, finding it hard to think of such a thing--so the plainest narrative of a balloon trip told strictly from the able airman's point of view, in perfect equanimity, never mounting into any purple clouds, never soaring above any reader's head, but sticking to the terra firma of plain fact, makes a far stronger impression upon the imagination than in any other style it ever could. That was a discovery exhibited by Miss Bacon's father in his 'By Land and Sky'; and the daughter has caught a little of his charm. The illustrations are particularly well chosen, and several of the most vivid and telling are quite new. We do not know whether the "Crossing the Channel" (p. 46) is so or not; but the frontispiece certainly is.

81 (13 July 1905) 33-34: NOTES

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

Another little book of the same "Practical Science Series," entitled 'Radium Explained,' by Dr. W. Hampson, is not so fortunate an attempt. It is largely taken up with an attack upon the electronal theory of matter and with preparations for that attack. Now that theory, bright though its prospects certainly are, is very far from being proved, and it is quite a legitimate object of attack. But unless its opponent has a clear understanding of the theory of electricity, his argument will make wearisome wading; and this is the first fault of the present book. Its further fault is that it undertakes to solve this profound problem on a basis of common sense. Now common sense, rightly interpreted, has nothing at all to say about

electricity, which ought to be left to specialists; and the attempt to drag common sense into the question can amount only to abandoning the study of experimental phenomena and to engaging in cloudy talk about words and phrases. Dr. Hampson, however, is not alone in his ill-managed argument. Professor Dolbear, in the Popular Science Monthly for July, takes up a similar line of discourse, and so have others done. All these writers say that "electricity" consists in a particular variety of energy; but they do not tell us what they use the word electricity to denote. If it means, what physicists and others usually mean by electricity, a charge of electricity, then, as Sir Oliver Lodge well remarks, to say that electricity consists in energy is very much like saying that a glass of water is composed of energy. But the question of the truth of the electronal theory is not a question of words--it is a question of how a certain laboratory experiment will turn out whenever we may be able, literally or virtually, to perform it.

81 (13 July 1905) 42-43: Sociological Papers.

Published by the Sociological Society. Macmillan Co. 1905. 8vo. pp. 292.

Foundations of Sociology.

By Edward Alsworth Ross. Macmillan Co. 1905. 12mo, pp. 410.

CSP, identification: Haskell, Index to The Nation. See also: Burks, Bibliography; List of Articles; MSS 1496, 1496(s) (drafts).

Edward Alsworth Ross was a pioneer of teaching sociology in American universities. He was graduated A.B. from Coe College in 1886 and Ph.D. from Johns Hopkins University in 1891. During his teaching career, Ross lectured at the University of Indiana, Cornell University, Stanford University, the University of Nebraska, the University of Wisconsin, Harvard University, and the University of Chicago.

"The Sociological Society was instituted [in London] in November, 1903.... Its aims are scientific, educational and practical.... It prosecutes its work by the means customary to an efficiently organized learned society." We commented last year upon Mr. Galton's brief paper on "Eugenics," a new name for an old study. It was first given at a meeting of this society, and is contained in this volume. The contributions by other members of the Society are well enough, but they evince no laborious research in producing them. The ideas of the essay called "Civics" by Professor Patrick Geddes (not the philologist) appear to us to be, as the Germans say, quite too "schematic," and too heavily loaded with misplaced imagery. One of the best papers is by Dr. Westermarck; but it really needed nothing beyond an ordinary acquaintance with men and women and some reading of books of travel to remark, what (with illustrative facts) constitutes the sum and substance of the piece, that a man's having a legal right to sell his wife or put her to death is no proof that the influence of women is nil (quite the other way, we should say); nor is this proved by the whole business of agriculture being in their hands. The one real addition to knowledge that the volume contains is by an outsider, Mr. Harold H. Mann. It is a minute account of life in the purely agricultural village of Ridgmount, where the population numbers 467, and where

a large and increasing proportion of the land is the property of the Duke of Bedford. The fullest statistics are given of every imaginable kind, and the individual circumstances of about one family out of every five are described. That is certainly a way of working from which some definite results may be hoped for. At any rate, it shows one indispensable qualification for valuable scientific work.

The interest of the volume is not confined to the papers; for the discussions that followed the readings are reported. In this way nearly eighty different speakers and writers have contributed to the publication, which has a curious interest for an American by showing what sort of timber goes to the construction of an English sociological body, and how English ways of working together differ from American ways. For example, Dr. Karl Pearson, who is not a member of the Society, seems to have attended the second meeting simply for the purpose of hearing Galton read his paper on Eugenics. There is no sign of his having had any arrière-pensée. But when he arrived, he was invited to take the chair, which would hardly have happened in this country; whereupon he accepted the honor, which an American would have avoided if it was going to oblige him to declare, as Professor Pearson did publicly declare, as soon as Mr. Galton had done reading, that he disapproved of the existence of such a society in toto. He said: "Frankly, I do not believe in groups of men and women who have each and all their allotted daily task creating a new branch of science," and continued for perhaps twenty lines of our columns in this vein, without any mitigation of his condemnation. Every man of science--certainly of any exact science--will say that there is a world of sound sense in the sentence we have quoted, abstractly considered. But, not to mention that (as a vague idea, at least) sociology is not a new science, Professor Pearson ought to have seen that the question was not whether the gentlemen and ladies who should join the Society were likely to be so very highly scientific, but whether they would not themselves, in the first place, get much good out of the meetings, and whether, in the second place, they would not create a centre of light and warmth that would surely radiate wholesome influences through the community. "If it adds to their satisfaction," he should have asked himself, "to tell themselves that they are members of 'an efficiently constituted learned body,' as their prospectus phrases it, will there be any harm in that at all comparable with the good that is likely to come of it?" When he went on to say that "the history of each great branch of science" shows that it was the creation of some one great man, he simply showed how impromptu and inconsiderate his speech was; for while some of the lesser divisions of science have so taken birth, such great sciences as astronomy, biology, chemistry, electricity, and the like were rooted in the observations of many men who had even less notion of scientific method by far than has the average member of a social-science association of to-day.

The very handsome volume has the appearance of being printed in six or seven different sizes of type, and we have been unable to guess why one man's remarks are printed in quite tiny letters and another's very prominently. We remark that an editorial from this journal appears in type several sizes larger than that of its original dress, and that Professor Pearson is not reduced to quite the smallest type, such as individuals like M. Rodolphe Dareste de la Chavanne of the Institut de France, Prof. Andrew Seth Pringle-Pattison, Dr. Shadworth Hodgson, etc., have to content themselves with, while Mr. Victor V. Branford's rather tedious reply to Pearson appears in the very biggest letters, far beyond what are accorded to the President of the Society, the Right Hon. James Bryce, D.C.L., etc. Mr. Branford is the secretary.

That there are social sciences--the natural history of religion, economics, political science, the science of human heredity--there can be no doubt. Whether or not there already exists a general sociology apart from the social sciences, is a question too vague to be answered. It is certain that many writers, Dr. Lester Ward, Tarde, Stuckenberg, Lilienfeld, Letourneau, Kovalewski, Benjamin Kidd, Giddings, Alfred Fouillée, Roberty, De Greef--to name promiscuously a few out of many--have worked more or less intelligently in the direction of such a science, although with considerable variety in their conceptions of what it is to consist in. Professor Ross's volume is an endeavor of considerable force in the same direction, and may be particularly recommended as easy to read, brief, comprehensive, and introducing the reader to most of the conceptions of value. The author is a pretty close adherent of the views of Ward, and attributes a more exclusive importance to the conscious desires of individuals than the psychologists would generally admit. The book's greatest fault is the fault of most books on the subject that are now appearing--that of undervaluing work which is too abstract to meet the conditions of a real practical problem; a spirit which would have effectually prevented the development of physical science. It is worth mention that the phrase "race suicide" was first used in one of the papers which make up this heterogeneous volume.

81 (20 July 1905) 56-57: WUNDT'S PRINCIPLES OF PHYSIOLOGICAL PSYCHOLOGY

Principles of Physiological Psychology.

By Wilhelm Wundt. Translated from the Fifth German Edition (1902) by Edward Bradford Titchener. Vol. I. The Macmillan Co. 1904.

CSP, identification: Haskell, Index to The Nation. See also: Burks, Bibliography; List of Articles; MSS 1497, 1497(s) (drafts).

Edward Bradford Titchener (1867-1927) exerted a strong influence on the growth of American psychology. He received a B.A. degree from Oxford in 1890, and later studied under Wilhelm Wundt at the University of Leipzig. In 1892. he was appointed assistant professor of psychology at Cornell University. Titchener lectured at the University of Virginia, Columbia University, the University of Illinois, and the Lowell Institute. He served as the American editor of Mind from 1894 until 1920 and was the editor of the American Journal of Psychology from 1921 until 1925.

When, in 1862, two years after Fechner's 'Psychophysik,' Wundt emerged from the physiological laboratory with his 'Beiträge zur Theorie der Sinneswahrnehmung,' students in this country there were who saw in the little volume the harbinger of a new science of experimental psychology; and the next year their hopes seemed to be crowned in the same author's 'Vorlesungen über die Menschen- und Thierseele,' concerning which, by the way, it had better be noted that, like other of Wundt's books, it has lost most of its original flavor in a second, reconsidered edition, and that the English translation represents this later edition. Without this explanation, the sensation it first caused would be incomprehensible. Its readers heard in it the promise that the new science should keep pace with the other strictly experimental sciences, and should quickly outstrip all those sciences (more numerous then than now) in which experimentation had not become practicable. Alas, to-day we are forty years wiser, and a chilling shade settles on hearts of enthusiasts of the sixties who now compare the advance that psychology has achieved--indisputable, but how modest!--with the unheard-of leaps that every other science has performed, be it an experimental one or not. Since 1860 the foundations of pure mathematics have been reconstructed; exact logic has been developed; physics has gained an optico-electrical theory, and radically new conceptions of molecular forces have been established; organic chemistry has followed out the doctrine of the aromatic compounds, and has been enriched by the doctrine of the unsymmetrical carbon atom; in its inorganic division the classification of the elements has been laid bare, the group of helium-argon elements has been added, and Mme. Curie has pronounced her magical "Open, sesame!" Besides all that, a new and more scientific kind of chemistry has been opened up. Biology has been equally revolutionized; astronomy has its new astrophysics, and geognosy has kept pace with the other sciences. Even on the psychical wing, linguistics, ethnology, archæology, the history of high antiquity, have all found and matured new methods. In short, there is not a science that has not left psychology lingering in the rear; and the burning question of to-day is, why this should be so? Who will diagnose the malady of psychology?

It has been remarked that, at present, there is nothing which for the psychical wing of science fulfils that function which the science of dynamics fulfils on the physical side. Everybody knows what that function is. Every attempt to explain any phenomenon physically consists in first proposing some hypothesis as to the existence of designated dynamical conditions from which, according to the principles of dynamics, phenomena such as have been observed would take place, and then going on to put the hypothesis to the test of making it the basis of predictions concerning untried experiments.

Now it is a circumstance most significant for the logic of science, that this science of dynamics, upon which all the physical sciences repose, when defined in the strict way in which its founders understood it, and not as embracing the law of the conservation of energy, neither is nor ever was one of the special sciences that aim at the discovery of novel phenomena, but merely consists in the analysis of truths which universal experience has compelled every man of us to acknowledge. Thus, the proof by Archimedes of the principle of the lever, upon which Lagrange substantially bases the whole statical branch of the science, consists in showing that that principle is virtually assumed in our ordinary conception of two bodies of equal weight. Such universal experiences may not be true to microscopical exactitude, but that they are true in the main is assumed by everybody who devises an experiment, and is therefore more certain than any result of a laboratory experiment.

The sort of science that is founded upon the common experience of all men was recognized by Jeremy Bentham under the name of cenoscopy, in opposition to idioscopy, which discovers new phenomena. But long before Bentham's day the situation was sufficiently understood to set up a movement in the more enlightened countries to supply the psychical sciences with an analogous analytical foundation. The innumerable grades in the distinctness of thought prevent us from assigning dates, but one may say that the idea is struggling to the light in Locke's 'Essay' of 1689, and that its development was the best fruit of the eighteenth century. It moved in Italy, in France, and especially in Scotland. The analytical economics of Adam Smith and of Ricardo were examples of it. The whole doctrine in its totality is properly termed the Philosophy of Common Sense, of which analytical mechanics and analytical economics are branches. That Pragmatism of which so much has been said of late years is only an endeavor to give the philosophy of common sense a more exact development, especially by emphasizing the point that there is no intellectual value in mere feeling per se, but that the whole function of thinking consists in the regulation of conduct. All this it is most needful to comprehend in order to assign to Wundt his proper rating in the history of philosophy.

The 'Physiological Psychology' is Wundt's most imposing and monumental work, but no man of science will call it his chef d'œvre [[sic]]. That rank can be accorded to one production alone, his 'Untersuchungen zur Mechanik der Nerven und Nervencentren,' of which the first part appeared in 1871; the second, which is less fundamental, but perhaps not less important, having been delayed by accidental causes until 1876, after the first edition of the 'Physiological Psychology' had appeared. Four traits of the 'Mechanik der Nerven' command admiration. One of them is a natural gift; two are results of scientific training; and one is a moral virtue. The gift is an astonishing sagacity about nerve-physiology--a subterconscious susceptibility to the noeto-meteorological premonitions of a hailstorm of evidence that, when it bursts, will be cold, hard, and cutting enough.

Of the two scientific perfections the more striking is the mature prestudy of the methods that were or might have been pursued in the investigation. The other is the vigilant scrutiny of all details of the phenomena, especially of such as, being unlooked for, might easily have been overlooked. But the most admirable trait of all--that self-respecting quality of Wundt's which no foibles can obscure--is his genuine anxiety to correct the opinions which he at the time entertains, and to cast away his most brilliant theories the instant the dicta of experience seem to be against them--a quality in which he so contrasts with all the metaphysical charlatans and self-admirers and with every other quintessential extract of littleness. Wundt's great service to man, aside from that special research described in the 'Mechanik der Nerven,' has consisted in teaching the students of cenoscopy the beauty of those virtues upon which the students of idioscopy, especially those on the physical wing, have always insisted--virtues that will necessarily result from any well-considered desire to know the truth. That such service has been Wundt's undoubtedly remains true, notwithstanding some lapses.

But the work of which Professor Tichener is publishing his translation is not to be classed as a performance of idioscopy and little given is idioscopy to expressing itself in big books. It is not a work of heuretic science of any kind. It is a product

of that useful industry of collecting, arranging, and digesting the deductions of mathematics, the analyses of cenoscopy, and the discoveries of idioscopy--a service of which the Germans have assumed the burden, and which, as being the "systematization of knowledge," they as well as the general public are too apt to mistake for the business of science. From the date of the publication of this work, Wundt has turned a corner in his career, and has pursued a course not determined by the intrinsic affinities of his previous work. His principal publications (aside from revisions and from papers in his periodical Philosophische Studien) have consisted in an extensive treatise on logic, another on ethics, and a 'System der Philosophie.' These are subjects to which the majority of their devotees have been led by a desire to settle their beliefs about God, freedom, and immortality. But students of science are a good deal given to thinking that high theory is more apt to lead men wrong than right about religion, while religion has never done theory more good than harm. The doubts which impelled the few men of science who have been led to any thorough study of philosophy have almost always been concerned with the limits of trustworthiness of scientific results. But Wundt has never entertained any such general doubts. He explicitly says that whatever is not based upon the results of the special sciences has no real basis at all. He makes no exception in favor of dynamics, on the truth of which all his own work reposes. But, for him, common sense is nothing but an imperfect kind of science; and it is remarkable that his physiology recognizes no very fundamental difference between the functions of the cerebral cortex and those of the organs at the base of the brain. To the question what could have been Wundt's motive in putting himself forward as a leader in philosophy, for which he had never displayed any genius, but rather the reverse, the answer to which the study of his writings must lead is that the results of experimental psychology, meagre though they be as compared with those of other sciences, so dazzled the imagination of Wundt as to make him think that that study alone must be set up as the queen of the sciences, and prompted him to try to prove that logic, ethics, and philosophy could be securely based on that special science.

Wundt's philosophical publications have not met the acclamations that he undoubtedly at first expected; nor can it be said that the two scientific merits above mentioned are here one whit better exemplified than in the general run of second-rate philosophical treatises of the time. They rather fall below that average. In the matter of the deliberate preselection of methods, for example, one will not often meet with anything weaker than Wundt's admission that it seems self-evident that metaphysics should not be made to depend on the results of special science, while defending himself by saying that, having come to philosophy from physical science by the route of experimental psychology, it is natural that he should be unable to pursue philosophical investigations by any other method than that which his own sequence of study suggested to him. ("Ship ahoy!--Where are you bound?" "For the port of Philosophy." "Then why, in Heaven's name, are you sailing on that course, Captain Wundt?" "Well, the truth is, this is the way the vessel was heading at the time it occurred to me to make that port.") Other equally gross departures from the two scientific ideals could easily be pointed out. Whether or not, if Wundt had possessed any analytical strength, it would have been possible for him to imagine that he could base such matters as dynamics, geometry, and arithmetic upon his physiological experiments, or whether in that case he could have failed to perceive the value of the pragmatist analysis in binding together nerve-physiology and psychology, must remain matters of opinion. But, unfortunately for his good fame, there exist departments of logic upon which he has touched that no more fall within the marches of opinion than does the principle of the lever or the doctrine of limits; and here he simply places himself where Hobbes placed himself by his attempts at reasoning on exact subjects; and those who, nevertheless, talk of Hobbes as a "great logician" will be free to entertain the same opinion of Wundt--and of Lord Timothy Dexter.

As for the 'Ethics' and the 'System of Philosophy,' we shall simply say that no person of discrimination would prove that quality by ranking them among works of the first order. We say no more, because such deviations from a great career are too unpleasant to comtemplate. Of course, even in the 'Logic' there are brilliant chapters; it could not be otherwise, their author having achieved such things as he had, though in a distant field. As to the 'Physiological Psychology,' there will probably be no break in the unanimity that it is the most important monument of the new experimental psychology. Professor Titchener's translation has been eagerly awaited for long years. He explains the delay in his preface. It appears that he has made three complete translations of the work which have twice been superseded by revisions of the original. He is himself of opinion that his third is the least good of the three, but one does not see how that could possibly be. His unusual skill in making agreeable English of a faithful rendering from disagreeable German had already been proved--a psychological accomplishment which Oxford training, the experience of the psychological laboratory, and practice in this very thing have perfected. It is not comprised in the verbal expression. Unerring judgment has been exercised in the editing both of the present volume and of others. The author's slips, if not too numerous, have to be corrected, with or without mention, according to circumstances. Whether the lettering of diagrams shall continue to represent German words or not, whether or not bad figures shall be replaced by better ones, etc., are questions about which the least talent for judging wrong would have betrayed itself if it had lurked in the translator. The present volume, the first of three, includes only the first and perhaps the most interesting of the six divisions of the original work. It relates to the subject in which Wundt's opinions have the greatest weight; and it is a subject whose practical corollaries will be obvious to every reader--"the bodily substrate of the mental life."

81 (3 August 1905) 97: NOTES

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

The tide of interest in the group of doctrines called pragmatism, anthropomorphism, humanism, radical empiricism, will-to-believe-ism, instrumentalism,

and by several other names, does not seem to abate. There is a "pragmatist club" in Florence, and several members of the review Leonardo have been considerably taken up with a controversy on the subject between Signor Mario Calderoni and a writer signing himself Giuliano il Sofista. Professor Vailati has touched upon the dispute in the Rivista di Psicologia. The last number of the Geneva Archives de Psychologie contains an article written by Professor James in French, and even more charmingly than he writes in English, because more clearly, in which he maintains that the distinction between thing and thought is exclusively functional, and by no means ontological. There are pragmatists who, holding that an ontological distinction is a distinction only so far as it is functional, will see in Professor James's doctrine only a fine elaboration and extension of the theory of immediate perception. It is, at any rate, a fine stone to be added to the edifice the humanists are building up. It is, so far, a house at war against itself concerning not inconsiderable questions; but perhaps this will not endanger its stability, and it certainly renders the discussions more interesting.

81 (7 September 1905) 205: Nos Enfants au Collège.

Par le Dr. Maurice de Fleury. Paris: Armand Colin. 1905. 18mo, jésus, pp. 315.

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

Six years ago, almost to a month, we noticed a volume of fifty thousand words (the present one has not seventy thousand), by Dr. Fleury, entitled 'Le Corps et l'Âme de l'Enfant,' a very sensible and detailed book, readable by mothers, concerning the bringing up of children from three to fifteen, and strongly marked by the characters of a medical man and of a Frenchman impressed by Anglo-Saxon superiority. He was full of Herbert Spencer, in whom he admired a trait which it needed his fine observation to discover, Spencer's wit and humor. It seems that that volume contained the results of studies which the author had been led to make by his having a son of his own; and the same circumstance has caused the present sequel. Dr. Fleury's ways of thinking remain what they were, those of a physician interested in psychology, and given, for example, to going about and asking many people the same question in order to tabulate the answers. He is not at present in quite so admiring a mood toward the Anglo-Saxons. In his former volume he marched under the Anglomaniac banner of Demolins, talked of our "vital superiority," our "vigor of expansion," and so forth. At present, he wishes it distinctly understood that he does not agree with Demolins, and talks of Anglo-Saxons, especially the American breed, as pirates eaten up with jingoism and imperialism.

For the moment, he is strenuously in favor of the suppression of Latin as a general study for boys. He says he could formerly recite the entire second book of the Aeneid, and now could not read it without a dictionary. Does this mean

that if a man has once read a book, and subsequently is for any reason in no condition to reread it and cannot repeat much of it, the reading can have done him no good? He gives about five pages of quotations from a book by André Beaunier, in which we are told that there are only five or six works in Latin that are worth reading, and that the principal reason for learning it is that it enables one to understand the formation of French words. Dr. de Fleury himself speaks as if one of the chief reasons for learning Greek is that it furnishes information of the meanings of technical terms such as "telegram." Really, it does not heighten one's respect for a writer on education to learn that, knowing Latin and Greek, he has found them of no service to him.

The author is alive to the medical side of educational questions; he has read physiological psychology and takes it into account, and he has the physician's skill in dealing with situations that he does not half comprehend. There is much good sense in the book. Thus, it is rightly insisted that the greater part of the labor of mental work consists in getting one's mind riveted down upon the problem in hand. This has been said often enough before. It accounts for a powerful intellect's need of a physical constitution which can go long without sleep or food, the first few hours being unproductive drag. Of course, it is as undesirable as it is impossible that boys and girls should accomplish any memorable thinking; and it is also true that very young children can gather all the mental power in their possession in a few minutes. But this is far from being the case with the big boy. In one hour he will not have got his second wind, he will not have reached the stage of enjoyment of mental work; and if at the end of one hour he is invariably set upon doing something else, it is the inexorable law of psychology that he should look upon study as thoroughly disagreeable. Who could ever write a book if his attention were entirely taken off from it for five minutes every hour? The really fine thinking is done in seconds; but hours must prepare for them, and many more hours must seize upon the product of these seconds and utilize it.

As in his former volume Dr. Fleury gave two highly useful chapters to the study of "l'enfant colère," two to "l'enfant peureux," three to "les paresseux," and one "sur le mensonge," among other subjects of the same order, so here he does not fail to consider the proper plan of treating inattention, the mauvais vouloir, and other things which some people still seem to think beyond the scope of science. His book has three parts: "La Vie Physique," in 44 pages; "La Vie de l'Esprit," in 150 pages, and "La Vie Morale," in 91 pages. It is not a great work; but it is a very agreeable and extremely useful series of talks.

81 (7 September 1905) 205-206: A Treatise on Chemistry.

By Sir H. E. Roscoe and C. Schorlemmer. New edition, completely revised. Macmillan Co. 1905. Vol. 1. 8vo, pp. 931.

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

Carl Schorlemmer (1834-1892) was born in Darmstadt, Germany, and studied at Heidelberg and Giessen. For many years, he was associated with Manchester College as professor of chemistry. Schorlemmer began publication in 1877 by co-authoring with Sir Henry Roscoe the Systematic Treatise on Chemistry. Also to his credit are several other works on chemistry and various translations.

The appearance of the first volume of a thoroughly revised edition, no doubt the last one that will have all the advantage of its author's skill, of Sir Henry Roscoe's 'Treatise on Chemistry,' is an appropriate occasion for noting the value to science of literary culture. The student of chemistry has no small task before him when he sits down so to impress upon his memory all the facts contained in these eight large volumes (as they probably will be, though they may be numbered as three) that each fact is ready at hand at the moment it becomes pertinent. In this undertaking he can receive from no other handbook in any language the degree of aid and comfort that he will gain from "Roscoe and Schorlemmer," because the facts are here set forth very plainly and with no suspicion of artifice, yet in such a way as to make him alive to them to the very end of the twenty-five hours of reading a day which is said to be necessary for the young chemist. They are not only stated as they appear to the experimenter, but they are clothed in good, pure English that does not annoy and does not draw attention from the facts to the words. There is no branch of science in which there is so low an average of general cultivation, or education, as there is among chemists; and yet on the average they write rather well. Perhaps that power of nice manipulation which they must have by nature and by severe training, that clean purposiveness in each muscular contraction and in every designing of an experiment, which renders it a keen æsthetic pleasure to see a good chemist perform--not a show experiment, but a serious analysis--may evidence itself in their use of words. It would certainly be very easy to name some fascinating books of chemistry; but no chemist has directed his literary accomplishments to a more useful office, lowly as ambitious vanity might deem it, than Sir Henry Roscoe has done in the composition of this treatise.

The most embarrassing question for the writer of a chemical handbook, and one which hardly any two have answered alike--a diversity most annoying to those who consult these books--is in what order to take up the elements, both in the main divisions and in the subdivisions. The simplest rule (and the more one considers it, the more one finds to approve in it) would be always to give precedence to the element of lower atomic weight. The effect of this would be revolutionary, no doubt; but it would be a salutary revolution, since it would put organic chemistry--the simpler subject, and in itself, no doubt, much the smaller subject, as well as the better understood subject--before inorganic chemistry, concerning which, in the ordinary treatment of it, the student acquires many ideas (as, for example, about saline solutions) that he has afterwards to unlearn as being exploded. The first compounds of any importance that would, in the proposed arrangement, be brought to his attention would be the hydrocarbons, the compounds of which our knowledge is the most nearly complete, while he would have impressed upon him at the outset the salutary lesson that our acquaintance with chemical substances is extremely slight at best. Then would come the ammonias,

amines, nitrites, etc., that do not contain oxygen; and here, too, we are upon a good solid ground of theory, relatively speaking. Oxygen would introduce him to more difficult questions, which have, however, in organic chemistry, been tolerably well answered. The separation of series of elements, such as F, Cl, Br, I, in this arrangement would force the student repeatedly to review, one by one, the facts that he had already learned, and would thus ensure the accuracy of his recollections. But, of course, the capital advantage would be the simplicity of the arrangement.

"Roscoe and Schorlemmer" suffers as much as any book we know from inconsistencies of arrangement. Its general idea, like that of many other works, is to treat the elements in one column of Mendeléeff's table together. But no writer has ever adhered to that plan consistently. The result of doing so would be too atrocious. In this volume, oxygen follows after iodine. Boron is wedged between arsenic and carbon, simply because, at the time Roscoe studied chemistry, it was supposed to be allied to silicon through their non-volatile, glass-making acids --a circumstance which certainly does not affiliate boron with carbon. Nitrogen, phosphorus, and arsenic are treated in this volume; while antimony and bismuth go over to the third as being metals, although the metal tellurium is allowed a place here. A student who wishes occasionally to refer to this work along with a half-dozen other handbooks, all differently arranged, will be annoyed by the absence of any plain rule of arrangement in any of them.

The revision has been admirably performed. Its thoroughness and accuracy, and the sound scientific judgment shown wherever fact or theory is in doubt, are striking. A careful reading has disclosed but one or two slight errors. The historical statements are particularly careful, though we cannot assent to the credit allowed to Watt and disallowed to Cavendish as to the composition of water. The latter said that the two gases "are turned into water"--an expression of which the scientific caution at a time when there was no evidence whether that which was given off (which we now know to be heat-energy) was matter or not, ought to be commended in contrast to Watt's unreflecting haste. It is absurd to treat his remark as a great discovery. What is supposed to have been the imperfection of the statement of Cavendish? Probably, his not explicitly recognizing that the imponderable something which escaped when the two gases were "turned into" water was not matter. But even Lavoisier in his chemistry, and all the treatises of his school, reckoned caloric among the chemical elements; so that really we cannot see that Cavendish conceived the fact otherwise than Lavoisier afterwards did.

81 (5 October 1905) 286-287: La Nature et la Vie.

Par Henry De Varigny. Paris: Armand Colin. 1905. 18mo, jésus, pp. 356.

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

Although it is the essential aim of physical science to explain phenomena without resort to any hypothesis of a direct action of mind upon matter (not but that

mind does act on matter in any common-sense understanding of the phrase, of course), yet many of us are attracted to certain results of biological study with a quality of interest only to be accounted for as being grounded upon some vague hope of getting light from them upon the mystery of the life we feel within us. It is to such readers that M. de Varigny addresses himself. His nineteen chapters, arranged in seven parts, wear, in the table of contents, a vague air of consecutive thought; and precisely so they appear when one comes to read them. They are composed of anecdotal facts of natural history strung upon a thread of reason without which they would lose half their charm, which is very considerable. In the way neither of observation nor of reflection has M. de Varigny much to communicate that is original; but he tells us much that is recent, and tells it with scientific accuracy.

By simply chatting about a few fair specimens of the work, we shall best convey an idea of its quality. The first chapter discusses the question of how life first came upon the cooling earth. Throughout, the author abstains from explicitly laying down any conclusions. As the advertisement puts it, his pages "n'ont rien d'un traité didactique." But his statement of the evidences pretty nearly amount to acceptance of Kelvin's theory that life was imported upon an aerolite; and perhaps that is the best opinion in the present state of science, though for quite different reasons from these which Varigny commends. To begin with, the author admits, as scientific men mostly do, that Pasteur proved that there is no such thing as spontaneous generation. Yet Clifford demonstrated to the satisfaction of every competent logician that Pasteur only refuted a particular agrument [[sic]], and left the general question untouched. For the hypothesis of spontaneous generation is that, under a certain complex combination of circumstances which may occur in the whole mass of the ocean once in a hundred centuries or so, living protoplasm will be produced; and all that Pasteur did was to show that in certain flasks of broth--say one or two hundred litres in all--no life was produced in several months. If Clifford were to meet a person who thought this a sound inductive argument against the possiblity of spontaneous generation (say, M. de Varigny), the following conversation might ensue:

Clifford--Suppose that a little over a century ago, when the possibility of aerolites was disputed, a man had carefully prepared a quarter of an acre of ground so that nothing could fall upon it undetected, and had closely watched it for a whole day and night without catching a single stone from the skies. Do you think that that would have amounted to solid proof that no aerolite could have fallen in any civilized country during the previous four hundred years? Or do you think it would have been an argument of only moderate value, or how should you esteem it?

Varigny--It would have been a silly performance because of the insignificance of the area and of the duration of the experiment as compared with four centuries.

Clifford--Yet, estimating the value of the argument on that principle, it is hundreds of times as strong as the inference from Pasteur's half barrel of broth to the million and a half millions of millions of tons of sea water and the thousands of centuries of time that are in question.

Varigny--Well, you must admit that Pasteur proved that nobody had ever observed spontaneous generation; and reduced it to the rank of a bare possibility, a mere fancy. Now do you mean to say that scientific men should give up their time to the examination of the host of fanciful things that the idle from time to time amuse themselves by believing in?

Clifford--No; but I think that to argue that because it is not wise for a scientific man to bother with a certain supposed phenomenon, therefore that phenomenon does not exist, is uncommonly bad reasoning.

The probabilities seem at present to be decidedly against the theory that life came upon the earth by spontaneous generation, not because if such a phenomenon were possible it ought to have shown itself in Pasteur's flasks during his weeks of experimentation, but, on the contrary, because, even if the event be possible, the whole mass of the sea and the whole time since the solar system was a nebula fall short by millions upon millions of multiples of being sufficient to render the requisite combination of circumstances probable. For we know, what was not known in Clifford's time, that a molecule of protoplasm is composed of thousands of atoms; and very likely thousands of molecules must concur to produce life. These atoms, too, must all be arranged in certain ways. It would seem, then, though we really know next to nothing about it, that for such a combination to occur by pure chance must require the fortuitous satisfaction of at least a hundred independent conditions. Now how rapidly can we imagine these conditions to be satisfied? If we suppose that in every single molecule of water they are satisfied as frequently as light vibrates, the probability of the fortuitous occurrence of one molecule of protoplasm in the mass of the sea in a million million centuries would be so small that one might call it a miracle. The merit of Kelvin's idea is that it opens our prison doors. We are no longer limited to this puddle of an ocean or to the twinkling of an eye that has elapsed since the solar system was a nebula, but have the whole vast universe of space and time to draw upon, in which it may be that living protoplasm was produced fortuitously in sufficient quantity to reproduce itself. M. de Varigny shows that there is no serious objection to Kelvin's theory. As it is sometimes stated, it seems to make life inexplicable; and that is the greatest logical sin an explanatory hypothesis can fall into. But the true effect of Kelvin's theory is to open a wider field of possiblity.

Among other entertaining chapters are those concerning an indispensable poison (carbonic acid and its effects upon climate), concerning the advantages of degeneration and concerning universal interdependence. In the last we learn that in New Zealand ill-managed lumbering spoiled the oysters, which were attacked by a parasitic worm that increased when more mud was washed down in the rivers. The penultimate part of the work is entitled "Ce que veut la Nature." This was inevitable. This crystallizes the interest which was in solution throughout. But biology, as M. de Varigny rightly says, "will have none of final causes at any price whatever." Now, then, can it tell us "ce que veut la Nature?" It can give no other answer than that which we read in 1852 in a little brown book republished by Ticknor. The poet ought to know "ce que veut la Nature." The man of science ought not to know; and if he does, it is surreptitiously.

81 (19 October 1905) 321: NOTES

CSP, identification: Haskell, Index to The Nation. See also: Burks, Bibliography; List of Articles; MS 1497a(s) (draft).

George William Hill (1838-1914) served in the office of the American Ephemeris and Nautical Almanac. He was educated at Rutgers College, recipient of a Ph.D. at age 27. Hill was the author of several articles on mathematics and astronomy.

--The severest of all touchstones of mathematical skill is universally acknowledged to be the working out of an exact numerical account of the way our satellite performs its intricate motions--the "theory" of the moon, as the mathematicians still call it, after Ptolemy. Well, inquire, say in Berlin, or in Pulkowa, or Paramatta, or Tacubaya, or in any corner of the earth where high mathematics is cultivated, who in our time has shown the most surpassing mastery of the theory of the moon, and the answer of any competent authority will come unhesitatingly, "It is Mr. G. W. Hill of Nyack Falls, N. Y." Had that village been aware of its renown, it might not have changed its name, alluring as the melody of "West Nyack" no doubt is. But Mr. Hill is the reverse of the kind of man to whom the Sunday Herald devotes a page, and it is probable that the villagers know him only as the genial but retiring gentleman who so loves the paternal farm on which he was born and where he still lives. The next most rebarbative problem of celestial mechanics, after the moon's, is perhaps the theory of Jupiter and Saturn (which have to be treated together), and in this Mr. Hill has outdone all other astronomers. But this is as nothing to his achievements in the theory of the moon. For here the method he pursued launched him on an unknown sea, requiring an entirely new chapter to be added to the calculus; and here, by means of the staggering conception of an infinite determinant, he succeeded in the hardy enterprise of virtually solving a differential equation of an infinite order. The boldness of the undertaking consisted in this: that Hill introduced into mathematics a kind of reasoning unrecognized by the mathematicians (albeit they had often unconsciously employed it), namely, the experimental reasoning of physics. For, an infinite determinant being a complete novelty, it was as yet unknown whether the particular type of such a complex series required for Hill's method of solution was convergent or not, or, if it were, whether it possessed the particular kind of convergency that would adapt it to the operations of the calculus. Hill accordingly treated its satisfying this requirement as he would have treated a physical hypothesis, and proceeded to put it to the test of experiment, by calculating, on that theory, the rate of revolution of the axis of the moon's elliptical orbit, which, of all the elements of the solar system, is observationally the one by far the most sensitive to any erroneous assumption about the perturbations. He relied upon the knowledge that if his mathematics were wrong, there was every reason to expect that his calculated motion of the perigee would be sensibly--would be enormously--at variance with observation. It turned out, however, to agree with observation as closely as the results of observation were known. Yet it must be confessed that it is not as clear as the noonday sun that Mr. Hill himself, any more than previous mathematicians, perceived that he was applying Baconian reasoning to mathematics. In any case the brilliant demonstration of Poincaré

was needed to enable future astronomers to apply Hill's method with entire confidence to all problems of three bodies. Nevertheless, when we consider that it would, after all, only be to physical questions that such complicated differential equations would ever be applied, Hill's procedure is seen to be of a piece with all the other reasoning that would go along with it, and therefore logically to be beyond criticism.

--By such means our countryman abridged the labor of certain numerical calculations from months to hours, while vastly increasing their exactitude. Mr. Hill's work upon the Moon, originally published over twenty years ago, has since been perfected in some parts and improved in others by an Englishman, Brown. Still, excellent as Brown's work is said to be, the chief merit of the new method confessedly belongs to our neighbor across the river, eighteen miles above High Bridge. Therefore, with a stately quarto, Volume I. of 'The Collected Mathematical Works of George William Hill,' published by the Carnegie Institution, there came to us a visitor too infrequent of late years--we mean the oldtime glow of exultant American feeling. The volume is prefaced with a long and most interesting account of Mr. Hill and his work from the pen (on the whole the most competent and suitable that could have been selected) of M. Henri Poincaré. It is in French, of course; and we find M. Poincaré writing collège with an acute accent, a practice which is redolent of Nancy as it was before the war, the Nancy of old Dr. Poincaré. The volume falls but little short of being a handsome one; paper and type are good. There is a pretty good portrait of the man; but in the pose of the head, though it is not foreign to Mr. Hill, we see more of the photographer than of his subject.

81 (26 October 1905) 340: NOTES

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

The third volume of Dr. Shields's 'Philosophia Ultima' has appeared posthumously (Charles Scribner's Sons). The whole work is a discourse about science from a mind whose incapacity for scientific thought was almost phenomenal. But he was a man of learning, in a certain obsolescent way, and the work may be used to advantage by others than psychologists, for whom it should be a document. The present volume contains a portrait and a biographical notice by Professor Sloane. One portion of Dr. Shields's share in this volume, dealing with "scientific problems of religion," has a serious value; for those problems need a new treatment which they have not received. There is a very judicious review and defence of Butler's 'Analogy.' The greater part of Dr. Shields's new pages seems to have been written to meet President White's 'Conflict of Science and Theology.' He endeavors to show what an aid the traditional view of the Bible has been to science. As a specimen of the author's candor, we remark that, far from representing that all anthropologists without exception have been decidedly in favor of resting their science largely on Biblical testimony, he expressly admits that

"Leidy, Cope, and Marsh may have left no directly religious testimony." Observe the fine caution of the subjunctive.

81 (26 October 1905) 340-341: NOTES

Attributed to Peirce by Fisch in Second Supplement. Peirce owned a copy of this book, and Peirce and Fine corresponded.

Henry Burchard Fine (1858-1928) was a distinguished American mathematician. He graduated from Princeton University in 1880 and from 1881 until 1884 was a tutor in mathematics there. After taking his Ph.D. from the University of Leipzig in 1885, Fine returned to Princeton as a professor of mathematics. In 1904, he became dean of the faculty, and in 1908, became director of the Princeton University Observatory. Fine was a member of the American Mathematical Society and the American Philosophical Society.

Prof. Henry Burchard Fine's 'College Algebra' (Boston: Ginn & Co.) is the most practical and at the same time the most truly mathematical of all the elementary books on the subject that ever came under our notice. It is quite worthy of the author's illustrious ancestor, who, early in the sixteenth century, came to Paris from the terrific Briançon precipices, so suggestive of all that is difficult and grand, and gave its first impetus to French mathematics. Oronce Fine was his name. Though the new text-book is as elementary and as easy as can be--easier than one would believe possible--it carries the student further than other college albegras [[sic]] do, and it is preceded by an elegantly worked-out exposition of the modern mathematico-logical doctrine of numbers. Professor Fine was a personal pupil of Georg Cantor, to whom (especially through his conception of the "well-ordered" series) our present understanding of this curious subject is almost entirely due. If it has not much to do with algebra, it is at any rate as excellent a lesson in the logic of necessary reasoning as could possibly be found, if it be not better than any other, as Sylvester deemed it.

81 (9 November 1905) 382: NOTES

This note is assigned to Peirce by Haskell (Index to The Nation); however, Christian Kloesel has provided the following convincing argument that Juliette Peirce is the likely author. The draft of this review (MS 1498) is in French in Juliette's hand. In a letter to his brother James in January of 1906, Charles writes, "Juliette reviewed a couple of French novels for The Nation . . . ." Therefore, it appears that the author of this review is Juliette Peirce, with Charles serving in the role of translater and polisher. See also: Burks, Bibliography; Fisch and Haskell, Additions to Cohen's Bibliography.

M. H. Carton de Wiart's 'La Cité Ardente' (Paris: Perrin & Cie) is an historical novel concerning Liège and its surroundings in the time of Charles the Bold. It seems to be built on a rich fund of archæological lore, and towards the end of the volume there is a moving love story which vividly and truthfully portrays a high-bred young lady, sublimely courageous and self-sacrificing to the honor and traditions of her family. The book dwells minutely on the apparel and picturesque scenes of the period, and certainly graves a deep impression of the misery of the turbulence prevailing at a time when, if there was no more wickedness than now, wickedness at any rate raised its head more audaciously. The novel will especially interest a person familiar with mediæval Belgium.

81 (23 November 1905) 417-419: THE NATIONAL ACADEMY OF SCIENCES AT NEW HAVEN

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

NOVEMBER 18, 1905.

The meeting of the National Academy of Sciences in New Haven on November 14 and 15 was perhaps not as full as might have been expected; no quorum of the Council was present, and the number of papers by members was not large, but it may be doubted whether there ever was a meeting in which so much was brought forward that had a flavor of great scientific novelty, inasmuch as the new conception of matter which has resulted from the study of various finds of radiation is now taking on the aspect of a greater revolution in science than did the advent of the doctrine of energy or that of natural selection, and therefore of the greatest since Newton, if not since Copernicus.

The most astounding surprise, though the matter had been rather skeptically heard of before, was the demonstration shown in photographs brought home from South Africa by Prof. W. M. Davis, of the ancient glaciation in that country. Prof. Davis reported that, as far as he heard, all the geologists at the Cape Town meeting of the British Association were fully convinced that the scratches on the rocks and other apparent vestiges of a great glacier sheet were veritably what they had seemed to South African geologists to be. Yet, in addition to some minor difficulties, such as the total absence of terminal moraines, this South African glacier sheet presents two features wholly unprecedented, one of them truly astounding. The former is that this glacier belonged to the Permian epoch, though it has hitherto been supposed that the glacial ages were all, geologically speaking, recent, and indeed have not yet quite come to an end. One is hardly prepared to say why there might not have been glaciers in Permian times; but belief is almost staggered when we learn that this sheet, starting well within the tropics, moved due south toward the Cape. We are reminded, however, that the newly discovered source of energy by radio-active decomposition of elements of high atomic weight is so tremendous that the age of the sun, and consequently the length of geological eras, must probably be far greater than those who have listened to Kelvin and the physicists have been accustomed to think. Moreover, the present view is that the planets generally have performed a half-somersault under tidal action, so as to rotate now from south to east, though at first they turned from south to west. But such a change, by dynamical necessity, must have been accompanied by a vast shifting of the axis of each planet. We are, therefore, tempted to think that there might have been a

time when South Africa was warmer than the parts that are now in latitude 20º S. Somebody ought to examine into this. It illustrates, at any rate, how limitless is the field of consequences which may result from Madame Curie's discovery.

We cannot be surprised at finding that young men are the most alert in assimilating the new conceptions which old and young are now driven to admit, but the old with some not very definite reserve. Dr. B. B. Boltwood, who was introduced by Prof. H. L. Wells, exhibited a curious table showing calculated ages of certain minerals in millions of years. Here, again, we have to remember that there is no longer any solid reason for refusing to acknowledge the lengths of geological time upon which the geologists have always insisted, nor, indeed, if the affirmative reasons are sufficient, the still vaster durations demanded by the palæontologists. It now appears that helium is set free in all radio-active decomposition and in point of fact the atomic weight of uranium exceeds that of thorium, as nearly as we know, by twice the atomic weight of helium, and exceeds the atomic weight of radium by three times the same amount, as if $U = Th\ He^2 = Rd\ He^3$; and radium, which undergoes five successive radio-active decompositions, has an atomic weight exceeding that of lead by five times the atomic weight of helium, as if $Rd = Pb\ He^5$. For this and another weighty reason, Dr. Boltwood suggested last spring that the final product of the decomposition of radium is lead, an idea which has been received with favor. The other reason to which we allude is, that the proportions of lead and helium in the radio-active minerals are correct if we suppose that they were all pure thorium minerals at first; and in point of fact all do contain thorium. It is singular that the rate of each kind of radio-active decomposition remains absolutely constant at all pressures, and at temperatures ranging from that of liquid air to the highest temperatures that have been tried. Nothing seems to disturb its march. On this basis, then, Dr. Boltwood has determined, from the amounts of lead and of helium per gramme of the mineral, the length of time required to produce that amount for all the minerals for which the calculation is possible--a long list from various parts of the world; and the truth of the calculated times seems to be decidedly confirmed by the calculations turning out to give all the minerals from any one region, however different they may be in composition, pretty nearly the same age, although minerals from different regions show decidedly, and sometimes vastly, different calculated ages. Thus, the calculated ages of five minerals of Connecticut range from 92 to 98 millions of years, those of North Carolina 119 to 127 millions of years, and those of Norway from 290 to 383 millions of years.

Stupendous as these periods of time are, it does not appear that the physicist can any longer deny them, and they appear to receive some support from the southward motion of the glacier sheet of South Africa.

There is, however, one point of serious doubt. Many chemical reactions proceed with extreme slowness at ordinary and lower temperatures, but, when the substances are heated to a certain point, they suddenly advance with great rapidity. Now experiments upon radio-active decomposition have never been made above 200 degress C, which is below the heat of a good baker's oven. It is, therefore, quite possible that at some higher temperature this decomposition should proceed very fast, especially if certain other conditions that might be pointed out should be realized. For the present, this consideration must throw a not inconsiderable doubt over Dr. Boltwood's calculations.

Dr. H. A. Bumstead, who was introduced by Professor Hastings, is a Yale graduate, already favorably known to the scientific world, who has been working for a year under the guidance of Prof. J. J. Thomson, the chief promoter of electrons and all these ideas, in the Cavendish Laboratory. His paper greatly impressed the physicists by its refinement and cautious attention to every source of error, although it is impossible here, in short compass and without drawings, to give any idea of its merits. When the Röntgen rays strike upon metals they liberate a quantity of energy considerably in excess of that which went to their production. This excess of energy can be due only to the radio-active decomposition of the metal, although it would be quite impossible to detect, by chemical analysis, that any change had taken place. We have been accustomed to think of the liberation of energy in combustion as very great; but that which is liberated in radio-active decomposition is millions of times greater. Dr. Bumstead has taken a pair of elements, zinc and lead, the one of low atomic weight, the other of high, and, by measuring the difference of energy resulting from the action upon them of the Röntgen rays, studied the radio-active decomposition.

Another young man, known for excellent work in collaboration with Dr. Bumstead, Dr. L. P. Wheeler, introduced by Professor Hastings, gave an excellent paper and one very convincing of the value both of Professor Hastings's leadership and of the influence of Willard Gibbs. It was a mixed mathematical and experimental research, admirable in both directions, into metallic reflexion, the polarization-effects of films and of scratches, etc.

Another paper connected with the new conception of matter, and indeed opening a rich field of suggestion concerning electrons, was by one of the leaders in physics, Professor Trowbridge. He described, and illustrated by photographs thrown upon the screen, the singular effects which he had produced in large tubes traversed by a current from a battery of twenty thousand cells of great resistance. When sparks are used, we get only instantaneous effects, but, with such a continuous source of high-tension electricity, slow alterations are observed, occupying minutes in their performance. A volume-ionization is manifest, which slowly increases, and also an ionization-lag. There is a decided unipolar discharge. With some arrangements, rotating brushes are seen, together with phenomena which decidedly resemble the shifting of the Northern lights.

A paper, perhaps indicating an unrecognized state of matter, was by the veteran Professor Brewer. He had formerly given some account of his observations upon the deposition of sediment from water in which clay had been stirred up. But, the vessels not being transportable without disturbance, it was not until the Academy came to New Haven that he could actually exhibit his results. He had several two-gallon bottles with vertical sides, containing clay and pure water, which had not been shaken for fourteen and for twenty-two years. Yet an opalescence was still visible in them, and Professor Brewer was of opinion that the clay never would completely settle. It seemed to be in a colloidal condition, in semi-chemical union with the water, and diffusing through it. But Professor Brewer informed the Academy that if a minute quantity of salt water were to be added, the water would completely clear itself in a few days. To this paper Professor Brewer appended another with specimens demonstrating that wood will never rot under the influence of pure air and moisture, even if considerably heated. If, however, the smallest particle of rotten wood be introduced into the vessel, the rot will soon spread through all the wood present.

There was one exceedingly interesting paper upon mineral chemistry by Professor Penfield, the well-known mineralogist. It seems that some time ago he received from a correspondent a package of small crystals found at Borax Lake in California, the sender remarking that they contained magnesium and sodium in the form of carbonates and chlorides. One crystal, being taken at random for analysis, was found to have the described composition, with the singular difference that, instead of chloride, it contained sulphate. Now every student of chemistry knows that a sulphate cannot replace a chloride without a complete change in crystalline form; yet all the crystals were alike. It was, therefore, at first supposed that some blunder must explain their having been described as containing chloride. However, further examination showed that of the crystals which remained after taking away that one, some seven thousand in number, not one contained any sulphate at all. After that, one other crystal was met with which contained sulphate and no chloride. The formula of the first and last was

$2MgCO^3$, $2Na^2CO^3$; Na^2SO^4;

That of the other seven thousand was

$2MgCO^3$, $2NA^2CO^3$; $2NaCl$.

The crystals containing chloride were somewhat soluble; those containing sulphate were insoluble. Moved by a rule of old-time chemistry about the precipitation of insoluble salts, Professor Penfield imagined that though the sulphate crystals were so rare in nature, it might be easy to crystallize them from a mixed solution of their ingredients. It proved to be far from easy, but was accomplished by five days' digestion over a bain-marie. There were, then, two independent phenomena to be explained: (1) the isomorphism of the sulphate and chloride compounds; and (2), the difficulty of formation of the sulphate salt, with its consequent infrequency in nature.

We now pass to the physiological and anatomical papers. The very first memoir read was an elaborate piece of work by Dr. L. B. Mendel, who was introduced by Professor Chittenden. It related to the presence of Sucrase, Maltase, and Lactase in the tissues and liquids of pigs and dogs in several embryonic stages and in the breathing animal. The details, though interesting, would require too much explanation to be given here. The differences found at different stages of development doubtless depend upon such facts as that the embryo-muscle has no work to perform, and that the embryo does not so readily get rid of substances which it can neither absorb nor transform, as the adult does.

Dr. F. E. Beach, introduced by Professor Hastings, dealt with the errors of eccentricity and collimation of the human eye. He corrected certain errors of Helmholtz, and showed how they had arisen. The axis of distinct vision is inclined some 3° or 4° from the axis of the cornea, which has not the form of a portion of a spherical surface. A person looking into an eye which looks straight into his will see three reflections--one from the cornea, and two from the two surfaces of the

crystalline lens. It was to the departures from the character of an ideal optical instrument which are betrayed by the locations of these reflections that Mr. Beach's measures and calculations related.

The paper of the session was unquestionably that of Prof. Edmund B. Wilson, who attacked one of the great traditional mysteries, the problem of sex-determination, and distinctly brought it one long march nearer to elucidation. His admirably clear diagrams, drawings from microphotographs of wonderfully prepared specimens, showed groups of bodies known as chromosomes, which are found not only in the cells immediately concerned in fertilization, but also in the cells from which these cells are formed by reduction. The animals from which they were taken belonged to six different genera of bugs. Three of these genera have, in each cell of the ovarian follicles from which the egg-cells are formed, a fixed number of chromosomes. This number is even without being divisible by four (such as 14 or 22), consisting of an odd number of pairs, the chromosomes of each pair being alike in size and general appearance, while the different pairs differ more or less in these respects. In particular, two chromosomes, in every case, are vastly larger than any of the others. These two have hitherto been called the "accessory" chromosomes; but Professor Wilson, having proved the gross impropriety of this designation, renames them the "heterotropic" chromosomes. Exclusive of these two, the number of chromosomes is divisible by four (as 12 or 20). The corresponding cells in the male contain each one chromosome fewer (as 13 or 21), since they possess but one of the heterotropic kind. The ordinary chromosomes in the male correspond exactly, one to one, to those of the female. From each of the cells of which we have been speaking, is formed, in each sex, two cells of the kinds directly concerned in fertilization. In this process of division the two members of each matched pair of chromosomes separate and go, the one to one of the new cells, the other to the other. But since the cells of the male contain but one heterotropic chromosome, half of the new male cells contain one, and the other half none at all. In the act of fertilization, one male cell is absorbed in a female one, which thus comes to contain the chromosomes of both; namely, a number of ordinary chromosomes divisible by four, and either only one heterotropic chromosome, which is the characteristic of the male, or two of these, which is the characteristic of the female.

This is what happens in three of the genera examined. In the other three there is a slight difference, in that the original cells of the male, instead of containing ordinary chromosomes equal in number to those of the male together with a single chromosome which is a good deal larger, contain, beside all those, an additional chromosome which is a good deal smaller than the ordinary ones. This little chromosome acts like the mate to the big one, in that, in the division of the cell, it always goes to the opposite one of the new cells; that is, to that one of the new cells which the big one does not enter. If we leave the little chromosome out of account, the phenomena of these genera are just like those first described. Professor Wilson entertains no doubt that it is this second arrangement that is the primitive one. These facts seem to show that the determination of sex takes place at fertilization, wherein a signal addition is made to our knowledge of the subject. Still, Professor Wilson does not believe that the chromosomes are the direct determinants

of sex, but rather that they are concomitants of that determination, which he thinks may be a matter of metabolism, perhaps of growth.

Mr. Agassiz, the President of the Academy, as usual wound up the session with one of his delightful papers, which are always followed without difficulty, and are always full of interest. It related to the various forms and functions assumed by the spines of sea-urchins. We have not mentioned three mathematical papers by Dr. Franz Boaz, Mr. C. S. Peirce, and Prof. Asaph Hall. Professor Hall's paper related to the calculation of the anomaly of Halley's, or any similar, comet.

The meeting was an exceptionally enjoyable one; and the New Haven members gave the Academy such a dinner as is not often offered to mere scientists.

81 (14 December 1905) 486-488: GOSSE'S SIR THOMAS BROWNE

Sir Thomas Browne.

By Edmund Gosse. (English Men of Letters.) Macmillan. 1905. 8vo, Pp. 207.

CSP, identification: Haskell, Index to The Nation. See also: Burks, Bibliography; List of Articles; MSS 1497, 1497(s), 1499, 1499(s) (drafts).

Sir Edmund William Gosse (1849-1928) was a British poet and man of letters. He is credited as having been the first writer to introduce Ibsen to the English public. Gosse served as Clark Lecturer in English literature at Trinity College, Cambridge, from 1885 until 1890. He was knighted in 1925.

No other English writer, unless one of the sublime few or one linguistically important, has received the lifelong devotion of two such editors as has the author of the 'Religio Medici' in the labors of Simon Wilkin and of Dr. W. A. Greenhill. You may not care for Sir Thomas Browne; it is conceivable enough that you should find his meandering stream to be tiresome; but if you do like him, you must personally love him. The warmth of affection one feels is his just due would be ill-bestowed upon Montaigne and uncalled-for toward Charles Lamb or toward Horace. How unlike one another these four writers are, and yet how strikingly set apart from others by the personal feeling they inspire! Browne tells us he never read above three pages of Montaigne; and nobody can wonder that the most extreme of believers, who held that Divine Omnipotence could override the principle of contradiction, should not be attracted to the extremest of skeptics.

Any lover of Browne carries a pass-key to the heart of any other; and if Mr. Gosse should only display such a badge, he may be sure that any mistakes he may commit will be lightly dealt with by the present reviewer. But, after a first perusal of his volume in the kindest spirit, attention having been focussed on the questions he revives, we re-read the whole of Browne, together with sufficient proportions of the five or six writers who seem most comparable with him--Boyle, Digby, Henry More, Charleton, Gui Patin, and Samuel Parker; and now, returning to a more deliberate study of what Mr. Gosse has to say, find our general impression to be that, instead of striving to put himself into his subject's shoes, as we, for example, conceive ourselves obliged to put ourselves into his, he rather struggles to

squeeze Browne's Alcidian foot into his own pump. You will find examples of this wherever you open his volume. Here is one. Imagine, if you please, Edmund Gosse rapping the author of 'Christian Morals' over the knuckles for not bearing in mind his Vaugelas (p. 197)! The 'Ramarques' of that purist did not appear until Browne had already published everything of importance that he was to publish. But the incongruity of the anachronism is nothing compared to that of tying down an English writer whose style was formed before the Great Rebellion, to the rules of French rhetoric! Expunge from Milton's epic whatever would have been out of place in Voltaire's, or even, if you will, whatever Shakspere wrote that Racine would not have written even if he could; but do not tell Browne to write according to Vaugelas!

On the page opposite to this, a sentence from the metaphysical part of the 'Religio Medici' is brought up for censure. This portion of Browne's masterpiece which, be it not forgotten, was written before the publication of any work in historical continuity with modern philosophy, is not a little curious--curious, and something more than that--but has never received the attention it deserves. More than one passage sounds almost like an anticipation of Hegel, but was really inspired, we must surmise, by dialogues of Giordano Bruno (which were published in England), whether Browne read them in Oxford, or, perhaps more probably, caught some reverberation of them in Padua. The sentence in question is this: "God, being all things, is contrary unto Nothing, out of which were made all things; and so Nothing became Something, and Omneity informed Nullity into Essence." Mr. Gosse's blame falls upon the word "omneity," which is somewhat rare, it is true, yet too clear to arrest attention. But when Gosse says that "oneness would have been better and simpler," the nonsense this proposes at once convinces us that no better word than "omneity" could be found to take its place. Besides, it had, after all, been long in use, and nothing short of an English Vaugelas (which, thank Heaven, never existed) could make it taboo. There is a school résumé of the 26th chapter of Metaphysics {D} in the form of an equation: "Unity + Omneity = Totality." Mr. Gosse would naturally first try substituting "totality," and it was perhaps on finding that that word would not do at all, that he inconsiderately set down "oneness."

Yet we must acknowledge that "oneness" would indeed have been "better and simpler" if Mr. Gosse's apparent theory of Browne's prose had been correct, namely, that it consists in "wrapping the trite in the coronation robes of fine language" (p. 197). In another place, he finds its secret in the use of "extraordinary words to heighten the effect of ordinary thought" (p. 180), or, as we may probably add, to conceal its vacuity. In short, he makes the last section of the 'Garden of Cyrus' the type of Browne's style. Unquestionably, it was a habit of Browne's to throw out now and then a somewhat unusual word. The infallible effect of such a word is to stimulate the reader's attention; but, whether it be desirable or not for a writer to employ this device, depends upon what he has to say. If the expenditure of energy will be repaid to the reader, and the word be not in itself distasteful to him, which would seldom happen in the generation of Browne, the unusual word will serve its purpose. But there are writers whose unerring instinct counsels them to shun a word whose effect would be the last they ought to desire. These writers wish others would do as they do. Browne's device, as he handles it, is of excellent effect with a reader who has acquaintance with pre-Rebellion English; for Browne, living in remote Norwich, continued all his life to write very nearly the language of his youth. Moreover, he has, more than most writers, a vocabulary peculiar to himself.

Mr. Gosse, as a student of our older literature, must know better than the rest of us how greatly the current vocabulary of books has changed since 1635, when the 'Religio Medici' was written. He certainly knows, too, that, so far as good usage can sanction any peculiarity of style, the usage of that day justified the filling out of the English vocabulary with new words drawn from the Latin. We are therefore puzzled to understand most of his verbal criticisms. Thus, he finds fault with the following from the second paragraph of the 'Christian Morals':

"Consider whereabout thou art in Cebes's Table, or that old Philosophical Pinax of the Life of Man; whether thou art yet in the Road of uncertainties; whether thou hast yet entred the narrow gate, got up the Hill and asperous way, which leadeth unto the House of Sanity, or taken that purifying Potion from the hand of sincere Erudition, which may send thee clear and pure away unto a virtuous and happy Life."

He pitches upon the use of the word "asperous" instead of what he does not tell us, but it meant rough, and harsh, or severe; and seems to reckon it as "one of Browne's clumsy audacities." But it was a common enough word. Dr. John Fitch asked a blind man who could distinguish colors how he did it. Boyle gives the answer "in the doctor's own [oral] words," beginning: "Black and white are the most asperous and uneven of all colors." Archbishop Parker, whose English enters into the Book of Common Prayer, employs the work in his psalter. The Oxford Dictionary, among nine examples of it ranging from 1547 to 1880, has one dated a year or two before Browne wrote, in the phrase "a craggy and asperous ascent"; and an elder contemporary of our doctor, Montague by name (but whether that one whom Mr. Gosse deems a far better writer than Browne, we do not not know), has "the asperous and narrow way of the cross." In short, "asperous" applied to a road or path seems to have been as familiar a phrase in Browne's day as "asper" in the same connection had been a century earlier. The majority of Mr. Gosse's strictures upon single words used by Browne are as unlucky as this.

Mistaken as such criticisms are, they contain nothing at which any lover of Browne need take offence; for we are bound to say that each rude expression is accompanied by some indication that it is meant to be understood as somewhat exaggerated. For example, Mr. Gosse does not flatly assert that Browne does wrap commonplace in coronation robes. He says that that is his "rock ahead"--which is the phrase of a teacher of rhetoric correcting his pupil. Such qualifications bring odious comparison within bounds. To our mind their effect remains substantially as strong as if the qualifications had not been appended.

But remarks there are in the book, and many of them, which inflict a sensible wound upon the heart of a lover of Browne. Such a person believes in him so unreservedly that the good doctor's belief in his religion alone outdoes it. He knows the learned knight, just as one might know a neighbor, for a gentleman in whom was no trace of snobbery, and who, with an innocent satisfaction in his own acquirements, never showed any concern as to what strangers might think of them, and was quite untempted to bolster up pretension with deceit:

"Not tied unto the world with care Of publick fame or private breath."

Here again Mr. Gosse fails to stand in Browne's shoes. He seems to belong to that numerous class of persons, many of them active church members, who no sooner learn that a man is devoted to physical science or to modern learning, or has a trained reason, than they straightway become immotibly rooted in the conviction that, whatever he may profess, that man is not a Christian. As long as theologians had the whiphand, a certain weak presumption to that effect there was. Browne, however was not a genuine scientific man; for though he admitted the circulation of the blood, but not the unguentum armarium and the like, and was neither a medical obscurantist, like Gui Patin, nor a medical phantast, like Charleton, yet he was an anti-Copernican, which, in regard to his relations to the world of science, meant everything. Of modern learning he had little or no conception. Nor was he by any means a dialectician. He probably relied in his practice of medicine, as in that of religion, upon what in the latter field he termed "faith"--that is, an intellectual habit which it seemed to him absurd to call upon to justify itself, which seemed to him an evident cognition of things not seen; and experience shows that, in practical matters, men in whom such instincts are robust can lean on nothing less likely to betray their trust.

Of all innuendoes, none are so hard to meet as those of insincerity in religious faith. None are more relished by lovers of innuendo, none more detested by haters thereof. A man so simple and straightforward in all his life and dealings, so universally beloved by his fellow-citizens, the ransacking of whose papers has brought not the smallest double-dealing to light, would be secure against this species of attack if any man were secure; but no man is. Mr. Gosse simply interchanges Browne's chief characteristic with one we can but seem to discover in himself, when he says (p. 28): "We detect a cunning in his apparent innocency," and when he says (p. 31), "Whenever Browne is particularly chatty, we shall find that he is concealing something," and in other passages to similar effect. When he asserts (p. 25) that "the mind of [Browne] had a curious mixture of directness and tortuousness, which disguises" no matter what "from all but the most careful reader," the directness alone is a real character of Browne; the tortuousness and mixed nature seem to be accounted for only as reflections of Mr. Gosse's own mental physiognomy. Although he has admitted that the 'Religio Medici' was written by Browne for his own eye alone, as he was obliged to admit it, this does not prevent him from saying (p. 27) that Browne opens the book "with series of statements which are intended to ward off discussion and to rout suspicion"; and (p. 29) "he makes his confession

rather glibly in order that, under the shelter of it, he may insinuate some more subtle reservations," and (p. 31) "under cover of . . . he now insinuates . . ."--with a dozen more such innuendoes. In short, Mr. Gosse intimates that the 'Religio Medici' is one long and cunning lie--not a very heinous lie in his own eyes, but one that should have been seen by Browne to be ignoble and contaminating.

Mr. Gosse penetrates very little into Sir Thomas's works, or we may as well say into the 'Religio Medici,' his one immortal procreation; for we should not very warmly protest against his other works being described, in the phrase of Mr. Gosse, as "pellets of sun-dried pedantry," having carbuncles sparsely scattered among them. But when we think of Browne as a writer, it is the author of the 'Religio Medici' we mean. At the time he put pen to paper, the 'Consolations of Philosophy' was a popular book; and doubtless the heart-bleedings of Boëthius helped to suggest the 'Religio Medici.' But Browne, being moderately wealthy at that time, thought of religion, in its consolatory aspect, less as a buckler against the adversity of which he never had any experience, than as unveiling the sweetness of Death. He is the poet of Death; and the melody of his minor-key prose is adapted to expressing the secret to which he continually recurs, as the jig of verse and rhyme never could be bent to doing. To him religion meant, above all, the gospel of a future life. Of those three topics which Kant says most concern us, God, Liberty, and Immortality, mere philosophy, to Brown's mind, guaranteed the first, while ethics postulated the second. His profession incessantly pressed the third problem upon him; and hence, as Mr. Gosse well notes, he seldom smiles and never laughs. The phenomena of death-beds suggested that the great change was as accidental as any other stupor; the anatomy of the cerebrum, even before the days of microscopes, seemed to put before him an amazing multitude of similar elements, ungrouped into differentiated organs, and thus suggested that the soul was "inorganical." That was as far as his controlled and critical reasoning could carry him. But the iatric profession, as exercised by the country practitioner, who has time, like Dr. Browne, to breathe a prayer for all in the sick house when first he crosses its threshold, is calculated to make him aware of the irresistible force of the instinctive belief that seems to give him the grand entry to the presence of the truth that binds human life to eternity. Thus we have proposed a different solution of the problem of the 'Religio Medici' from that proposed by Mr. Gosse, which it would be a shame to allow to go untraversed.

The first chapter of Mr. Gosse's book, concerning "Early Years," sheds some light upon the subject. We will jot down a few points. As to the enigma of how Browne could have spent three years in Continental travel if his fortune was only the lesser part of £1,500, the bearing of the opening sentence of §77 pf Part II. of the 'Religio Medici' has been overlooked in this connection. It is plain that money came to him in some wholly unexpected way; possibly, for example, in a legacy from an Oxfordshire patient. Such a fact may yet be discovered. When Browne says he was born in the eighth climate, Gosse explains (p. 8 n) that this means in 8° of latitude. But it is the first climate added to Ptolemy's seven, called ultra Mœotidis paludes, extending from 50° N. to 56° N., and therefore covering England. The general tone of the account of Montpellier is somewhat too favorable, and Gosse's laudation of the severity of the examinations for the doctorate should be confronted with the third intermède of the "Malade Imaginaire," where the bit of examination given must be supposed to have the degree of resemblance required in a burlesque. That it really had far more truth than that, is shown by its agreement with Locke's account of his visit to Montpellier. Sir Kenelm Digby's sympathetic powder is called an anodyne. It consisted of calcined blue vitriol. It was specified that it should be Romans vitriol; but Boyle, in his 'History of Mineral Waters' (sect. iv., art. 9), says that this was pure or nearly pure sulphate of copper, or, in his own terms, vitriol of copper.

Although it is the melody of Browne's prose which constitutes his entire value in Mr. Gosse's judgment, yet he makes no attempt to analyze its mechanism; and perhaps he may be excused, seeing that we are not yet so much as agreed upon the nature of the accent of English words, though it is easy to see that this is not a mere affair of stress. The somewhat nonsensical ending of the 'Garden of Cyrus' it is easy to see is composed of irregular quantitative verses; but this is not the whole story.

Though Somnus in Homer II be sent to rouse up Agamemnon, I find no such effects II in those drowsy approaches of sleep.

To keep our eyes open longer II were but to act our Antipodes.

The huntsmen are up in America II and they are already past their first sleep in

Persia, But who can be drowsy at that hour II that freed us from everlasting sleep?

And so on. In the 'Christian Morals,' the imitation of the balance of the Psalms is obvious.

1906

82 (18 January 1906) 61: Radio-Activity.

By E. Rutherford. Second edition. Cambridge (Eng.): University Press; New York: Macmillan. 1905. 8vo., pp. 580.

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Ernest Rutherford (1871-1937), Baron Rutherford of Nelson, was born in New Zealand. In a competition for a scholarship to Cambridge, Rutherford finished second, but went on to the University when the winner decided to remain at home and marry. This was a fortunate turn of events for science, for Rutherford, under the guidance of J. J. Thomson, brought forth many great discoveries. He is credited with having discovered alpha and beta rays (now called particles), the radioactive half-life, and the proton. In 1908, Rutherford observed the scattering of alpha particles when passed through a sheet of gold, which led him to evolve the theory of the nuclear atom. For this work, he was awarded the Nobel Prize in chemistry in 1908. He was knighted in 1914 and is buried in Westminster Abbey near Newton and Kelvin.

A throng of public sensations due to advances in natural science crowd upon a sexagenarian's recollection (Stewart's syrup was the greatest personal sensation of his infancy), e. g., vulcanized rubber; daguerreotypes; the telegraph; Dr. William T. G. Morton's demonstration of the anæthetic property of ether (and the ether wonder was not a week old when news came of the discovery of the planet Neptune), the same memorable year bringing straw paper, gun-cotton, and the sewing-machine; the stereoscope; then, the doctrine of the conservation of energy, long debated, though at first pooh-poohed by scientific magnates; the mechanical theory of heat; the Ruhmkhorff coil; Foucault's pendulum experiment; Bessemer iron; aniline dye-stuffs; the Atlantic cable; wood pulp; Pasteur's refutation of spontaneous generation as an ordinary event; spectroscopic analysis with rubidium and cæsium; the theory of natural selection; Deville's aluminium; the extensive use of nitroglycerine; Andrews's discovery of the critical temperature; the wonderful Holtz machine of 1865 (the simultaneous and equivalent Töpler machine somehow being less noised abroad), and, three years later, the Gramme dynamo; in 1869 Mendeléeff's periodic law; then a long calm, hardly broken by such successes as that made by the ammonia soda process, or such half successes as Loomis's wireless telegraphy, which, however, came into wider notice in 1877, when the telephone and phonograph had turned public attention into that channel; the first confirmation of Mendeléeff's law in the discovery of the metal gallium, duly melting, according to prediction, in the warmth of a man's palm; the azodyes; Pasteur's germ-theory, followed by Koch's detection of the tubercle-bacillus, and, later, by the enzyme theory; osmotic pressure; the incandescent light; stereochemistry; Weismannism; Cowles's aluminium; smokeless powder; kodaks; the new physical chemistry guided by Willard Gibbs's phase rule, and leading to liquid air and hydrogen; the successful linotype (though that can hardly be reckoned as a scientific sensation); Hall's aluminium; the electric furnace and acetylene; argon; the Röntgen rays; the flutter about Herzian waves; the contact

process for the manufacture of sulphuric acids and anhydride; and, latest, radio-activity and radium. Of all these the last promises to mark the deepest revolution of scientific conceptions, by reducing matter from the rank of primordial substance to that of a special state of electricity. After that, we shall be prepared for anything, even for experimental demonstration of the tychist's doctrine that electricity is a psychical phenomenon.

To any person who wishes to be thoroughly informed concerning radio-activity, the above-cited complete digest of all that is known about it, worked out as it is to the utmost secure conclusions of a general kind, is entirely indispensable. It is, to be sure, only the second edition of a work of which the first edition seemed as perfect as possible, and follows that first edition by but fifteen months. Nevertheless, it is largely rewritten; and it would deserve notice if there were nothing more remarkable about it than that, though the preface is dated the 9th of last May, the contributions to its science made in April are so fully discussed that it is manifest that a considerable part of the rewriting of the first edition must itself have been again rewritten while the new edition was going through the press. The fact that the second edition is almost a new work, although the first edition was everywhere hailed as most remarkable, simply evidences the wonderful advance of the science in which Professor Rutherford is himself so large and active a factor. The methods he pursues are wholly novel, though they rest on familiar and indubitable principles, and his conclusions are not open to intelligent doubt. Accordingly, we are to know that all elements whose atomic weights exceed that of bismuth, 208, are endothermic compounds, which are undergoing spontaneous dissociation accompanied with a liberation of heat energy millions of times as much as that which could be due to the combustion of as much matter. This decomposition seems to consist in the separation of helium, whose atomic weight is 4, leaving an element whose atomic weight is only 4 less than that of the element first taken. However, as to the products of the disaggregation, it is not likely that any one rule will cover all cases. We simply mention what seems to be the prevalent type.

Radium is a metal, extremely like barium, except that its atomic weight is about the sum of those of barium and of strontium, and except that the average life of an atom of radium is only about two thousand years. It casts off helium, and what is left is a chemically inert gas, somewhat like xenon.

The average life of the atom of radium emanation is only five days. It decomposes, and its principal constituent, called Radium A, is left as a deposit on the surface of the vessel. The average life of the atom of Radium A is only four and a third minutes. It is converted by throwing off helium or something like that into a somewhat more volatile element called Radium B, of which the average life is half an hour. Radium B is decomposed in a different way, little studied as yet, into another, less volatile element, Radium C, whose atom has an average life of 40 or 41 minutes. This gives off, not merely such rays as radium itself emits, but, besides others, great penetration, and there is left another element, a little more volatile, called Radium D. The average life of the atom of Radium D is over half a century, and in its decomposition no rays at all are given off, and the very non-volatile element, Radium E, remains. This has an average life of nearly nine days, and, in decomposing,

gives off only the more penetrating rays, none of those by which radium affects the photographic film, and is converted into the first of the new elements discovered by Mme. Curie, polonium, which has an average life of ten months. But we will not pursue these vicissitudes further. Polonium is an element very much like bismuth. Dr. Bollwood holds that all these changes come to an end with the production of lead, but we cannot understand that in an absolute sense; and it is not particularly unlikely that lead should be converted into gold. In fact, the tales of the philosopher's stone and of projection do not seem to-day half so marvellous as what we may see at any moment by looking into a spintharoscope.

The index, though it is not quite as full as we could wish, fills twenty-two pages of double columns.

82 (22 February 1906) 160-161: ALFRED RUSSEL WALLACE

My Life: A Record of Events and Opinions.

By Alfred Russel Wallace. Dodd, Mead & Co. 1905. 2 vols. 8vo, pp. 435, 464.

CSP, identification: Haskell, Index to The Nation. See also: Burks, Bibliography; List of Articles; MS 1503 (draft).

One of the scientific notables of the past forty-five years, a man to whom Darwin could write (and in substance often did), "I wish I had your power of arguing clearly" (vol. ii., p. 11), and that in reference to the very theory which his own radiant argument had illumined, must evidently be a spirit of no ordinary force; and his opinions, of which he has set forth a greater number than most scientific men find time to mature, make a more curious collection than all the rare birds and butterflies he sent home from the islands of the East, the famous Semioptera with its panache included. He believes in paper money as a standard of value, in national ownership or all the land, in Socialism, seemingly in astrology (ii., 335, bis), and unquestionably in full-blown spiritualism. If we are not mistaken, he is opposed to vivisection, as he emphatically is to vaccination, enforced or voluntary, to interest on money, to all inheritance and testamentary disposition of property. He scorns as utterly uncritical the modern scientific determinations of centres of psychical function in the cortex of the brain--not merely the work of Flourens, but also the later attempts of Broca, Munk, and others; and in this he is not so very far from the general opinion of students of the subject, who have at the most yielded but a hesitant and provisional assent to any one attempt to characterize the distinctive functions of the different regions of the brain. On the other hand, he warmly espouses the old phrenology of Gall and the bumps of the travelling lecturers of the forties. These paradoxes are defended by him with all the conviction of his reason, and more. He believes in all that he believes down to the very soles of his boots; and his arguments are mostly so surprisingly strong that some one of his works, say his 'Studies, Scientific and Social,' ought to be made the basis of a course of lectures on logic. Happy would be the university which

should find itself equipped with a professor of logic really capable of dealing with his text.

As to Darwin's encomium, it does not stand alone. John Stuart Mill, Herbert Spencer, Sir Norman Lockyer, Huxley, John Fiske, Chauncey Wright--in short, almost everybody whose judgment concerning the logic of science had any particular value--have ranked Wallace among the past masters in scientific argumentation; yet his narrow training has rendered him an easy mark for whatsoever evil spirit there may be, personal or not, that beguiles men into sophistries, confusions, and rash assumptions, and it perhaps goes far to explain his willingness to serve as an instructor of the public on original lines in such a vast curriculum of subjects. He tells us (ii., 39) that he has "a positive distaste for all forms of anatomical and physiological experiment," and that he never even saw a dissection; nevertheless, biologists attach great weight to his conclusions about the distribution of animals, the classification of the races of mankind, etc. He has to have translations made for him from the German--and Malay, which he speaks fluently, will hardly be reckoned as an equivalent; but many a naturalist of good sense would doubtless be glad if he could exchange all his knowledge of German for half of Wallace's acumen in balancing scientific evidences. Wallace is an Oxford D.C.L., as well as an LL.D.; in this exceptional case the degree of D.C.L. really effected something, namely, it showed experimentally that, for all his paradoxes, even a university which is above all else orthodox, which would shiver at the bare idea of being paradox, or so much as paraclit to paradoxy, perceived that to "honor" Wallace in show would be to honor herself in deed. Bearing all these things in mind, and knowing well that Wallace never wrote a dull line in his life, and couldn't if he tried, his very tables and diagrams being as entertaining as they are valuably instructive, our reviewer, we will answer for him, was not a little curious to read this autobiography and to discover what schooling of child and man had produced this conglomerate personality.

In 1823, George IV. being King of England, Louis XVIII. of France, on the eighth day of the year, Alfred Russel Wallace was born two miles from the north bank of the Severn, at Usk, on the river Usk. He was named after a Mr. Richard Russell, and we learn that the reason why he always spells his middle name with one l is that, after the christening, the parish clerk, who was no great speller, so wrote it in the register! No more inexplicable case of "psychic influence"--and it has lasted for eighty-three years--is told of in the spiritualistic chapter, although Home and Stainton Moses figure there. He came of virtuous Church-of-England middle-class stock, not at all sordid or vulgar. His father, having a competency at first, did not practise his profession of attorney, and, by his ignorance of law and business, gradually sank into extreme poverty. When Alfred was about six, the family moved to Hertford, and, after a year or two of teaching by his father, he went to the public grammar school, where he learned nothing but the nomenclature of geography, chiefly of English towns, and above all the Latin grammar; and this is the only schooling (in the narrow sense) that he ever had. The vestiges of some knowledge of Latin still appear, now and then, in his sentences, especially in constructions that are bad in a language in which the order of succession of the words is the only clue,[†*] as well as in the frequency of "I and brother William," "I and my wife," "I and Mr. Mitten" (vol. i., pp. 246, 247, 251, 337, 339; ii., 49, 61, 238), though in the accusative it is "my brother and me" (i., 256). More than once in this book he deplores an incapacity for language which he attributes to himself. But, as to this, it is necessary to distinguish between a natural incapacity and early want of facility due to one's self-communions not having been such as to exercise one's faculty. We take leave to doubt any lack in him of the faculty itself, for the few facts at our disposal rather point the other way. Thus, his description of his school life shows that he was anything but industrious; yet he gained enough Latin to pick out the sense of the Æneid, and no doubt to parse the sentences. Later he found it "very easy" to learn Malay; and although that language is, as he says, of the simplest construction, especially the dialect of Sumatra, with which he presumably began, yet it may be doubted whether any grown man whose capacity for language was decidedly defective would have been so particularly struck with the facility of the task of learning it. So, during his sojourn in Wales, he greatly enjoyed the Welsh church services; he enlarges upon the beauty of this ancient tongue, which is quite noticeable for its various modifications of its words, and he praises the elocution of the preacher in a way that implies that he followed the speech, word for word, though it was only a Sunday recreation for him. But the evidence we most rely upon is his own remarkably lucid, easy, and harmonious style of writing; remarkable, we mean, in comparison with that of others who, like him, have never received any instruction in rhetoric. With little opportunity to compare his own performances with those of other unpractised writers, he would at first naturally judge of his own talent by the effort it cost him to express his ideas, although this effort must have been largely due to want of habitude. His self-estimation was further influenced, no doubt, by the grade-numbers that two itinerant phrenologists had assigned to his bumps of language.

At the age of fourteen his school days were brought to a close, and after a few months he joined his eldest brother, who was a surveyor. Alfred took very kindly to this business. The alternation of outdoor and indoor work was greatly to his taste, and the mathematical ingredient attracted him strongly. This is deeply graven in his correspondence. The disposition he has shown through life to express himself in maps and diagrams, together with his love of regularity and order, may incline us to think that Wallace is one of the mathematical class of thinkers. Meditation is dialogue. "I says to myself, says I," is the vernacular account of it; and the most minute and tireless study of logic only fortifies this conception. The majority of men commune with themselves in words. The physicist, however, thinks of experimenting, of doing something and awaiting the result. The artist, again, thinks about pictures and visual images, and largely in pictured

bits; while the musician thinks about, and in, tones. Finally, the mathematician clothes his thought in mental diagrams, which exhibit regularities and analogies of abstract forms almost quite free from the feelings that would accompany real perceptions. A person who from childhood has habitually made his reflections by experimenting upon mental diagrams, will ordinarily lack the readiness in conversation that belongs to one who has always thought in words, and will naturally infer that he lacks talent for speech when he only lacks practice.

Another part of Wallace's education that must not be altogether forgotten consisted in his spending nearly a year in a silent and contriving trade, that of the watch-cleaner and jeweller. But circumstances carried him back to surveying, and just then the railway fever rose to such a heat that surveyors commanded high pay; so that, though we may be sure that he would not have had the audacity to obtain what some others would, yet in six months he laid up £100. This being increased by a legacy of £50, he was enabled to join his friend, Henry Walter Bates, in a voyage to Pará. Some years earlier, he had become deeply interested in botany; and more recently Bates had drawn him into a passion for beetles and butterflies. Before sailing, he had had the great good luck to secure the services of Mr. Samuel Stevens as his London agent. He remained on the Amazon, Rio Negro, and Uaupes through four years, dispatching collections to Mr. Stevens just sufficient to pay his expenses. On his return voyage he took with him more, to the value of £200, astutely calculating, we may presume, that if he himself got safe to England, so would they. It fell out otherwise. The ship took fire. Wallace was miraculously rescued, and saved only his life and a few sovereigns. The trusty Stevens, however, had of his own motion insured the specimens for £150, and lo! this was paid. He now wrote and published two books, which just paid the printer, the time being a dead loss, from a monetary point of view. He next desired to go collecting in the Malay Islands, and, after much difficulty and delay, Government, at the instance of Sir Roderick Murchison, who was no ordinary scientist, but a swell, presented him with a first-class ticket overland to Singapore.

Wallace remained in the Malay Archipelago for eight years, studying the living forms in the forests of the chief islands and many smaller ones. He was thirty-one when he went, thirty-nine when he returned. Those years were passed in intellectual solitude. All that time he hardly spoke except in Malay, a language without abstractions, comparatively. His only constant servant was a native picked up on the north shore of Borneo. That such a life must bring a great but dangerous education to a young man we may be sure. He came home even more ignorant of how to steer his bark than he went out. He had gone for no better reason than that he was captivated by the accounts of the fauna and flora. He had not the slightest idea that he was going to the one country where a collecting naturalist could gather a fortune in specimens. Before he returned, he committed the folly of sending home a paper giving the theory of natural selection, and defending it. Was he a duke or a millionaire, that he could afford to shock every right-minded man with such a theory, whose enormity was aggravated by its being pretty evidently true? Perhaps he thought it his duty to mankind, though mankind decidedly thought not; yet even when he learned that Darwin had long had in hand a

great work to the same purport, he had not the common sense to suppress his own book, and sink it deeper than ever plummet sounded. His conception of Natural Selection (at least, as he now holds it) is superior to Darwin's, in that he maintains that variation in every character of every form is so great in every generation that the vast majority of the young are destroyed without reproducing; so that a new species could be established in a century, if changes in the environment were rapid enough to call for such swift transformation. Of course, such variations exist.

Returning to England, he found he had earned a competence. Let him keep still, leave mankind to shift for itself, and distrust his own potential folly, and a happy life was before him. Alas! his ignorance of the world and want of appreciation of that ignorance were such that ere long the savings were evaporated, and he found himself in the desperate condition of having to live on his pen. Still, even then, had he written what was most conservative and indisputable, carefully concealing his original power, he might doubtless have obtained an appointment to a position where he could give carefully measured vent to his genius. But, perhaps feeling that he had not been put into the world for that, he preferred defending startling hypotheses that are not of a nature to be verified or disproved by decisive experiment. The result naturally was to press him more and more into byways of thought, diverging constantly further from the sober conservatism of worldly interest. Far be it from us to blame the veteran naturalist whose paradoxes have been so instructive to us. But it concerns us to understand how he came to develop as he did, since several of his arguments must derive much of their weight with the general public from the high scientific standing of their author; and two of the most impressive chapters of the present volumes are calculated and partly intended to produce momentous changes of the reader's opinions largely by force of the confidence he will have come to place in the author's power of eliciting the truth of the matter to which they relate.

We repeat that Wallace is a great scientific reasoner; and of course this implies that he is perfectly fair-minded, and sincerely anxious to do full justice to that side of each question which he combats. We may add that, where he differs most from received opinions, his arguments are in general the most carefully considered and consequently the strongest. Certainly, his argument against vaccination, as it is presented in his 'Studies,' is extremely strong. The presentation of it in his 'Wonderful Century' has been more admired by lawyers, but its force is too much directed against refuting his opponents rather than to studying the facts of the case.

The spiritualistic experiences detailed in the second volume of 'My Life' simply cannot be read by any person of open mind without producing a strong impression. But the author admits that the impressive phenomena come very rarely; and when we turn to such a book as Arthur Lillie's account of his friend, the Rev. William Stainton Moses, who was probably in all respects that one of the powerful mediums who most inspires confidence, and there see in what an ocean of incredible nonsense the manifestations are swamped, we ask ourselves whether it is possible for anybody to hold his attention long upon such dire rubbish without

great danger of being thrown into such an abnormal state of mind that his testimony may perhaps be no better than that of a person in an hypnotic trance. At any rate, it is the most unwholesome nutriment for the mind, and we are glad that Mr. Wallace did not long continue his active interest in it.

His Socialistic doctrine, which seems to be of a variety peculiar to himself, rests wholly upon a definition of justice as requiring that every child shall have, in every respect, an opportunity precisely equal to every other's. He seems to think it an axiom that such justice ought to be carried out. It is a kind of justice singularly at variance with the dealings of nature with individuals. It could only be remote from viva-voce criticism and discussion that such a proposition could in his mind be metamorphosed from being a thing impossible to believe to being a thing impossible to doubt.

To sum up, this is certainly a very entertaining book, highly instructive in several distinct ways. The volumes are very attractively clothed, and there is an index of near fifty pages.

82 (22 March 1906) 242-243: HALDANE'S DESCARTES

Descartes: His Life and Times.

By Elizabeth S. Haldane. E P. Dutton & Co. 8vo., pp. xxviii., 398.

CSP, identification: Haskell, Index to The Nation. See also: Burks, Bibliography; List of Articles; MS 1504 (draft).

Elizabeth Sanderson Haldane (1862-1937) occupied her time not only with philosophy but also with the political affairs of her day. Haldane was quite well known locally for her interest in the politics of her native Scotland. Besides this volume on Descartes, she also published a translation of Hegel's Lectures on the History of Philosophy (1892) and a biography of George Eliot (1927).

The facts which render Descartes interesting to us of to-day, may be summarized under three heads. First, modern philosophists are substantially unanimous in reckoning him as the founder of modern philosophy. Secondly, some of those who are most competent to judge of such a matter, tell us that his vast influence in philosophy is closely connected with the fact that, in an age of great mathematicians, he was either the second or nearly of that rank. That is, he cannot be compared with Fermat, but his power was not far from that of Desargues. He was particularly helpful in mathematics, for it was he who gave analytical geometry to the public, went far toward settling the signs of algebra, and gave a useful rule about algebraic equations.

The third class of facts which stimulate our curiosity concerning Descartes consists of sundry characteristics of the man which seem almost inexplicably at odds with the first two. It is staggering to common sense to find metaphysicians ranking Descartes so very high, and yet denying almost everything that he pronounced to be mathematically evident. What he plumes himself upon most and almost exclusively is his institution, for inquiry into any subject, of a method which, as he maintains, perforce must absolutely exclude all danger of falling into error; and

yet almost every scientific proposition to the truth of which this method led him, and which, because it so resulted, he insists is as certain as that twice two is four, is now seen to be wildly false. Such is his notion that "the brutes" have no feeling, and may be vivisected as unconcernedly as one would saw through a log; that a vacuum is unthinkable, and that consequently our universe must be unlimited; that the movements of the planets are determined by vortices or whirls in the ether; that light is a material substance, resistance to the translation of which determines the law of refraction; that colliding bodies must behave in a way in which the fact is that no bodies really do. One is further surprised to find that so great a man seemed unable to see any merit in the work of Galileo, and was so disgusted with his denials of Scripture truth that he took no interest in his work; that he could see no mark of genius in the discovery by the boy Blaise Pascal of that hexagram from which all the properties common to conic sections can be deduced; and that he even affected to look down upon the works of Fermat, of Vieta, and of Desargues.

The most surprising thing of all, however, in a mind of such unquestionable power and greatness, is that Descartes seems to have been continually engaged, and that very successfully, in deceiving himself. Thus, he plainly regarded himself as the only philosopher worthy of that name that ever lived; and yet it seems impossible that, after eight years in perhaps the most admirable Jesuit college there ever was, he should not have been perfectly aware that his famous Je pense, donc je suis was taken entire out of St. Augustine's 'De Civitate Dei,' or 'De Anima,' or 'De Quantitate Animæ,' for its substance, as the form of the 'Discours de la Méthode' and of the 'Meditationes' is imitated from the 'Confessiones'; nor that he should have been totally unconscious of how far he availed himself of the results of Galileo, of Thomas Harriotts, and others whom he ignores. It would seem that at two different times he persuaded himself that he had made an absolutely clean sweep from his own mind of every vestige of belief in everything; for we cannot think that the inconsistent narratives in the 'Discours' and in the 'Meditationes' refer to the same occasion. And yet each time his definite purpose --as he would have hotly maintained it to be, had it been questioned, and as he distinctly states in the dedication of the 'Meditationes'--was to put certan predesignate propositions of theology beyond question. As long as this universal and absolute doubt lasted (for he apparently had no doubt at all that in a month or two, at the most, it would be over), he decided that it would certainly be best for him to continue in all respects to conduct himself as if he retained his old belief; as if it were possible for a man for days to keep up, without fail, a line of conduct about all things without the slightest belief in the advantage of such conduct--always, for example, using the tongs to stir his fire, instead of his fingers, though he had utterly dismissed all belief that fire would burn his fingers. One of the provisional rules that he adopted for his guidance during his period of doubt was that he should firmly and resolutely adhere in his conduct to the effect of each and every item of his former beliefs, no matter how utterly improbable it might be shown to be, so long as it was not mathematically demonstrated to be false! Verily, had he included among his doubts a very strong doubt whether he really

was doubting the while, his state of mind would have been less childish. The last part of his life was devoted to the study of physiology and medicine, entirely without books, as was his wont, with no view to any publication, but simply to prolong his own life. Yet he died of pneumonia at fifty-four, probably in consequence of his obstinate opposition to the physician whom he had called in.

Such are the facts that excite our curiosity about Descartes. We want to know, first of all, better than anybody has yet told us, what the particular character of his mathematical genius was; and then, what relation there was between his mathematical thought and his philosophy. We want to know what all our hand-books of the history of philosophy, except Hoefding's, leave very mysterious (and even that does not sufficiently explain), wherein and whereby he is the founder of all modern philosophy. We want as detailed a picture as possible of the wilful and irrational element of the man, or of whatever else it may have been that seems such, together with all that seems wise and practical; and after the facts have been given we want to see them treated by scientific psychology, so that we may gain a comprehension of the make-up of this extraordinary intellect. We want to know with the utmost minuteness about the education of the boy and of the man.

On the other hand, there are facts which restrict our curiosity, or give it special directions. Descartes took not the slightest interest either in scientific politics or in the political and ecclesiastical movements of his times, excepting so far as they might have a bearing upon his own security, peace, and dignity, and, further, excepting a lively interest in some sieges and perhaps other military operations. Consequently, Miss Haldane's chapters on the general history of his "times," however interesting in themselves (and really they tell us nothing that we have not often read before), connect themselves only in their most general outlines with Descartes. He was a bachelor, and a thorough one. After he had devoted himself to philosophy he lived in more than seclusion, changing his domicile every few months from one Dutch town or village to another, and giving as his address some place sufficiently distant from where he really lived, whence letters could be forwarded to him. He used to lie abed till noon, doing his thinking. The rest of the day he spent in writing and in amusing himself in his solitary fashion, often doubtless at the gaming-table. He took care always to have one good correspondent in Paris, for scientific and other correspondence, another at Leyden for business, and so on. The only ones in whom we are quite assured that he took a real personal interest were two royal ladies. This known, we do not particularly care in what precise order of succession he made his different abodes.

The narrative of the events of his life was told in two volumes with all-sufficient accuracy by the industrious Adrien Baillet in 1691, from information assiduously collected by an Abbé Legrand (or perhaps only later an abbé; but he must not be confounded with Père Antoine Le Grand, called the "abbreviator of Descartes" on account of his still useful 'Institutio Philosophiæ'). Baillet was not an elegant writer, but he knew, by great experience, how to write an accurate and useful book; he had all the information about Descartes that anybody can have now (barring a few minutiæ), and much besides. The volume of Blackwood's admirable "Philosophical Classics" that is devoted to Descartes is from the pen of

Professor Mahaffy. We need not say that it is a very useful book, nor that it leaves much to be desired, were it only owing to its smallness. The latter must still more apply to Edward Caird's article in the 'Encyclopædia Britannica.' It becomes clear enough, then, what was wanted in the way of an English volume on Descartes. We should have preferred something like a reproduction (abridged, if necessary) of Baillet, with annotations, and with three long appendices, one of which should explain Descartes as a mathematician, another what he did for philosophy, and a third the man himself. Of these four desiderata, Miss Haldane sufficiently supplies the first, although she omits some facts with the apparent purpose of avoiding what would be unfavorable to Descartes, such as the details of his behavior in his last illness. She evidently knows nothing of mathematics or its history--even referring to Desargues, who carried projective geometry to wonderful heights, as one "whose work in life was to make inventions which might prove of practical value to artisans and mechanical workers," and whom "we may judge to have been a popular writer and adapter rather than purely a man of science." Had she taken the trouble to refer to Moritz Cantor's work or any other modern history of mathematics, that bit of silly stuff would have been spared. Of philosophical comment there is more than enough; but it is not of the right kind, being neither critical nor elucidative of the historical position of Descartes, and simply consists in telling her readers, as if they were three-year-old tots, what they "of course" believe. Moreover, it is written from the standpoint of the vaguest and weakest variety of Hegelianism. However, the reader can skip all those parts, and probably will. The reviewer had not this happy privilege.

The nature and character of the man are insufficiently considered. We should like to have been told more of the studies at the Jesuit College of La Flèche, or, in default of that information, of the best Jesuit instruction of the time. In particular, we should like to know how much Descartes would be likely to hear there about St. Augustine and the 'De Civitate Dei.' The 'Correspondance' of Descartes, as now so ably edited in the new edition of his works by Adam and Tannery, would supply material for the exercise of a great power of psychological analysis. Instead of anything of that sort, we find only insipid, vacillating, inconsistent reflections, such as the most superficial reader would make for himself. The style of the book is easy and unperiodical; a little too much so, perhaps. Superfluous words are not avoided. We venture to guess that the phrase "later on," for an adverbial later, may occur, on the average, once in five pages, in the chapters where there is any occasion for it. At any rate, one tires of it. The index is sufficient. We might have picked the author up about many points; but we have endeavored to avoid fault-finding that would not touch the essential merits or demerits of the work.

82 (26 April 1906) 341-342: MEETING OF THE NATIONAL ACADEMY OF SCIENCES

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

WASHINGTON, April 21, 1906.

Owing to the Franklin celebration at Philadelphia, the National Academy of Sciences held a session on Monday, April 16, the day before its annual statute meeting, and adjourned at the end of the following Wednesday. The papers may have been fewer than they would have been but for the exceptional circumstances of this year, yet they included four or five of capital importance. Of the sixteen communications, four related to Geology, Geognosy, and Palæontology, three to Zoölogy, one to Botany, two to General Physics, two to Heliography, one to Statistical Psychology, and one to Logic; while two were memoirs of deceased members, George P. Marsh and Admiral John Rodgers.

In respect to pertinence to the history of these days, the palm must be awarded to Major Dutton's proposed explanation of volcanic eruptions. It was more than thirty years ago that this savant submitted to the Academy evidences that the centres of volcanic actions have nothing to do with the supposed molten interior of the earth, but are, on the contrary, never more than five miles below the surface; and by the majority of students of the subject his argument has been accepted as substantial proof of this. There are some difficulties, it is true, of which the greatest has been that neither Major Dutton nor anybody else could specify any adequate cause for so enormous an evolution of heat in the upper crust of the earth. Stromboli, for example, certainly reminds one strongly of a geyser; and for many reasons it is generally believed that the more immediate cause of any volcanic eruption is the penetration of sea-water into some space where it is subjected to considerable heat and to tremendous pressure, and sets up some chemical action which suddenly liberates a titanic energy. It is very difficult to believe that sea water ever can penetrate to any considerable fraction of the radius of the spheroid of the earth, and yet it has hitherto seemed impossible to specify any probable cause for the heat requisite to kindle any great chemical action only a few miles underground. The discovery of radio-activity at once suggested the interrogation whether this might not be the cause sought. Major Dutton, in his new paper, shows that it might supply sufficient energy, not only for kindling a chemical action, but even for producing the eruption directly. It still remains to find evidence that it actually, or even that it probably, furnishes either the whole heat or the kindling heat that is required. Thus far, no known source of radium and no known product of radio-activity has been signalized in lavas or in the beds where volcanic action has its probable centre; nevertheless, there is no reason to deny the presence of radium there.

Our leading palæontologist, Prof. H. F. Osborn, read a paper by Mr. W. J. Sinclair upon the Bridger beds of Wyoming. We may mention that there is in Wyoming a basin, some two hundred and fifty miles in diameter, called the Green River Basin. Upon the floor of this there are various deposits, some of the lowest of which are known as the Bridger beds. One of the earliest operations of Clarence King's survey of the fortieth parallel was the examination of the Green River Basin. In those days microscopic petrography was not yet practised, and the parties were unsupplied with microscopes. Nevertheless, it was distinctly made out that some of the deposits in the basin were formed of débris from the erosion of

the Uinta Mountains, while some other deposits were as clearly volcanic; but the provenance of the material of the Bridger beds was not ascertained. That they are largely lacustrine appeared clearly from the great numbers of fossil turtles and crocodiles found in them, and it was commonly assumed that they were composed of erosional matter. But Mr. Sinclair, in a most thorough search, could not discover the least bit of any. These beds, therefore, must be wholly volcanic, and Mr. Sinclair thinks it probable that the same conclusion may be extended to all the other Eocene basins of the Rocky Mountain region.

Professor Osborn also read a paper of his own upon the faunal and geological succession in Eocene and Oligocene basins of the Rocky Mountain region. He began by mentioning the prodigious multitude of fossils that had been collected, and the enormous amount of isolated study that had been made of the separate specimens. The time has now, he said, only just begun for putting the results together, and the present paper was understood to be merely preliminary to this further work. At the beginning of the geological age to which the paper refers, no ancestor of our present mammals was to be found on this continent, and some great change must have taken place by which these mammals were introduced. Last year, however, an armadillo was found at the bottom of the Bridger beds, and the

White River basin affords a large number of new types derived from Europe.

The director of the Geological Survey, Dr. Charles D. Walcott, gave a résumé of the results obtained from the study of the collections of the Carnegie Institution expedition to China, under Messrs. Bailey Willis and Eliot Blackwelder. They brought home collections of fossils showing the faunas of the Middle Cambrian especially, but also of the Lower and of the lower part of the Upper Cambrian; the latter agreeing substantially with that of the upper Mississippi basin. The fauna of the Lower Cambrian was found to be of the same type as that of the Cambrian of the Salt Range of India, which is thus at last definitely placed. Furthermore, the fauna of the Middle Cambrian in China turns out to be altogether comparable to that of the Middle Cambrian of Mt. Stephen in British Columbia and of Utah and Nevada. And again, for the first time a species of cephalopod has been found in the Cambrian, four or five hundred feet below the top of the Cambrian limestones, and it agrees with the predictions of Hyatt, although the very simplest form which he expected has not yet been unearthed. Lantern illustrations were shown of 217 species, trilobites, brachiopods, gasteropods, and half a dozen other orders. The memoir was evidently regarded by the geologists as of unusual importance even among the works of Dr. Walcott.

The chief zoölogical paper was read by Dr. Agassiz with his inimitable hereditary charm of manner, in his own name and that of Dr. H. L. Clark. It related to the classification of certain sea-urchins called the Cidaridæ, of which the very same genera have subsisted from the Jurassic era down through all the ages of geological life even to this day. The spatial range of single forms is as remarkable as their duration. One form may extend from the shore to a depth of eleven or twelve hundred fathoms. The shore species are naturally more widely distributed than the deep species, because they are transported by tides and other currents. The material studied for this work of classification embraced select specimens and series of thirty-four of the sixty-four species found. The classifications hitherto proposed have rested too much upon single features, especially upon the shapes of the spines with which these creatures are covered or upon the tubercles which are only the beginnings of spines. They have, moreover, often resulted from the examination of single specimens of the different species. The consequence has been that Messrs. Agassiz and Clark would now and then find a single individual which, according to the definitions, belonged at once to two or three different genera. The new study, conducted with far fuller collections and (we may venture to say) with equally greater dilligence, has brought out a classification that has none of the inconveniences of former attempts. In commenting upon this paper, the eminent naturalist and ethnologist, Dr. Edward S. Morse, in trenchant terms added the weight of his disapproval of the too common practice of basing not merely definitions of species, but even classifications, upon single characters.

A memoir on the life history of Pterophryne, by Dr. Theodore Gill, was read only by title, but perhaps we may hope to enjoy the perusal of it in the quarterly issue of the Smithsonian Collections. We were not entirely deprived of the pleasure of listening to Dr. Gill's refined and clear elocution, for he gave us a very curious account of a phenomenon found in the Sargasso Sea. It seems that this sea is full of small globular snarls of seaweed which are hollow and which contain fish-eggs. Now because these nests are so ubiquitious in that sea; and because by far the commonest of the fishes there is the so-called Sargasso fish, which is a sort of pediculate, the Pterophryne histrio; and further, because the eggs of these nests were believed to resemble eggs known to belong to fish allied to the Sargasso fish; and, still further, because the peculiar fins of this fish seemed adapted to building the nests, it has been unhesitatingly taken for granted by all naturalists, beginning with Louis Agassiz, who first described the balls of seaweed, and including Alexander Agassiz, who has collected and still possesses a number of them, that the eggs are those of the Sargasso fish. But Dr. Gill thinks this cannot be. Setting out from the assumption that the eggs are not fertilized before being deposited--such a phenomenon being quite unprecedented, except among ganoid fishes and their allies--he remarks that it seems quite impossible that they should be fertilized after they are thickly wrapped in the globe of seaweed. Therefore, we must suppose they were fertilezed before being wrapped up. Then, because a hollow ball would be far more easily shaped by a creature inside it than by one which should be compelled to swim all over its convex surface in correcting its form, and also because the hypothesis comports better with what we know of the habits of fishes, he assumes that the ball is formed around the fertilized eggs by these eggs themselves, which are provided with long filaments that seem peculiarly fit for such work. Next, among all the fish that naturalists have remarked in the Sargasso Sea, the only one which is at all likely to have eggs like those of the nests are the Exocetidæ, or flying fishes, which abound, round the borders of the rafts of seaweed; and Dr. Gill propounds the hypothesis that these, being surface-fishes, deposit the eggs just under the surface of the water, where they are incontinently

fertilized, after which the eggs, by automatic motions of their filaments, wind themselves up in balls of seaweed. Mr. Agassiz, as was quite proper (for there is a system of advocacy even in science; only it is held in check by a genuine desire to learn the very truth), raised such objections as he could to Dr. Gill's hypothesis, urging that there were no flying-fish in the Sargasso Sea. But Dr. Gill, though he had never himself been in this unfrequented wilderness of the ocean, cited sundry authorities who named the flying-fish among the chief denizens of this region, next after the

Sargasso fish itself; and among these authorities Alexander Agassiz was preëminent.

The truth perhaps is, that even exploring steamers avoid penetrating the great rafts of seaweed, that all about their borders flying fish abound, and whether there are or are not many of the nests in the middle parts of the great rafts is not known. The Sargasso Sea is a great whirlpool, as large as the United States, its centre is eleven feet lower than the surrounding sea-level, and it is in a region of calms. Consequently, it is a bad place for either a sailing vessel or for a steamer, whose screw might get all snarled up in seaweed, so as to be retarded, or stopped, or even broken.

Of the two papers on General Physics, one was by the world-renowned chemist, J. M. Crafts. It related to determinations of temperature between 100° and 350° C. After sketching the history of thermometry, beginning with an account of the Florence collection of thermometers supposed to have been made by Galileo, and coming down to the unrivalled thermometers of Baudin, père et fils, and to the new kinds of glass made by Schott of Jena, of which he mentioned one particular borosilicate of calcium and sodium as the most suitable for thermometers (noting by the way that glasses containing both sodium and potassium acted badly, showing not only a lag in their expansion, but, still more strangely, a lag in their expansibility), he mentioned that chemical thermometers always suffer a rise of their zero points, which sometimes amounts to twenty or more degrees of the Centigrade scale. Nevertheless, some physicists express their determinations of temperatures to thousandths of a degree, and Dr. Craft thinks that the errors can be brought within a hundredth of a degree. For that purpose, it is desirable to have as many liquids as possible, each capable of entire purification, freezing at a definite temperature, boiling at a constant temperature, and not undergoing spontaneous decomposition. Mercury and water are held to be two such substances. The only others that Dr. Crafts has been able to find are naphthalene and benzophenone. In order to use these, it is necessary to know their boiling-points under all pressures, or, what is the same thing, their vapor densities or tensions at all temperatures, or say at five-degree intervals from their freezing-points to the point of softening of glass. In the course of his own well-known researches, Dr. Crafts did this for himself. But methods have since been improved, and glass has been greatly improved, and it is more than desirable that the work should now be done by some bureau of standards. The other physical paper was by Professor Pupin, who, in a narrative of entrancing interest, carried us through the whole course of mathematical reasonings, of experiments, trails, and exercises, by which he finally attained success in getting his coils for cables made and adjusted without highly expert workmen.

The two papers on heliography were by the two chief masters of that subject, Hale the former, and Campbell the present, director of the Yerkes Observatory. Dr. Hale's spectro-heliograph has already been briefly explained in this journal, showing how he photographs the sun at different levels of its atmosphere. He has now mounted on Mt. Wilson, California, a five-foot spectro-heliograph, such as could of course never be carried on any equatorial telescope, and has obtained new and most surprising photographs, which were thrown upon the screen; but you could not afford the space for any description of them or of this most ingenious and superb instrument. The paper of Dr. Campbell, who availed himself of the collaboration of Dr. C. D. Perine, related to observations of the solar corona by an exceedingly interesting and ingenious method, the description of which must likewise be postponed. One very interesting result is, that the matter of the fibres of the solar corona are at rest relatively to the sun, or at any rate have no motion greater than one kilometre per second.

Of the two remaining papers, one, by Professor Cattell, on the distribution of the thousand American Men of Science given in his just published volume bearing that title can hardly be said to have been read, so very brief was the time allotted for it. The other was a long paper by Mr. C. S. Peirce, on the Method of Existential Graphs, by means of which he showed that this system gives a sort of diagram of the mind in reasoning, and also that there is, strictly speaking, but one way in which the different logical elements of any concept or judgment are combined; namely, by each being indefinite or indeterminate in some respect in which another element renders it determinate.

82 (7 June 1906) 475-476: Congress of Arts and Sciences, Universal Exposition, St. Louis, 1904.

Edited by Howard J. Rogers, Director of Congresses. Volume I.: History of the Congress, by the Editor; Scientific Plan of the Congress, by Hugo Münsterberg. (Philosophy and Mathematics.) Boston: Houghton, Mifflin & Co. 1905. 8vo, pp. 627.

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

Hugo Münsterberg (1863-1916) was a German-born psychologist. He was appointed professor of psychology and director of the psychological laboratory at Harvard in 1892. Münsterberg wrote extensively on psychology.

In every future history of the human mind the signal success of the St. Louis Congress must be commemorated. Justly to distribute the credit for it would probably be impossible at present. We certainly shall not attempt such a task. Speaking in the rough, as men usually speak of the credit for great achievements, the idea was the creation of Professor Münsterberg, and it was a creation such as few men are privileged ever to make. The history of its evolution will be found narrated in this volume as minutely as possible by Mr. Rogers, while the plan is explained by Professor Münsterberg. This first volume contains twenty-five papers read to the philosophical and mathematical sections of the

Congress by some of the men of all the world whose words on such subjects best commend themselves to our attention.

The introductory address, by Professor Newcomb, is very properly a blend between an ordinary presidential address and an oration, and is very accurately in taste. Custom calls for the suggestion of a scientific idea in a presidential address, and an idea of wide scientific appeal. If Professor Newcomb had a hundred such jotted down in his note-book, he could not have selected one of more pressing importance or of more evident truth than the idea he chose, while in its whole philosophical scope it has certainly been treated hitherto with something like disdain; so that those who for many years have been preaching its salutary effects and consequences must be gratified to find their idol taken up by a man so prominent in the world of science as Professor Newcomb. The idea is, that while it may be true that all evolution, be it physiological or physical, intellectual or spiritual, individual or social, proceeds without any strict breach of continuity, yet it is universally found that in every development there are at least two extraordinary leaps. Professor Newcomb only mentions one, which he illustrates in the launching of a ship; but we venture to point out that the laying of the keel is another such great step. So in the development of an individual animal, one step takes place when, the unit-cells of two progenitors uniting, a third life suddenly appears, while another step takes place when the new being is launched and breaks its way into the element it is to inhabit. Now the evolutionists never cease to tell us that we are to look at the life-history of the individual to find there a miniature record of the past history of its race. If this be so, the logic of science commands us to begin with the hypothesis that there have been at least two cataclasmic epochs in the development of the race, and forbids us to surrender this hypothesis until inductive inquiry has fairly ascertained its truth or falsity. There is a natural presumption in favor of something like the doctrine of universal continuity, but upon uniformitarianism, which goes further, there lies a heavy burden of proof.

We can here mention only one or two among at least a dozen strikingly instructive papers that the reader will want to ponder long before he will have sucked their juice. It is a curious classification which adds mathematics to the usual list of normative sciences, æsthetics, ethics, and logic; and since a reviewer's métier is infallibility, we will curtly say that it is a confusion of thought to class mathematics with the theory of reasoning simply because its business is to say what conclusion would necessarily follow from each given assumed premiss or premisses. One of the most interesting communications in the volume is a discussion of the definition of mathematics, by Professor Bôcher of Harvard University. Like all others who have discussed the question, he seems to have quite overlooked a definition which one would think would have been insured against neglect by being embedded in one of the most famous of all philosophical writings. We refer to the definition of mathematics as the science of order. It is Descartes who puts it forth. He, indeed, says "measure and order"; but it has been for many years well known that quantity is nothing but ordinal sequence. We mean that this is well known to those who are versed in the subject, but not that it is well known to all those who ought for their own sakes to understand it; nor that it is well known to the whole indolent breed who call themselves "thinkers."

But, to return to the juxtaposition here of philosophy and of mathematics, the comparison that it compels between the general state of intellectual development of the two groups of students is one of the most impressive lessons of the whole volume. Yet mathematics is not as well represented in the volume as is philosophy; and particularly the expression of all that semi-logical department of mathematics which keenly interests every variety of pure intellect is, excepting in Bôcher's paper, distinctly weak. In applied mathematics, Boltzmann and Poincaré continued at St. Louis a controversy--a very uncontroversial controversy, it is true--which was not really of a mathematical nature and which had been quite exhausted, as far as they were concerned, years before. The subject should lie fallow until some new point of view is found.

A congenital defect of such a congress, in the province of philosophy at any rate, is that the principal speakers must be very few. At St. Louis there were two only for each section. To select the best two, the selector should add to a superhuman insight an absolutely complete and thorough acquaintance with all the young philosophists. For it must be the young men, if any, who are to open our eyes. Their elders have been tried and found wanting. The selection that naturally gets made is that of the men who, in the later of the previous years, have put forth the most prepotent ideas. But it is just these men that we have no need of hearing. The old tune is still running in their heads; they will harp on the one old string. The man who in silence and obscurity has been creating some strange, beautiful, and illuminating conception is the man from whom we desire to hear, but from whom there is little chance of hearing in such a congress. Some of the old leaders, and some only, express themselves in this volume, together with a few who, if not old leaders, have recently so expressed themselves that all philosophists would know what they had to say. The truth is, that the personal rencontres were of liveliest interest, but the record of what was said is considerably less so.

83 (5 July 1906) 17-18: The Dynamics of Living Matter.

By Jacques Loeb. Columbia University Press. New York: The Macmillan Co. $3.

Chemistry of the Proteids.

By Gustav Mann. Macmillan. $3.25.

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

Jacques Loeb (1859-1924) was a German-born physiologist. He was graduated M.D. from the University of Strassburg in 1884. For one year (1891-92), Loeb was associate professor of biology at Bryn Mawr College. He later became professor of experimental biology at the University of Chicago.

It is needless to say that no living wight comprehends even the outlines of the dynamics of living matter or the chemistry of any protoplasmic body. But it is an intensely interesting thing to see with what resistless march science seems to be

now approaching the final investment of the two strongholds, of which the latter must be the key to the former. The attack upon that may be said to be the highest enterprise that man has ever undertaken with any reasonable prospect of success. The body of a living being is a chemical "works" of stupendous complexity, whose operations are of a most delicate nature. One could not, thirty years ago, have ventured even upon that statement without fear of contradiction; but it is now plain that it is a problem of chemistry, in which other sciences have to be considered about in the same proportion that they must be in explaining the manufacture of sulphuric acid.

In his "Concluding Remarks," Professor Loeb declares that the goal of biology is "experimental abiogenesis." He doubtless thought the time had not come to pick one's words upon such a point, but in any sense in which such a feat could be the goal of biology, it must include a practical understanding of the chemical constitution of albumens and nucleic acids. These are all optically active bodies, twisting the rays of light which pass through them, albumens to the left, nucleic acids to the right. At present, we can isolate such bodies only by the aid of other optically active bodies. Indeed, as yet we do not know what holds the atoms even of ordinary chemical compounds together. If chemical synthesis were uniformly accompanied by an evolution of heat, we should, no doubt, infer that attractive forces hold different kinds of atoms together in one molecule; but as long as there are decidedly stable bodies, such as acetylene, in whose synthesis heat disappears, it is plain that something besides attractions or repulsions must be concerned in the effect.

When we say that we understand the constitution of a chemical substance, we mean that we know what all the linkages of pairs of atoms are, and also what those modes of connection are that are not described by saying with what atoms each atom is linked and by how many bonds, but require a "stereochemical diagram" to represent the case. We now know that the reactions of a chemical body depend, not only upon its constitution, but also upon its impurities; for many of the most violent reagents are absolutely inert when they are absolutely pure; and there can be no doubt that the minute quantities of different salts which accompany the different albumens are essential to their chemical behavior. Even neglecting them, we cannot write the constitutional formula of any natural albuminous substance; and even if some archangel were to draw it up for us, with its fifteen thousand atoms represented by close-packed letters on a large sheet, where is the human intellect, to which the diagram could reveal much? We do, however, now know, in a general way, that perhaps three-fourths of an albumen molecule consists of various alpha-amino-acids (which unite the characters of acids and bases), linked together as acid-amides--a description which, to a chemist, is sufficiently comprehensible and does not imply any terrible intricacy. There are further theories of Dr. Mann which appear to be almost proved, and which promise a still more definite conception of the protein bodies. It can no longer be said, as the chemistry books of only a few years ago told us, that the constitution of the proteins is "completely hidden in night." Dr. Mann gives a list of twenty-eight of their "primary dissociation products," or bodies which are almost fully present in the albumens, and discusses them clearly. His work professes to be based upon that of Prof. Otto Cohnheim, but in truth has many original merits of its own, and upon more than one point opposes Cohnheim's opinion, sometimes with great ability.

Dr. Mann's volume is severely scientific. Professor Loeb's, without any special charm of style or manner, recounts a history of ingenious and sound research by many biologists which will absorb the attention of average readers even if they find a reference to a dictionary occasionally helpful. Its main purport is that all the phenomena of development, self-preservation, and reproduction are capable of plain and complete physical and chemical explanation. It is not pretended that we are already in possession of such complete explanation; but that the explanation awaits only a further development of chemistry and of physiology (possibly of anatomy, too) to come to light. Upon this point, which is the kernel of his volume, Professor Loeb does not make his logic quite clear. His conclusion, that physiological life is fully explicable upon physical and chemical principles, is repeatedly asserted by him in unmistakable terms. His declarations are so emphatic that he is led to deflect such terms as "mysterious" and "metaphysical" from their precise philosophical acceptations in order to intone the disapproval, not to say reprobation, that he entertains for the opposite opinion. Yet he never tells us just how his position is supposed to be logically justified. If he only meant that it is a thoroughly vicious scientific method to introduce any other than purely physical and chemical hypotheses, in the present state of the question, then the reviewer, to speak for one very humble intelligence, would go along with him most heartily (though in opposition to some eminent anatomists), and would quite agree that it is morally wrong to contaminate science with such uncalled-for considerations. But such a merely regulative maxim of method would, after all, concern the scientific investigator alone, and not the general public, since it would not necessarily carry with it the slightest denial of the likelihood that "mysterious" and "metaphysical" agencies are at work, but only a denial that we are yet in a condition to prove their existence scientifically. Or, again, if Professor Loeb merely meant to say that the facts already in our possession are sufficient to render it improbable that the "mysterious" and "metaphysical" agencies, even if they exist, play more than an exceedingly subordinate part in the phenomena of physiological life, then we could understand how he might perhaps logically have reached such a conclusion. But his language is too absolute to afford room for such an interpretation.

The last sentence in the book, in its two clauses, both defines his position and affords some ground for characterizing it. It runs thus: "The idea that mutation is working in a definite direction is a mere anthropomorphism [this expands the term "anthropomorphism" to a vast and nebulous word, expressive of little but the utterer's aversion to that of which it is predicated], and, like all anthropomorphisms, is in contradiction with the facts." That is to say, because the writer is a brilliant leader in a difficult branch of physiology--a degree of eminence which can hardly have been attained without almost exclusive absorption in that branch of activity--he undertakes to make an absolute pronouncement upon a vexed

question which concerns every department of human experience. Nevertheless, think what we may of such questions of logic, it is undeniable that the book is full of the most instructive and extraordinarily interesting matter, in large part new to all but the most fully informed, which is presented with great perspicuity, and put in as simple a form as possible.

83 (12 July 1906) 43: The Life and Experiences of Sir Henry Enfield Roscoe.

Written by Himself. New York: The Macmillan Co. $4.

CSP, identification: Haskell, Index to The Nation. See also: Burks, Bibliography; List of Articles; MS L 159.273.

This makes the third autobiography of an English pupil of Bunsen, though one of the three never actually worked in that master's laboratory, and the reminiscences of another, a really great chemist, have only been privately printed. Roscoe, if not great, has been unquestionably eminent in inorganic chemistry. His memoir on vanadium is perhaps the most complete and admirable study of an element that has ever been made; for although it was made forty years ago, nothing additional of serious consequence has since been contributed to our knowledge of the chemistry of that metal, excepting the existence of certain compounds belonging to that class of complex inorganic acids which were first explained and as a class discovered by Dr. Wolcott Gibbs. We must not, however, forget the recent application of vanadium carbide to the production of steel having desirable properties. The photochemical researches of Bunsen and Roscoe are likewise deservedly famous, forming as they do one of the foundation stones of modern chemical dynamics. The interest of the present book, however, lies chiefly in the account it gives of the development in England of scientific education; for it has been in this and other public services that Sir Henry's life has been most valuable.

He joined the faculty of Owens College in its darkest hour in 1857, and labored for it through twenty-eight years, until largely--we may say, chiefly --by his effort, it had blossomed out into the University of Manchester.

Subsequently, when the London University needed to be remodelled, he was called to its vice-chancellorship, and under him it took on new and better life. He it was who carried its home from Burlington Gardens to South Kensington. He has done a vast amount of truly patriotic work on several royal commissions on technical education, on secondary education, and on matters of hygiene. He sat in Parliament as an adherent of Gladstone for some ten years, and gives an interesting account of the great man's ascendency and of its causes. In short, though Roscoe is a very different man from Playfair, and quite opposite to him in several characteristics (among which, we must warn his intending reader, is that of his anecdotes), yet the interest of his volume, beyond the picture it affords of a certain scientific circle, is mainly its carrying on further the narrative of English progress in scientific ideas that is contained in the Memoirs of Lord Playfair.

Mr. Galton has more than once noticed the strongly marked resemblances among the descendants of William Roscoe, author of the Lives of Lorenzo de Medici and of Leo X., as well as of a monograph upon the Ginger-Arrowroot (-Banana?) family of plants. He was Sir Henry's grandfather. Stanley Jevons was his cousin and intimate friend. He himself has decidedly the traits traditionally attributed to Englishmen (minus insularity, however), and, more specifically, of the Lancashire man, who is rather a downright, outspoken, uncompromising person, apt to air his peculiarities, yet easily taking the color of his environment. But even more than other Lancashire men, Sir Henry has always been a sympathetic and highly popular personality.

The dress of the book, like all Roscoe's publications, is pleasing. It contains over seventy reproductions of photographic portraits and views, which are exceptionally perfect as reproductions, as photographs, and as likenesses.

The index, on the other hand, is so meagre as to be almost worthless.

83 (26 July 1906) 78: NOTES

CSP, identification: MS 1505 (draft). See also: Burks, Bibliography.

Miss A. M. Clerke's "System of the Stars" (London: Adam & Charles Black; New York: The Macmillan Co.) is a second edition; but it has been subjected to such drastic revision, that scarce a page has escaped material modification, and it may be considered as substantially a new production. It first appeared in 1890, when the present system of astronomical observation, which almost revolutionizes the work, had hardly come into use. In 1890 the direct optical observation of the heavens was the rule, and scientific observation of photographs was the exception; while at present, at all the great observatories, it is the photographs that are mainly relied upon in physical astronomy, and direct observation is only resorted to exceptionally. Miss Clerke thoroughly understands the work of observatories, their problems and their methods; but the peculiar merit of her writings lies in the judgment with which she selects the topics which will be the most interesting to the auditory that she addresses. Although the present publication followed soon after her "Problems of Astrophysics" (a book of which a well-worn copy will be found in every observatory), it does not traverse the same ground. The object of the present work is to instruct the general reader, while that of the "Problems" was rather to suggest to astronomers the manner in which the frontier of their advances might best be rectified. Upon one point a note of warning may be sounded. Regarding all those problems toward the solution of which astronomy is advancing by well-settled methods, Miss Clerke's representation of the present state of its siege-operations is in every way admirable; yet on certain questions so much remains doubtful that no definitive answers to them ought now to be attempted. One such question is whether the visible universe occupies but a finite region of infinite space. There are indications which seem to point to such limitation; but sound logic must still counsel a suspension of opinion. Miss Clerke writes as if it were quite certain that we see only to a limited distance. The volume contains beautiful new plates, reproductions of some of the best of the celestial photographs. Of course, they cannot show all that is to be seen upon the originals. The index is excellent.

83 (13 September 1906) 226-227: ARISTOTLE'S ETHICS

Aristotle's Theory of Conduct.

By Thomas Marshall. London: T. Fisher Unwin; New York: The Macmillan Co.

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

During the past hundred years more books have been written about Aristotle's "Ethics," and mainly the Nicomachean treatise, than any other work of Aristotle; but they have not, on the whole, been distinguished for intellectual power. Minds of more than ordinary force have endeavored to give truthful representations of the Stagirite's moral views; but their accounts have been pretty uniformly tinged with their authors' own opinions. For anybody who may resort to the original, the most useful aid will be J. A. Stewart's "Notes." The presumption that the reader of Mr. Stewart's work has the original text before his eyes may perhaps palliate its ceaseless and glaring misrepresentations. It is, in fact, not Aristotle's ideas of morality, but those of Mr. Stewart, that are there to be found. Often they involve conceptions that no dweller in Athens in Alexander's time could be supposed to have; sometimes they are the most modern ideas; often they are simply the doctrines of Kant or of a Kantianized Platonism. There are not wanting cases in which Aristotle is represented as saying what in other passages, or even in the very passage interpreted, he categorically denies. Moreover, Mr. Stewart's work, although almost indispensable to the student of the Greek text, is too narrow in scope to answer the purpose either of the student of philosophy or of the general reader. He never, for example, except in the most desultory manner, touches upon the question when and how the existing text came into being. We speak of the general reader, because Aristotle's work, unlike modern treatises, is not chiefly occupied with the theory of morality. Its main purpose is practical; namely, to aid men to behave on all occasions with moderation and good sense. In that respect it is certainly one of the most interesting and improving books that ever was written. Mr. Marshall gives a skilful paraphrase of the whole treatise, intermingled with explanatory remarks showing the relation of what is said to the state of Athenian society at the time, etc., while at the bottoms of his pages he skims the cream of the Greek text, for the benefit of those who though they can enjoy that language in bits, would lose patience on being asked to read long passages.

The plan is admirable, and is well carried out. The practical parts of the work could not have been rendered more judiciously; so that the volume makes agreeable and profitable reading.

The work has, however, certain shortcomings. Mr. Marshall is capable of making somewhat sweeping assertions that seem to be supported but by the slenderest of premises. We do not know how he has ascertained that the Athenians of Aristotle's day did not regard the great works of sculpture and architecture as worthy of any deep admiration; but (p. 217) he gives us his word that "to regard them as ennobling agencies for the education of mankind would have seemed nonsense to an educated Athenian.... To admire art is not vicious; it is a permissible relaxation; it relieves moodiness and low spirits--so Aristotle and Pericles seem to have thought, but their admiration did not go much further." In like manner, it seems to us that Mr. Marshall is over-confident of the completeness of the historical record, when he avers (p. 174) that the problem of free will was not raised as a serious difficulty until the fifth century of our era. For, not to speak of the fact that the debates on that subject mentioned in Saint Augustine's "Confessions" took place in the fourth century, it is difficult to believe, when the Stoics, from Zeno down, insisted on Destiny, while Epicurus and his followers were emphatically for free will, that the endless disputes between these sects on every other conceivable question should never have touched this one; especially after Parmenides had asserted the universality of Necessity, and Socrates that the man who knew virtue would inevitably pursue it, while Aristotle puts forth the doctrine of free will with no hint of its being a novel idea, but rather the reverse.

The whole volume is more or less tinged with the author's attribution to Aristotle of an opinion to which he himself happens to be personally predisposed. This opinion is that the distinction of right and wrong is an artificial creation of men, or at any rate that their bounds are so. His only formidable reason for attributing the opinion to Aristotle lies in a single sentence which that philosopher set down on the first sheet of papyrus of his manuscript. Namely, having remarked that the whole inquiry is a political one (which was the natural point of view to a Hellene), and that, as such, it cannot be expected to do more than to render the matter clear, since scientific exactitude does not belong equally to all subjects of reasoning any more than to all handiworks, he adds: "The ideas of the honorable and right, as politics treats of them, present so much divergence and anomaly {diaforan kai planhn} that they seem due to instituted law alone, and not to nature" {wste d okein nomw monon einai, fusei de mh}. This sentence arrests attention, and at first sight appears to support Mr. Marshall's view. But on re-reading it we remark that it is not a categorical assertion; that Aristotle does not even use the expression, "it seems to me," but merely says that so it seems from the point of view of politics {peri wn h politikh skopeitai}. Moreover, the utterance stands quite alone. Mr.

Marshall is able to bring no other passages to its support, except those in which it is said that conduct is the subject of praise or blame, as if this were not true on any ethical theory, and more so on almost any other than on the one he attributes to Aristotle. Finally, in the fifth book, we come upon a categorical and emphatic denial of the truth of the doctrine in question. What does Mr. Marshall say to that? He simply uses the higher critic's routine method of dismissing difficulties, by supposing that the reporter of the particular lecture represented in that part of the fifth book misunderstood what Aristotle

had said. If we suppose, he says, that such utter misunderstanding took place, and that Aristotle never revised the report, there is nothing to prevent our believing that Aristotle said just the opposite of what we read in the text. He is quite right there: on such terms, we can give any interpretation we like to any passage.

For our own part, we entertain no doubt that the manuscript of the Nicomachean "Ethics," though assuredly not intended for publication, was prepared by the hand of Aristotle himself. One of several valid reasons is suggested by the title of the longer exposition, "Nicomachean." Nicomachus was the name of Aristotle's father and of his son. Half a dozen ancient authorities tell us that the work was dedicated to the latter by Aristotle. It is altogether probable that this was the case. But surely Aristotle would not have dedicated a work he had not himself written. During the many years through which the manuscript was in use in the school it is natural to suppose that annotations, such as cross-references, would have been inserted in it; but that any change of its main doctrine should have been permitted is quite incredible. The history of the Aristotelian texts ought to be investigated by a comprehensive, objective, thoroughly scientific and well-considered method; and that done, the present practice among even eminent critics of suggesting inconsiderately that this or that sentence, or even chapter, is spurious, should be discredited.

As for the present question of whether Aristotle regarded the distinction of right and wrong as wholly conventional or not, we have only to read the text just as it stands, and we obtain a result that is consistent and intelligible in every particular. He would have shown himself a poor rhetorician if he had planted himself upon immovable ground in his opening lecture. It was far better to let the mixed audience understand that every theory would receive fair examination at his hands. Mr. Marshall's method, however (if such it can be called), leads him into such a slough of contradictions that he is at length obliged to declare that Aristotle addresses his treatise to the kind of people who do not care to carry their beliefs to their logical consequences.

The weak spot in Aristotle's treatise considered as an aid to the practice of virtue is that he assumes man to have an immediate power of will which, without any previous preparation, can be summoned upon emergency to overcome any temptation. It is the usual error of the partisans of free will. He recognizes, indeed, that a given virtue can be acquired only by habituation; but he seems to think that a person who does not possess the virtue in question is able, on occasion, to behave as if he had acquired it. Great as his discoveries in psychology were, he had never found out that repeated performances of any action in vivid and detailed imagination--say, for example, in imagining that one moves one's right foot round a horizontal circle clockwise, while one moves one's right hand round a parallel circle counter-clockwise--is almost as effectual in creating a habit of so acting, as if the outward acts were really performed. We now know that that same action--the same in quality, if not equal in intensity--that is performed when we really act, is also performed when we vividly imagine we act; only, in the latter case, we add to that exertion an opposite exertion inhibiting it. This principle could not be directly applied to the cultivation of a habit of activity, since along with the habit of making the desired exertion one would equally be growing a habit of inhibiting the exertion. But in a self-warfare against any of the innumerable vices for the cure of which a habit of inhibition is alone required, this method is advantageous.

83 (25 October 1906) 353-354: An Introduction to Logic.

By Horace William Brindley Joseph. Oxford: The Clarendon Press.

CSP, identification: Haskell, Index to The Nation. See also: Burks, Bibliography; List of Articles; MS 1506 (draft).

It is surprising in what a variety of ways the different schools of logic of our day endeavor to lay the foundations of their science. One appeals chiefly to mathematics, another to metaphysics, a third to the general notion of a sign, a fourth and fifth to this and that branch of psychology, a sixth to linguistics, a seventh to the history of science; and still the list is incomplete; and there is an equal disagreement as to the business of logic. The school to which Mr. Joseph seems to give his adherence, which is that of the majority of English logicians, is none of those just mentioned; for it troubles itself very little with any questions of method, but just goes its way, scattering opinions upon points of logic, and attaching to this one and to that any reason that may suggest itself. Such promiscuity of method might be expected to issue in great variations among the doctrines of the different members of the school; and it is true that some of its adherents--Miss

Constant Jones, for example, and Alfred Sidgwick--have written books of marked originality, which readers who dip into the subject without diving too deep find very suggestive. The greater number, however, among them the author of this volume, are led, by whatever unseen and inscrutable power it may be, to reproduce in the main the divisions and forms of the traditional logic, slightly modified by metaphysical doctrines, partly in most cases those of Mill, mixed not infrequently with those of Mill's philosophical antipodes.

When, however, we say they reproduce the traditional divisions, we only mean that verbally they do so, for the significations some of them attach to the old terms of logic, have only preserved the shell and have cast away the kernel. For instance, Mr. Joseph excludes from the class of universal propositions all those which do not imply the existence of some individuals denoted by their subject, as well as all those which do not predicate by "conceptual necessity," both of which exclusions flatly conflict with the Dictum de omni of Aristotle and with all tradition, and which, taken together, make one simple proposition to express at once both existence and necessity, which constitutes a third breach of traditional usage in connection with the use of the single term "universal." In such ways as this he effects a verbal agreement with the traditional doctrine by demolitions of parts of the existing system of nomenclature and using the debris to begin, but only to begin, the erection of a new system in the place of it, like that architectural performance of Charles V. in Granada.

We shall not find fault with any man for any sincere study; and if it gives him any particular pleasure to call his study logic, that word is by this time pretty near past spoiling. Only we would suggest that to define one's object of study in such a way that scarce anything would be excluded--as when Mr. Joseph and others of his school tell us that logic ascertains "how we think"--it seems fair to demand that their conclusions should be based upon inductions correspondingly broad. The reviewer, for example, can have nothing but praise for a logic he dimly remembers reading in the sixties, which carefully anallyzed all those phrases of the German language that are equivalent to conjunctions, without any pretence that it covered all the possibilities of thought in this narrow class of conjunctional elements. We might think, however, that before coming to those variations of thought, it would be better to begin by considering all the ways in which we must think in order to draw all kinds of necessary conclusions; for even if logic be concerned with all modes of thinking, we are inclined to the opinion that it is specially concerned with those forms of thought that have some bearing upon the validity or non-validity of different ways of reasoning. It was fifty-nine years ago that Professor De Morgan called attention to certain forms of inference of great practical importance, which nothing in the logic-books explained or gave any clue to, although it is necessary to sound reasoning to distinguish the cases in which such arguments are valid from the cases in which they are invalid. Yet we do not remember ever having seen any mention of these forms of inference in any treatise of Mr. Joseph's school. Here is an example of one of them:

Every dollar that ever was or will be in the safe was or will have been received as a loan.

For every dollar ever received as a loan a payment of a dollar will be made.

Hence, every dollar that ever was or will be in the safe was or will be paid out.

When De Morgan opened this road to logical inquiry, he opened a road to sempiternal glory for British logic; but unfortunately investigators of any vigor of thought were lacking. De Morgan did much more than that; for he also opened up the logic of relations, which had from the beginning been a well-recognized branch of logic, and which, when American and German logicians developed it, turned out to throw a wonderful new light upon every part of logic, while the logicians of the Oxford school merely advanced little reasons for thinking a logic of relations to be impossible.

The distinguished mathematical genius, George Boole, produced a method of logical inquiry by means of algebra, which was a most brilliant achievement. A few logicians of the most numerous British school have paid attention to the Boolian logic. We mention here, not because they are the best, but because they are the most soundly critical, Venn, Jevons, and Keynes. The great body seem never to have looked into it.

Posterity will say that human intelligence is under vast obligations to the logical work of Alfred Bray Kempe, sometime President of the London Mathematical Society. But one may search in vain for any evidence that logicians of the

school we are speaking of, have so much as divined what relevancy his laborious researches have for any real problem of reasoning.

It is a matter of regret to us that the brevity of this notice forces us to confine ourselves to Mr. Joseph's faults, since they are the faults of his school, and it is not quite fair to the individual to judge him exclusively according to the genus to which he belongs. But the truth is that when we have said that this treatise possesses such original merits as the majority of the thousand treatises can claim that have appeared since Michael Scott and the western publication of Aristotle, and perhaps possesses a little more, we have accorded to it all the notice it merits; while its school is of more importance simply on account of its numerical greatness, and because of the evil that it is working to British thought. This school is composed mostly of indolent and often feeble minds whose interest in logic lies in the professorships, the fellowships, the popularity as tutors, or other sources of bread and butter which they enjoy, and who are therefore sworn obscurantists, bound to oppose any movement of real thought in English logic. When a study fails to develop definite and well-considered methods;

when it is not animated by a sufficient passion to find out the truth, whatever the truth may be, to insure the careful study of all the work that earnest students do within its province; and when in place of manifold new discoveries, it does no more than verbally reproduce foregone conclusions, it is idle to boast that is a science.

Mr. Joseph sometimes lays down general propositions without any pretence at making their reasonableness evident; and he justifies this practice by saying that his book is not a complete treatise on logic, but, as its title indicates, is only "an introduction to logic." Now, says he, in the first introduction to any science there must be more or less dogmatism. Is this Oxford pedagogics? Elsewhere, if a man is to write an introduction to any science--say chemistry--he will draw a clear line between information as to what has happened, and dogmatic insistence upon principles and the like, and will take the utmost pains in describing, say, the experiments of Lavoisier upon the oxidation and reduction of mercury, to show the convincingness of the reasoning; so as to remove the idea that there is to be any appeal to authority or other arbitrary determination of principles. If he has to teach botany, he will probably set the beginner at work to dissect a flower with his own eyes and fingers, and to describe what he sees; and will be careful to make him understand that botany is only an orderly description of what can always be seen under favorable conditions. It appears to some of us that the first steps in any science ought to deal with those departments of the science that come most in contact with the life and interests of the students; and we should not approve of an introduction to botany, whose first two hundred pages were occupied with the artificial, though indispensable, technicialities that botanists are compelled to use in order to describe species and other forms. Yet it is the like of that, that Mr. Joseph does in logic; and useful as his book may prove to an advanced logician, it is almost the worst possible for a beginner's introduction to the subject.

83 (20 December 1906) 544-545: Side-Lights on Astronomy and Kindred Fields of Popular Science: Essays and Addresses.

By Simon Newcomb. New York: Harper & Brothers. $2 net.

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

Simon Newcomb is not only the most eminent astronomer now living, according to the judgment of the French Academy, expressed in making him one of its five carefully selected Foreign Associates, a judgment supported by the opinion of the scientific world, but he is also a remarkable reasoner; and a good dozen of the twenty-one chapters of the present volume afford valuable lessons in logica utens. In addition to that, he has, for a scientific man, a surprising command of language, not of phrases recherchées, but of that eloquence which comes from turning the tap of thoroughly filtered thought and allowing it to run crystal-clear and copious. His "Reminiscences" show (unconsciously, no doubt) how well adapted the circumstances of his boyhood were to making him a conversationalist; and this volume is conversational, in the sense of being at once light and serious. It would be hard to find a serious book more entertaining, or a light book that affords better exercise in reasoning.

Perhaps it is a critic's perversity that disposes us to note the kind of reflections that so distinguished a mind either overlooks or suppresses. In a chapter on the "World's Debt to Astronomy," after showing how valuable the services of that science have been to navigation, surveying, and geography, the author very truly says that, great as that debt is, man owes less to the stars on that score than for the tremendous message of their awsome reality. Very true; but does the science of astronomy add to that lesson more than it deducts from it? Professor Newcomb will hardly contend that lying on one's back of a summer night contemplating the poetry of the heavens is science; and if Goethe was right, the scientific view is that the smallest smoke-ring is, in itself considered, as vast and as sublime as the Galaxy. The lesson of devoutness seems to have put the veritably greatest debt of man to astronomy quite out of the head that should be the last to forget it. For Professor Newcomb ought, at any rate, to have mentioned that in the common view it was astronomy that actually taught men to reason scientifically.

The volume is divided into a smaller part discussing those questions in which the astronomer almost becomes a metaphysician, and a larger part devoted mostly to consideration of the methods of astronomical and other scientific research. The smaller part, in accordance with the turn of the author's genius, seems to have received more of his care. It is true that he has discussed such questions as the limits of the universe with more elaboration in another book; but the presentation here is captivating. We confess we cannot understand how the promise of the preface, conveyed in the words, "it became incumbent to do what he [the author] could . . . by revising the material and bringing it up to date," can be reconciled with a number of statements in the larger fraction of the volume. For instance, on p. 213, we are given to understand that the computations of the American "Ephemeris," so far as they concern Venus, are derived from the tables of Dr.

George W. Hill, and so far as they relate to Mars, Jupiter, and Saturn from old tables corrected on account of more recent observations. But every recent volume of the almanac in question states that the places of Venus and Mars are derived from Newcomb's own tables, and those of Jupiter and Saturn from Hill's.

The most valuable chapters are those in which Professor Newcomb definitely limits himself to the needs of science at the present day and in this country. He repeatedly alludes with great force to the disadvantages of personal isolation, from which investigators in this country have often suffered. Think of the momentous consequences to science of that little accidental chat between De Vries and van't Hof, which led to the proof of ionization and to much else. Professor Newcomb sanely preaches a greater consolidation and unification of scientific research. He rightly says that this need not stand at all in the way of the individualism which is also requisite.

If we have conveyed the idea that the book deals wholly with generalizations and generalities, we beg to say that such an impression would be entirely false. The interest is largely derived from its explanations of details; and some of the chapters are almost entirely of such matter. The whole process of making an astronomical objective, according to Alvan Clark's method, is described, with prices, etc.; and in other chapters there are details, though not always the very latest items. But the work is not a treatise; it is a collection of addresses and of magazine articles, and as such treats mainly of broad questions. The dress of the work is most agreeable. The uncut folds of the paper are at the bottoms of the leaves, which are gilt at the top, thus preserving the volume from dust.

1907

84 (24 January 1907) 92: The Scientific Papers of J. Willard Gibbs.

2 vols. New York: Longmans, Green & Co. $9 net.

CSP, identification: Haskell, Index to The Nation. See also: Burks, Bibliography; Fisch and Haskell, Additions to Cohen's Bibliography; MS 1507 (draft).

Josiah Willard Gibbs (1839-1903) was one of the greatest mathematicians of his time. He was a Yale man throughout, having taken his undergraduate degree there and his Ph.D. there in 1863. He was elected professor of mathematical physics at Yale in 1871. Gibbs delivered a series of lectures at Johns Hopkins University during the time Peirce was associated with that school. Gibbs is credited with the development of a new system of vector notation, for which work he was elected to membership in the Royal Society of London in 1897.

That Josiah Willard Gibbs advanced science the world over more than it has ever been given to any other American researcher to do, can hardly be questioned. He published but one separate book, his "Elementary Principles in Statistical Mechanics" (Charles Scribner's Sons), which appeared in the Yale Bicentennial Series in 1902, the year before his death. Another volume in the same series, written by his pupil, Edwin B. Wilson, was founded on his lectures. His only other printed remains are the papers now collected, which are few but fundamental. They are substantially limited to three, not counting an unusually small number of preliminary and supplementary outputs.

Of the earliest, relating to diagrams and models representing the effects of temperature and pressure on all sorts of substances, Clerk Maxwell once spoke to the present reviewer in terms of warm laudation, before Gibbs had produced anything else, and when he was all but unknown in this country. His second work, on the equilibrium of heterogeneous substances, taught chemists how to reason about the final results of reactions (without reference to the processes by which they were reached), and it stands to-day the stone at the head of the corner of dynamical chemistry. The memoir itself (in which, by the way, was first given the now celebrated "phase rule") occupies three hundred pages of the first of these two volumes, a good many more pages being substantially parts of the same whole.

The second volume is mainly occupied with Gibbs's peculiar calculus called "vector analysis," which was designed to supersede quaternions and Grassmann's Ausdehnungslehre. It is now taught in sundry European universities; but its vogue was prevented or hindered by a trait of its author's character that struck everybody that ever met him, and that we know not how otherwise to designate than as diffidence. Yet this is not a fit name for it. It certainly was not that diffidence which consists in timidity; nor can we assent to his brilliant scholar Prof. Bumstead's apparent view that he was unconscious of his own superiority, which would come too near to making him a gifted idiot, rooting up his mathematical truffles like a Périgord pig, and as oblivious of being deprived of them. We should rather conceive of it as an exaggerated estimate of the possibility of any opinion of his being erroneous that might concern a difficult question not susceptible

of a demonstrative solution. He thought his method ought to be left to make its own way in the world; but he overlooked the fact that he was not giving the offspring of his brain the fair start to which it was entitled. For he limited himself to printing and privately circulating a fifty-page syllabus of that method, with no illustrations of its application. The industry of a man of great parts and attainments would not more than have sufficed to construct any decided opinion upon such a question from such a basis. If Gibbs himself, after devoting his own surpassing genius for some years to the matter, was not prepared to put forth a categorical decision as to the merits of the method, pray who else could be expected to undertake the office? We can only say that the ease and mastery with which his scholars have handled some of the most thorny problems of physics, as contrasted with the infertility of the quaternionists, incline us to put our trust in "vector analysis."

The book is clothed in all dignity and beauty of paper and type, carries a noble photograph of the master, and in every way (except by an index) recommends itself to the liking of friends of American science. There is a good, but restrained, notice of this most genuine of high intelligences by a worthy interpreter, Prof. H. A. Bumstead, who has taken Dr. Ralph Gibbs Van Name as his collaborating editor.

84 (21 February 1907) 181-182: A History of Chemistry.

By Ernest von Meyer. Translated by George McGovern. New York: The Macmillan Co. $4.25 net.

CSP, identification: MS 1508 (draft). See also: Burks, Bibliography.

This is a third edition; but great pains and no little ability have been used in bringing it to date. It is decidedly the most useful history of chemistry, though Ladenburg's smaller book carries more weight as an authority. The author has undertaken little more research than that of a faithful compiler, guided by good sense. He intends to give very brief, but generally fair, notices of the most interesting controversies, whether chemical or personal; and has aimed to narrate the whole history of chemistry so far as one volume could include it, without wasting one line upon any facts extraneous to the science, merely for the sake of holding a reader's interest. Three quarters of the volume are devoted, as was proper, to the modern period, when every theory has been subjected to quantitative tests. It is divided into two chapters, the one a survey of the advances toward an understanding of chemical combinations generally; the other, somewhat longer, going into particulars, with separate sections on analytical, inorganic, organic, physical, mineralogical, physiological, technical, and pedagogical chemistry.

The book begins with a short history of the chemistry of the ancients. Many excellent special works on this subject, some of which are named, absolved the author from a longer account of it. We note that the word "bronze," from {bronthsion} may be of Persian origin. The name "Nubia" is thought to be from the

Egyptian word for gold, "nub"; and we read that the gold mines of that country yielded, under Ramses II., no less than £125,000,000 annually. The remainder of the first quarter of the book is composed of three chapters, on alchemy, on the chemistry of Paracelsus, Van Helmont, George Agricola, and their contemporaries, and on the chemistry of the age of phlogiston from Boyle to Black and Priestley. The services of Western alchemists are exaggerated, as they usually have been. Science is enriched by those whose nature is to aspire after truth, not by those who are goaded on by naught but gold-greed.

The section of the work that deals with organic chemistry should be the best, not merely because it is the field in which chemical research proper, as distinguished from chemical physics, has met with its greatest, perhaps its only signal, successes, but also because we read on the title-page, "Being also an Introduction to the Study of the Science." Now in organic chemistry every group of bodies has presented a problem, the problem of the constitution of those bodies; and in every case, as the science now stands, this problem either has already been worked out, or is approaching that end, by the thorny, but interesting and instructive, path of alternate reflection and experimentation. What we find is that, at the bottom of each page, Professor Von Meyer has cited, on the average, four or five memoirs. The dates are seldom given; merely the authors' names, the titles of the journals (of which there are forty or more), numbers of the volumes, which run up into the hundreds, and the numbers of the pages. If, however, the young student does not read those memoirs, there is nothing in Von Meyer's pages to give him any clear idea of the course of chemical thought, unless in its very first essays. All he will find is only a very general indication of what each memoir contains, expressed in technical terms, with vague comments about each, such as a chemist whose memory was rather hazy concerning a memoir might make in conversation with a brother chemist. Take, for example, one of the most important and long puzzling matters, upon which a vast deal of reasoning and laboratory work was spent by a dozen leading chemists, as well as by many others. We refer to what is called Perkin's Reaction. On this subject Von Meyer begins with this vague remark:

The beautiful synthesis, by W. H. Perkin, sr., of unsaturated acids from the aldehydes and the salts of the fatty acids, has made these compounds more easy to come by, and therefore to investigate, and has thus helped to elucidate their constitution.

He goes on for a dozen lines of such matter, which, even if a beginner could understand it, would give him no idea of the thought that has been expended upon the phenomenon. Not one word is vouchsafed in regard to the successive intensely interesting researches which were believed to throw light upon the rationale of the reaction. The memoirs cited at the foot of the page may refer to them. He does say that "the systematic researches of Fittig and his pupils have contributed to round off and deepen our knowledge of this class of compounds"; but is not that a bit vague? We find no mention in text or notes of the names of Bertagnini, Genther, and Hübners, who before Perkin had severally published pertinent facts; or of the paper of Conrad and Bischoff, which corrected an error in Perkin's formula.

In short, the work is convenient, because there is no better one (except Ladenburg's, which is too small), and in spite of its numerous inconveniences. Among these is its avoidance of dates, for which the word "recent," in a highly elastic sense, and relative to the first, second, or third edition, is made to do duty far too often. As to the book's serving as an "Introduction" to chemistry, we do not understand in what sense it can have been so regarded. A volume far more explanatory, from the same publishing house, Lachman's "Spirit of Organic Chemistry," begins, "This book is intended primarily as a supplement to text-books of organic chemistry." Even that book could not be understood by a beginner in the subject, though it might introduce a more advanced student to the practice of reading the memoirs. Nobody can be a scientific man of any kind who has not that habit. Chemists of this continent, quite unintentionally no doubt, have received signally less than justice at Von Meyer's hands, except Mallet, Sterry Hunt, and one or two others.

84 (28 February 1907) 203-204: Thought and Things: A Study of the Development and Meaning of Thought; or Genetic Logic. Vol. I.

Functional Logic, or Genetic Theory of Knowledge.

By James Mark Baldwin. New York: The Macmillan Co. $2.75.

CSP, identification: MS 1509 (draft). See also: Burks, Bibliography.

We will say, at once, that this is a most earnest, profound, laborious, systematic analysis of cognition, such as cannot fail to be of continual utility to students of psychology. But this does not mean that the work is fundamentally sound; for the imperfection that belongs to all human works necessarily appears in a philosophical doctrine in the form of error. Now, Professor Baldwin's book, although it is a work of psychology, and although it is the boast of modern psychology that it is a special science, like any other, yet undertakes to solve the deepest problems of philosophy, just as Wundt and many other psychologists profess to do; just as biologists often attack these problems under their several special lights.

What is meant by "Genetic Logic"? This is a hard question. The author devotes an introductory chapter of five and twenty pages to it, without affording any satisfactory reply, although, of course, he supposes that he has done so. Everybody knows what the science of logic, in its proper sense, means. It is the comparative anatomy of arguments. Everybody who has anything like an inside acquaintance with any of the successful sciences knows that an essential to the well-being of any science--its bread and butter, so to speak--is the restriction of every technical word to its strict meaning. Each new scientific conception calls for the manufacture of a new word. Extensions of the application of terms ought to be marked as extensions. When chemists talk of "alcohols," the plural suffices to show that they are not speaking of spirits of wine merely, but when mathematicians speak of symbolical "multiplication" and logicians of logical "multiplication," their practice begins to be rash. Although generalization should be as free as air, still generalization ought to find some modified form of expression showing that it is generalization. The reflections of any mind that takes its part in the present life of science must bring all this home with insistence; but before the days of Young and Lavoisier, or when Buffon was at the head of biology, it had not yet become manifest; so we can find ample excuse for Kant, that, having a multitude of new conceptions to introduce into philosophy, he should have sought to render them acceptable by giving them the names of more or less obsolescent conceptions more or less analogous to them. Along with much still worse wrenched nomenclature, he spoke of "transcendental logic"; and Professor Baldwin's "genetic logic" is by no means unlike an elaboration of that part of Kant's "transcendental logic" which was omitted from the second edition of the great "Critik"; and was omitted, not, we take it, because the great philosopher thought its substance erroneous, but because he thought it irrelevant even to "transcendental" logic.

It would be interesting to trace out the history of logic from a little before Kant until to-day, and to show how, as physical ideas acquired predominating influence, and as the so-called "moral" sciences aped the physical sciences, the original idea of logic as the study of how reasoning ought to proceed sank to the idea of how the human mind normally and regularly does proceed. The old-fashioned or pure logician will tell you that the question whether an argument is sound or not has nothing in the world to do with how men think, but is simply and solely whether the being of the facts virtually premised (including the facts of the order and effect upon the mind of its experiences), does or does not include the fact inferred. These things the psychological logicians hardly discuss. They analyze the process of development of thinking; how, precisely, from sensation we come to perception, from perception to memory, from memory to imagination, from imagination to fancy and playing, and so on to thought. One of Professor Baldwin's reasons for calling his investigation "Genetic Logic" is that he undertakes very minutely to trace out this "progression" or series of "progressions," as he holds that it takes place in the mind of every man, every time he thinks.

But he has a further reason. Professor Baldwin's studies of the minds of two children are famous, all the world over. He believes, and many psychologists, no doubt, believe with him, that he accurately interpreted all the stages through which the consciousness of those children passed, from birth until they became as easy of study as are the minds of grown persons. Now, without being explicit, he pretty plainly thinks that the stages of growth of the child's mind are, to an important degree, reproduced in the "progressions" of the mental representations of the grown person, every time he thinks. In this second respect his "logic" is genetic. But it is more than inconvenient that it should be called logic. After explaining, none too lucidly, in his first chapter, what he means by "genetic logic," the author remarks:

There will doubtless be many who refuse to follow the use of the term logic here suggested, especially among those who find it impossible to give up the tradition. . . . [Tradition is simply everything in the use of words, and especially of terms of

science.] Those who wish to call it Psychology, or Epistemology, may be free to do so with no fear of doing the work injustice. As to names, chacun à son goût.

Ah, but here is a great misapprehension. It is by no means a question of taste; for the fact is that Professor Baldwin does not call this "logic" as if he had called it "subcutaneous pneumatics," or by any other meaningless vocable. He calls it so under the impression that it is a legitimate generalization of the traditional meaning of the term "logic." He is evidently quite unaware that he and the pure logician inhabit different universes. They not less do so than two persons of whom, throughout a long debate on "reminiscence," one by that term should understand the beating of brains in the effort to recall experiences, while the other should all the time mean the events remembered.

What the real nature of Professor Baldwin's book is, so far as a careful study of the published third of it discloses, is a psychological analysis of the operations of cognition; and so considered, it appears to the reviewer to be a signal setting forward of science, what the Germans would call, by an exaggeration customary with them, an "epoch-making" work. This judgment by no means implies that the theory is true in all its details: it rather implies the contrary. Indeed, we venture to think that at least one error permeates the whole, while many of the steps of the proposed progressions are open to no slight nor vague doubt, even if we grant, what the author has by no means proved, that there is any one invariable process by which thought is developed. But the reason why, in our opinion, the publication must serve as a precious landmark in future investigations, is that it lays down, for the first time, a definite project of structure of the theory of cognition in great detail. When one has to build a house, at length a definite plan is drawn, in which all the conditions--the utilities, the prices of one and another kind of masonry, the sizes of timber that are staple, etc.--are duly considered; and this first plan becomes, from the time of its execution, the focus of study out of which the veritable building plan is to be evolved. It is a plan to be pulled to pieces, to be patched up, and perhaps, at last, to be discarded. Yet otherwise than by the aid of such a preliminary plan, a thoroughly satisfactory house can hardly be had. Just such a preliminary project it is, with which Professor Baldwin has now enriched the psychology of cognition. The mere vocabulary of well-considered new technical terms--some fifty in number--that this volume expounds is in itself a precious gift to psychological investigation. For with each of these new terms there goes a valuable new conception.

We fear the reader of this volume, who has most likely found it pretty difficult and complicated, will be almost staggered when we say that we believe the prevalent fault of the theory to be that it makes the structure of thought and of ideas to be too simple. We only mean, however, that it is too simple in a certain definite respect; namely, that what the author calls "dualistic"--that is, referring to two subjects, or objects--is sometimes rather triadic; while the projective consciousness which he admirably describes, but which according to him involves no dualism and we grant does not explicitly recognize two subjects, must, as we venture to think, in order to be projective, regard the objects perceived as differing from others by their insistent presence. If so, there must certainly be a dualism,

however little it be recognized. For insistence implies something resisted, and where there is resistance there must be effort, and effort with its correlative resistance is clearly dualistic. Moreover, the author frequently speaks of the "meaning" of an "object." It appears to us that meaning belongs exclusively to signs; and a sign, as the medium between two minds or between an object and an idea, and being so regarded, however obscurely, must involve a triplet.

85 (12 September 1907) 229: NOTES

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

Albert Stickney's "Organized Democracy" (Houghton, Mifflin & Co.) is one of those radical pleas for political reconstruction which, however little likely to be adopted or even seriously considered, are not without usefulness as criticisms of existing political evils. Mr. Stickney is convinced not only that we have not true democracy in this country, but also that we cannot have true democracy so long as the present electoral and administrative systems prevail. Under popular election of all officials for fixed terms, joined to the party system, all that the voter can do is to vote for the candidate of this or that machine; his own personal choice, if he have one, he cannot possibly register. The remedy Mr. Stickney urges is the establishment, in local, State, and Federal Government, of a system of single-headed administration, with the heads of departments controlled directly by a Legislature the members of which are popularly chosen by viva voce vote. For tenure during short terms there would be substituted tenure during good behavior. Congress, for example, would become a body of one house with the power of removing the President, but without control over subordinate appointments. We fear that Mr. Stickney is too optimistic, and too little appreciative of the difficulty in this country of achieving reforms by wholesale; but his shrewd observations and obvious seriousness make his book not uninteresting. Incidentally, we commend to the curious the extraordinary punctuation of the volume.

85 (17 October 1907) 355: The Collected Mathematical Works of George William Hill.

Vol. IV. Washington: Carnegie Institution.

CSP, identification: MS L 159.277, MS 1509a(s) (draft). See also: Burks, Bibliography.

This is a quarto of solid mathematics of 460 pages, which differs from the other three volumes in that about a quarter of its matter here appears for the first time in print. The thirty-three memoirs are of various degrees of importance. All but two or three relate to planetary and lunar theory.

This science, which in Ptolemy's time, in Keppler's, in Newton's, represented the very highest climbs of scientific intelligence, to-day, by force of its own perfectionment, and by the growth of other sciences that began by being its pupils, is reduced to an art of performing excessively intricate calculations. It must be a peculiar mind that can devote a lifetime to it; and with less devotion there is no chance of being able to improve it. Tradition (along with something else) influences the scientific world greatly to honor any exceptional mastery of it; and yet it has now come to offer the barest minimum of interest from the point of view of philosophy or that of positive science, and scarcely more from that of logic; not even very much from a purely mathematical standpoint. Its difficulty consists in its extreme intricacy and in the extraordinary exactitude of the observational data with which its results must be confronted. As to the complexity of the facts themselves, it is as nothing compared with that of almost the simplest terrestrial phenomena; for the single dynamical law involved--that of gravitation--is all but the simplest conceivable, and has already been most thoroughly studied.

In another five or ten centuries we may hope that such calculations as Mr. Hill's will bring some discovery of vital moment, just as Lord Rayleigh's fastidiousness about the specific gravity of nitrogen did. But our hope is subject to two parious conditions: first, that the Greenwich Observatory will keep up its work during all that time; and, secondly, that men will continue to be produced with faith, like Mr. Hill's, sufficient to induce them to spend their lives in computations which can have no useful results in their time, and for which none will thank them but those in whose breasts their enthusiasm can kindle a sympathetic spark. It is that spark, hot and lasting, which is the second reason of the honor that all true men of science pay them.

Among the papers in this volume which do not relate to planetary or lunar theory, there is none that compares in interest with that infinite determinant by which Mr. Hill succeeded in solving for the first time an important class of differential equations. A useful method is developed for deducing the coefficients of a power series from special values of the series. There is an extremely interesting memoir of forty pages on the distribution of gravity over the earth's surface, in which the author introduces an idea which he has repeatedly introduced into his astronomical work.

There is also a paper upon the proper choice of a projection for a map whose "chief end is to present to the eye a picture of what appears on the surface of the earth." The paper occupies but eleven pages, and we should not have taken notice of it here, but for the fact that it furnishes some data for the study of a question in a branch of science whose students would little dream of seeking light in anybody's "collected mathematical works." The question relates to psychology: namely, how much justification is there for two imputations ordinarily made upon the wisdom of mathematicians in general, especially theoretical astronomers, and above all others Laplace?

One of these imputed characters is a readiness to assume superior competence to deal with some matters that are altogether outside their horizons. The other is a disposition to take short cuts to the solution of problems, mostly practical, that properly demand examination of the results of extended experience, and to take these short cuts by setting up hastily adopted principles as entitled to overrule every other consideration. This is substantially what Napoleon said of Laplace,

although the real fault was Napoleon's own for supposing that great capacity in one narrow direction was any reason for expecting marked talent in a totally different line.

The particular problem of the map-projection which Mr. Hill treats, plainly calls for a thorough acquaintance with three extensive classes of facts. In the first place, the problem is a psychological one; and a large mass of psychological observations bearing upon the question are on record; and they can readily be supplemented by experiment. In the second place, a knowledge of the methods of the cartographical draughtsman is called for. In the third place, the problem, far from being at all novel, has been many times luminously treated by men well-versed in all three classes of facts.

Mr. Hill's memoir, however, shows insufficient consideration of each of these three classes of facts. He bases his conclusion upon principles which seem to be, considering the high rank of the author as a scientist, astoundingly arbitrary, and certainly not universally true. Two of these principles were first put forward by great mathematicians; but one of them is, none the less, obtrusively absurd to anybody acquainted with the art of cartographical draughting, while the other, apparently based upon a consideration of small consequence, conflicts with the defined purpose of the maps under consideration. In short, the memoir is a remarkable instance of a publication which, beginning by clearly defining its purpose as a practical one, neglects all the practical aspects of the problem, and busies itself exclusively with matters of curiosity which are practical trivialities. The map of Asia that is given at the end is certainly better than the frightful deformities which, until recently, were given in our atlases. But it does not present as good a picture of the continent as some others. If, instead of Asia, what was to be pictured had been the entire United States and all its possessions, a very different projection would show the relations between the parts very satisfactorily.

85 (31 October 1907) 396: THE WORK OF GEORGE W. HILL

Peirce wrote the editorial response to this letter.

TO THE EDITOR OF THE NATION:

SIR: I am sorry to see the Nation promulgate so inadequate an idea of the work of George W. Hill and other investigators like him in celestial mechanics, as it does in the issue of October 17. In substance the statement is that the science of planetary and lunar theory, which once represented the highest climbs of scientific intelligence, is to-day reduced to an art of performing excessively intricate calculations. You then imply that the performance of these calculations is all that the class of men of whom you are treating have to do, and you naturally see very little interest in it from the standpoint of philosophy or of positive science. Now, if you will slightly change your wording, and say that through the labors of a series of investigators from the time of Newton to that of Hill the theory in question is being reduced to an art of performing intricate calculations, you will hit the truth.

What gives significance to the work of Hill and those in the same field is not their patience in performing calculations, but their ability to show how it is possible, by calculations within the power of one man, to reach results which would have required the labor of many lives if the methods had not been invented. Any good computer, under capable supervision, can make the intricate calculations. It is the method that costs. SIMON NEWCOMB.

Washington, D. C., October 17.

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

85 (19 December 1907) 570-571: LORD KELVIN

CSP, identification: MS L 159.287. See also: Fisch, First Supplement.

Sir William Thomson, Baron Kelvin, died December 17, after an illness of some weeks. He was the greatest reasoner of his time about physics, and at the moment of his death was, without dispute, the greatest scientific genius living. In the art of subjugating a question of practical physics and bringing it under the salutary domination of mathematics, it may be doubted whether history can show his equal.

He was born in Belfast, in 1824. His father had broken the ancestral line of farmers, and ultimately became professor of mathematics in the University of Glasgow. His brother, Dr. James Thomson, professor of civil engineering in Glasgow, made at least one important and penetrating contribution to mathematical physics--facts indicating a family bent in that direction. William Thomson was a precocious lad; but he retained through life a generous enthusiasm and a sprightly wit that gave him something of the charm of a boy. He entered the university at eleven years of age. He was twelve when the Glasgow circle received an accession in the person of J. P. Nichol, author of "The Architecture of the Heavens," who became a close friend of Thomson's father and of the boy. The latter, late in life, testified that the poetical imagination of Nichol had first fired him with devotion to physics, and that the same master's enthusiasm had incited him to study Fourier's "Théorie de la chaleur." From Glasgow he went to Peterhouse, Cambridge, where he was graduated in 1845 with high honors. Already he had an established reputation as a mathematical physicist. In 1841, at the age of seventeen, he had published an able memoir upon the conduction of heat and upon the connection between the mathematical theories of heat and electricity, involving, too, important new discoveries in pure mathematics. Another paper published by Thomson at the age of eighteen gave his method of determining geological dates by means of underground temperatures. Immediately upon his graduation, Thomson repaired to Paris and entered the laboratory of Regnault, who was then

engaged in his fundamental determinations relating to the theory of the steam engine. In the same year, Thomson published in French his vindication of Coulomb's law of statical electricity, for the supposed refutation of which Sir W. Snow Harris had received the Copley medal of the Royal Society. Though Harris's conclusions were based upon an elaborate series of delicate measures, Thomson overthrew them with one blow, and that upon the strength of well-known experiments of a rudimentary nature. A logical objection has of late years been raised to Thomson's argument; yet those who bring it forward do not revert to Snow Harris's conclusion. Ten scientific papers, all of great merit, were published by Thomson the year of his graduation. The following year appeared his wonderful theory of electrical images, which is a geometrical method whereby a certain class of refractory problems about the distribution of electricity receive a solution. This excited high admiration among the mathematicians.

In 1846, at the age of twenty-two, Thomson was made professor of mathematics in the University of Glasgow, and also editor of the Cambridge and Dublin Mathematical Journal. He soon began to produce his wonderful series of instruments for electrical measurement, and this modern art, with its extraordinary precision, owes far more to him than to any other individual. We now come to his researches upon heat, which contributed in no small degree to the progress of civilization, both directly and indirectly. Two theories of heat had been entertained from early times, the one, which is now current, that it is an agitation of the particles of the hot body; the other that it is a fluid. Sir Humphry Davy had advocated the kinetical theory, and Rumford had put it into a strong light by his experiments. Nevertheless, the difficulties of this hypothesis seemed to be such that the fluid theory generally prevailed. Sadi-Carnot had in 1826 published a great work upon the steam engine founded on the latter basis. Meantime, general ideas about energy were developing themselves. The now celebrated memoir of Helmholtz on the conservation of forces was published in 1846; but it failed to attract much attention. At length many things--Joule's determinations, certain confirmations of the kinetical theory by Regnault's researches, and the increasing importance of economy in steam engines--brought physicists face to face with the question, How is Carnot's theory to be amended so as to accord with the doctrine that heat is a mode of motion? It was Rankine in 1849 who first answered this question; but only upon an assumption quite unsupported by observed facts. The next year Clausius outlined the theory as it is held to-day, taking for his postulate that temperatures tend to equalize themselves by the flow of heat from hot to cold bodies. Thomson had worked out the problem independently from a slightly more cautious premise, before the memoir of Clausius appeared. This confirmation was useful in giving physicists confidence in the analysis, and hastened the general acceptance of the modern doctrine. Thomson now became much occupied with the theory of heat. In 1852 he announced the principle of the dissipation of energy; that is, that there is a flow of heat from warmer bodies to cooler ones, which goes to waste, since it cannot be reconverted by ordinary means into mechanical work. Thus, the whole universe would seem destined to become lifeless by the conversion of all energy into heat uniformly distributed throughout the whole. From 1852 to 1862 Thomson and Joule published in collaboration ten memoirs on the thermal effects of fluids in motion. The most important result obtained was that the force between the molecules of an ordinary gas is on the whole attractive, and not repulsive, which was somewhat startling, in view of the expansive power of gases.

One of the first great enterprises with which Thomson was identified was that of the Atlantic cable. Electric signals sent through a submarine cable were found to undergo a peculiar retardation that threatened to blur them beyond recognition. Faraday had long before furnished a partial remedy, but Thomson supplied a complete one, so as to secure reasonably satisfactory clearness and speed. The correctness of his statement of the laws involved was disputed by an electrician of the cable company, Dr. Wildman Whitehouse, but the Glasgow expert disposed of the argument so effectively that he was retained on the spot as consulting engineer. He officiated in that capacity both for the cable of 1858 and that of 1866. He was also electrical engineer for the French Atlantic cable in 1869; the Brazilian and River Plate cable in 1873; the West Indian cables in 1875, and the Mackay-Bennett Atlantic cable in 1879. To the success of these enterprises he contributed in several other ways. He prescribed a method of testing the conductivity of a submarine wire while it was being laid, in order that any defect might be promptly discovered and cured. He also invented instruments to receive messages. Those employed for land wires were not sensitive enough. Thomson so mounted a mirror on a tiny magnet that the feeble electric impulses which traversed a cable would cause it to sway. A beam of light was thus deflected, first to the right and then to the left, on a blank white wall in a dark room. The magnet was suspended by a silk fibre, and its movements were practically unimpeded by friction. This invention was supplemented by one called the "siphon recorder," which would-leave a permanent trace on a strip of paper. Without question he did more than any other scientist to promote submarine telegraphy, and in recognition of these services he was knighted in 1866.

Thomson was an enthusiastic yachtsman, and did much for the marine art and science. He published special tables to facilitate Sumner's American method of navigation; he invented the compass now generally used on shipboard, as well as the indispensable method for applying Airy's theory of the correction of compasses on iron vessels. He originated the only practical instrument for deep-sea sounding. He was also one of the chief investigators of tides; he invented the remarkable instrument called the harmonic analyzer, for mechanically determining the magnitudes and phases of the twenty or more different component oscillations which enter into the tide at each port. Thomson invented a number of other calculating machines, among them a tide-predicting machine, a machine for solving equations, and, in conjunction with his brother, a remarkable mechanical integrator.

It is impossible to enumerate here all the lines in which our civilization is in debt to the labors of this indefatigable man. His genius was the dominating influence in investigation of questions relating to the age of the earth, its internal solidity or fluidity, and its rate of cooling. Another great field in which his was

the leading mind was that of speculation about the molecular constitution of matter. Upon this subject he delivered a course of lectures at the Johns Hopkins University in 1884. He did much to illustrate the exceeding complexity of the problem, and to throw light upon parts of it, while leaving it as a whole unsolved. His last extensive experimental work related to the electrification of air; but he still pushed analytical investigations when he had passed the age of eighty. Besides the treatise of Thomson and Tait, entitled "A Treatise on Natural Philosophy," but confined, in fact, to a part of analytical mechanics, Thomson wrote no books. His Baltimore lectures were reported and lithographed, his scientific memoirs were collected in three stout octavos, and his popular addresses in as many duodecimos. In 1877 he was made foreign associate of the French Academy of Sciences, usually counted as the highest of scientific honors. In 1892, he was raised to the peerage at the instance of Lord Salisbury. Baron Kelvin succeeded another illustrious mathematical physicist, Stokes, as president of the Royal Society. In his later years he was president of the Royal Society of Edinburgh.

The distinguishing characteristic of Thomson's intellect was his power of analyzing physical facts into their elementary components mathematically defined, and of identifying these components with those of other facts. He was a cautious theorizer, taking care not to lose sight of possibilities that other men might not think worth considering. His ingenuity was marvellous. His gyrostat, a thing that would stand in apparently impossible positions; his gyrostatic pendulum and gryostatic chain, with their weird motions; his bag that would allow water to run in and out freely, and yet was absolutely impervious to air; his instruments for measuring temperature, his jelly model of a molecule, and his paper of February, 1894, on the partition of space (a crystallographic problem), may serve as examples. It is the men who have themselves achieved the most in science who will be most penetrated with admiration for Kelvin. But we can never give full credit to a great man's excellences until we have weighed his defects. Kelvin had become infected by Nichol with a bias in favor of the miraculous. Hence, many of the hypotheses by which he proposed to explain physical phenomena would, if admitted as facts, themselves clamor for explanation. We refer for examples to his theory of germs diffused through the universe, and to the idea that atoms are "manufactured articles." No hypothesis seemed too unaccountable for him, so long as the observed phenomena would be necessary consequences of it. Because he was without a peer in that sort of explanation, because, after all, he did not succeed in explaining the constitution of matter (as he himself confessed), and because more evolutionary hypotheses are likely to be favored in the future, Kelvin's death may mark one of the great epochs in the history of our understanding of the physical universe.

85 (26 December 1907) 579: NOTES

CSP, identification: same as preceding item.

The career of Lord Kelvin illustrates the great opportunity and appeal which modern science makes to capable and aspiring natures. There is the element of intense mental satisfaction. To be grappling with the problems that underlie all human problems must yield its daily thrill. But this higher type of scientist never loses himself in unrealities. His sense of the actual is too strong for that. Hence he is always eager to show the bearing of his most refined speculations upon the thoughts and lives of men and the march of progress. And if, as an aside from his more arduous labors of the intellect, he is able to devise or perfect some process or tool to bring the triumphs of science home to the hearts of men, he has the contentment which comes from feeling his feet upon the earth and doing service to its denizens. His example shows what is really meant when we speak of the romance of science. De Maistre has a striking passage on the contrast between the scientific spirit of the present and the conception of science in primitive times. Then there was something almost supernatural in the idea of science; it brought up a mystic figure, "looking only at the sky, and with a foot disdainfully touching the earth only to quit it." To-day, on the contrary, science is pictured as loaded down with books and instruments, "pale with vigils and labors, and pressing forward panting on the path towards the truth, with eyes fixed ever on the ground." Yet the two conceptions are not wholly irreconcilable. In Lord Kelvin they were blended, and our point is that something of the old awe felt for the Magi reasserts itself in the case of such a scientist as he. He was seen to be one "commercing with the skies." Yet he would emerge from his closet, and display a kind of magic control over the forces of nature. Thus both reverence and gratitude attended him. The peculiar impress he made upon his time, such satisfactions of attainment as were his could scarcely be paralleled in any other calling.

1908

87 (20 August 1908) 164-165: Thought and Things: A Study of the Development and Meaning of Thought, or Genetic Logic. Vol. II.

Experimental Logic, or Genetic Theory of Thought.

By James Mark Baldwin. Pp. 436. New York: The Macmillan Co. $2.75 net.

CSP, identification: MS 1509. See also: Burks, Bibliography.

The author here continues the working out of his purpose with the same industry, and with the same quality of ability, if not perhaps in so full measure, as in the first volume (see the Nation of February 28, 1907, p. 203). But it now becomes quite clear that, however desirable the main inquiry of the work may be in itself, the project of connecting it with the science of logic was very unfortunate for the one subject and for the other.

The main motive of logic has always been to get possession of a method for determining the values of arguments. Now, it is obvious that whatever bearing the truth of one thought may have upon the truth of another will depend exclusively upon what the states of things are which the two thoughts represent to be real, and not at all upon the psychical or linguistic forms in which they are dressed, nor upon the psychical processes by which that dress is given to them. Whether we say that among sea-animals will be found some that give milk to their young or whether we say that among animals that give milk to their young will be found some that inhabit the sea, is for all purposes of argumentation quite indifferent; and the equivalence is here so evident that the school of "exact," or mathematical, logicians are almost unanimous in adopting, as their standard, or canonical, form of expressing the same fact, substantially this: "There is an aquatic mammal." Newton's great discovery is usually stated in elementary books, and is thought of by ordinary people in the form that each separate body in the solar system has an instantaneous component acceleration toward every other proportional to the mass of that other and inversely proportioned to the square of the distance between them, but is otherwise constant for all and at all times. But in writings on celestial mechanics (as in Equation 15 on p. 175 of Dr. Moulton's admirable little "Introduction" to the science), the form in which the same fact is often stated and intended to be thought is that the sum of the vires vivæ (or their halves, according to the old definition) of all the bodies of the system subtracted from the sum of the reciprocals of the distances between the several bodies, each reciprocal being multiplied by the product of the masses of the pair of bodies concerned and these masses being expressed in terms of a gravitational unit, remains unchanged. Since these two statements represent, and would in all conceivable cases represent precisely the same state of things, they are for all purposes of reasoning interchangeable. It follows that for logic they are equivalent, although, since this equivalence is not self-evident, they cannot strictly he called identical. From such considerations it follows that, in general, logic has nothing to do with different dresses of thought which cannot possibly represent different states of things; or at most has no more to do with them than to demonstrate that whatever state of things is represented by the one is equally represented by the other. That this principle, suitably modified for modals, ought to determine what is and what is not relevant to logic has been practically or virtually acknowledged in every system of logic excepting some of those which have arisen since the bankruptcy of Hegelianism, with the consequent de facto supremacy of psychology in current philosophy. But none of those which deny that application of the principle have improved reasoning in the smallest particular.

What Professor Baldwin means by calling his logical system "genetic" is that in it the main stress is to be placed upon the psychical processes by which each form of thinking is brought about. As soon as the first volume came before us, we thought it almost if not quite inevitable either that there was to be no logic, properly speaking, in the work, or else that the logical matter was to be confused by the introduction of entirely irrelevant conclusions. Since the whole of that first volume, with the exception of seventy pages, was regarded by the author himself as relating to "pre-logical" topics, and since it seemed unfair to condemn the whole on account of that fragment of seventy pages, or simply because it did not relate to logic as we conceive that science, we contented ourselves with acknowledging that it was a sound piece of scientific work as far as it went. But we find the second volume to be distracted from the pure consideration of the genesis of thought by discussions of truly logical questions--discussions which are far from strong in themselves, and which do not evince the knowledge of logic that would have been necessary for carrying them through intelligently. In these discussions, positions are taken which neither necessarily result from the genetic theory nor are supported in any solid way, but which, rather, seem to have been selected on grounds of personal predilection, or at random. For example, the author regards judgments of probability as intermediate between the "universal" and the "particular" propositions of formal logic. He comes to that opinion in consequence of his understanding the "particular" form as being, for example, "Some men are mortal," and the "universal" as being, "All men are mortal." He thus shows us that he has not read logic with sufficient attention to remark that the subject in both the logical forms is in the singular number, "Some man is white," aliquis homo est albus, and "Any man is white," omnis homo est albus. Had he told us that he proposed to wipe out the existing terminology of logic and to use the old terms in new senses, the question would have been a different one; but as the architect of a "Dictionary of Philosophy," he must, and does, know that to do so (especially without notice) would have been to trifle with the ethics of science; and therefore he certainly intends to use the terms "universal" and "particular" according to their authoritative definitions. He even goes so far as to say that when in a judgment of probability, the probability becomes 1, the proposition becomes the logical "universal." A student of the doctrine of chances who did not distinguish between the two would soon find himself in a snarl. A very large number of players sit down to play an even game against a banker. That is, each bets at each play one franc that an event will turn out one way or another, the probability being one-half that it will turn out in the one way and one-half that it will turn out in the other way. If the player loses, he pays a franc

to the banker; and if he wins, he receives a franc from the banker. But as soon as a player has made a net gain of one franc, he retires from the table, and his place is taken by a fresh player. On the other hand, as soon as the banker has netted a gain, he yields the bank to a fresh banker. Now, every player and every banker is supposed to have unlimited funds or credit. Consequently, by one of the easiest of those problems in the doctrine of chances that are called "problems on the duration of play," the probability is 1 that any given player will, sooner or later, make a net gain, and the probability is equally 1 that every banker will ultimately net a gain. So, then, if probability 1 were equivalent to a logical universal affirmative, every player and every banker must come out of the game richer than he went in, which would obviously be making money out of nothing. But the truth is that probability relates to what would happen in "the long run"; that is, in an endless run; and probability 1 means that in such endless run the expectation to which it refers will be verified infinitely oftener than it is falsified; but, for all that, it may be falsified infinitely often. A teacher of logic ought to make this clear.

Some of Professor Baldwin's work in this volume is of a far more ambitious kind than that which we have illustrated. The reader will naturally suppose, however, that if he has not been able to control his mind to sound reasoning in the small problems, he is unlikely to have done so in the greater ones. At any rate, we can testify that, having gone through the whole with the utmost care and with predilections not unfavorable to the author, we do not think it worth our reader's while to enter into the necessarily more lengthy criticisms of the more difficult problems as treated in this volume. We greatly regret our disappointment with it.

87 (17 December 1908) 609: NOTES

CSP, identification: MS 1323. See also: Fisch, Second Supplement.

Oliver Wolcott Gibbs, chemist and physicist, Rumford professor emeritus at Harvard, died at Newport, R. I., December 9. He was born in this city February 21, 1822, the second son of George Gibbs, an eminent mineralogist. The boy was, besides, brought under the influence of quite another section of the intellectual world, by close family relationship with the Channings. He was graduated from Columbia College in 1841, and thereupon entered the laboratory of Dr. Robert Hare in Philadelphia. Subsequently, he enrolled himself as a student in the New York College of Physicians and Surgeons; but after receiving the degree of M.D. in 1845 he went to Berlin in order to devote himself to chemistry under the great analyst, Heinrich Rose, then at the acme of his fame. He simultaneously studied mineralogy under the guidance of Rammelsberg. Subsequently, he was led by the rising star of Liebig to Giessen, there to bend his attention to that organic chemistry which was just beginning to crystallize in urea and uric acid; and then, as Victor Regnault was engaged in those determinations which have never yet been superseded, the young student betook himself to Paris. In 1849 he was

appointed professor of chemistry in the Free Academy, since entitled the College of the City of New York. He was already becoming distinguished in his profession; and when, in 1853, Dr. James Renwick retired from the chair of chemistry in Columbia, Gibbs was regarded as his natural successor. But to the Board of Trustees of that day, under the presidency of Charles King, the idea of appointing a Unitarian to teach chemistry in Columbia was quite too shocking to be entertained; and Dr. Gibbs continued his work in the Free Academy for another ten years. In 1863, on the resignation of Eben N. Horsford, who had been in charge of the chemical laboratory of the Lawrence Scientific School at Harvard, Dr. Gibbs succeeded to the position, and became Rumford professor. His success there is shown by the great attention the contributions from that laboratory everywhere attracted, and still more positively by the number of Gibbs's students who have since received distinguished scientific honors. He made important investigations in light and heat, but his greatest triumphs were in inorganic chemistry, where he opened up new realms, so to say, particularly in reference to complex inorganic bases and acids. His work on the platinum metals is also important. He was author of numerous articles and contributions to scientific journals, as well as to the Nation; and he was a member of many scientific societies. American and foreign. Gibbs was not only eminent as a scientist, but he was, besides, a man of great public spirit. During the war of the rebellion, he served upon the Executive Committee of the Sanitary Commission--nobody more actively. In order to aid and supplement that work, he thought it best that the earnest supporters of the war should be able to see one another daily in a club. To that end, he called a meeting in 1863, which resolved itself into the Union League Club.

— 302 —

— 303 —

OBITUARIES OF CHARLES SANDERS PEIRCE FROM THE NATION: **1914**

98 (23 April 1914) 473: NOTES

CHARLES S. S. PEIRCE, logician, mathematician, and philosopher, who died on April 20, was born in Cambridge, Mass., on September 10, 1839, the son of Prof. Benjamin Peirce, the foremost American mathematician of his time. His mother was the daughter of United States Senator Mills, of Massachusetts. He was graduated from Harvard in 1859. He was connected with the United States Coast and Geodetic Survey for a number of years, and for several years was lecturer on logic in Johns Hopkins

University. It was in this subject that his most original work was done, his papers on the algebra of logic and on the logic of relatives being pioneer work, and giving him an international reputation. His most conspicuous contribution to the philosophical thought of the time was the idea of pragmatism, afterwards developed and modified by William James. In 1887 he retired to Pike County, Pa., to devote himself completely to logic. His only companion was his wife, who, before her marriage, was Juliette Froissy. Mr. Peirce was a member of the National Academy of Sciences, and a fellow of the American Academy of Arts and Sciences. He was the author of "Photometric Researches," and of numerous articles upon logic, history of science, metaphysics, psychology, mathematics, gravitation, astronomy, map projections, color-sense, chemistry and the cataloguing of libraries. He contributed a large part of the scientific definitions to the Century Dictionary and some of the chief articles on logic to Baldwin's Dictionary of Psychology and Philosophy. During many years, Mr. Peirce was a highly valued contributor to the Nation, his reviews of mathematical and other scientific works being of unusual interest and brilliancy.

98 (30 April 1914) 489-490.: THE LONELY HEIGHTS OF SCIENCE

Haskell (Index to The Nation) cites Fabian Franklin as the author of this obituary of Hill and Peirce. Franklin studied with Peirce at The Johns Hopkins University.

Looking under the name "Hill," in the Encyclopædia Britannica, we find "Hill, Ambrose Powell, American Confederate soldier," "Hill, Daniel Harvey, American Confederate soldier," "Hill, David Bennett, American politician," and other Hills, American and English, but not Hill, George William, American astronomer. Nor did the death of George William Hill, a few days ago, evoke from the American newspaper press anything more than perfunctory notice. Yet he was one of the extremely small group of men in this country whose scientific work was of the highest order. His researches in celestial mechanics will rank permanently among the memorable achievements of the human mind in a domain in which some of the greatest intellects of the race have exerted their utmost powers. One of the many notable testimonials to his rare eminence was the publication, several years ago, by the Carnegie Institution, of his "Collected Mathematical Works," with an introduction by Henri Poincaré, the most illustrious French mathematician of the last two generations. In this introduction, Poincaré gives an outline of Hill's researches, and discusses their relation to what had been done by his predecessors, and their bearing on the future development of celestial mechanics.

So remote are labors like these, not only from the ordinary interests of mankind, but from the possibility of even a dim apprehension of their nature on the part of any but mathematicians, that it is not surprising that the man whose life is given over to their pursuit gains none of the glittering rewards, such as they are, of popular notice. And in the particular case of George W. Hill, it would be peculiarly inept to treat the absence of such notice of him, alive or dead, as a personal grievance. For Hill was, by deliberate choice, a recluse, a solitary, even in relation to the small circle of the elect who were capable of fellowship with him in the special intellectual field to which he was so exclusively devoted. And yet one can but feel that, in one way or another, the consciousness of this kind of high endeavor and achievement might well be more widespread than it is; and that if it were, the result would be a reciprocal benefit--a benefit to the man of science and a benefit to society at large--of no small value. In the case of an almost freakish simplicity and self-withdrawal like that of Hill, it may be that nothing of the kind would be possible; but his case is highly exceptional. It is not many years since there died an American mathematical physicist of similar scientific eminence, a man whose work is acknowledged to be in the first rank, and who was during his entire career professor in one of our leading universities; but how many Americans knew the name of Willard Gibbs? How many persons in New Haven--how many Yale students even--were aware that this modest scholar was one of the world's leaders in a great field of scientific thought?

It is something of a coincidence that within the few days since the death of Dr. Hill, another American man of genius has been taken away who, different in almost every possible way from Hill, was like him in that his life had for many years been passed in extreme seclusion. A man at once of extraordinary acumen and originality, and of a phenomenal range of intellectual interests, there was in Charles S. Peirce a vein of the erratic, perhaps one may say the unstable, which seems to account for his achievements having fallen far short of what might have been expected of his unquestionably splendid powers. Even as it is, however, he left his mark on at least two branches of the intellectual activity of the age. In that modern development of the science of reasoning which is usually designated by the name of Symbolic Logic, his work well deserves to be called bahnbrechend; the leading German writer on the subject, Ernst Schroeder, makes it the foundation of his extensive treatise. And it is from Peirce that William James derived the name "Pragmatism," and the doctrine for which that name stands in James's work; though, in the interest of accuracy, it is necessary to add that Peirce repudiated James's development of his seminal idea. But the concrete fruits

of Peirce's labors, many as are the fields in which he worked, are small in comparison with what those who came into contact with him could not but feel would have fallen into his hands if he had but possessed that something--constancy, balance, or whatever it may be--which nature seems to have denied him.

That there might be--that there has been in other times and other countries --an atmosphere more genial and friendly even for those whose labors carry them to those strange and lofty heights in which dwells the mathematician or the abstract philosopher, we have already intimated. But these almost self-condemned exiles from "the kindly race of men" are not the only scholars who suffer from an intellectual isolation that is deplorable, and to the nature and consequences of which we seldom or never give attention. That free-masonry of the intellect which has played an inestimable part in the higher culture of Europe is almost wholly absent, as yet, in our university circles.

98 (14 May 1914) 571: THE PASSING OF A MASTER MIND

TO THE EDITOR OF THE NATION:

SIR: I desire to record a word of tribute to one of the master minds of America. The recent death of Charles S. Peirce removes an heroic figure from the field of American learning. Living for the most part in retirement, he was known to a relatively small circle. Yet where known, his name was spoken with exalted respect; and his fame, critically appraised, placed him with the chosen few of any generation. How far he sought and failed to find the wider recognition that falls to those whose labors are in fields open to public approbation, how far he did not care to pursue the accredited steps to preferment and recognition, I cannot say. Yet this aspect of his career is significant.

My acquaintance with Mr. Peirce began in 1882 when I came as a graduate student to Johns Hopkins University. He was there lecturing on "The Algebra of Logic"--a subject which he in large measure established in this country. He had interested certain able students--all of them since distinguished in various fields--in his explorations into a broad domain of thought; and of this enterprise a volume of studies by himself and his pupils bears record. In those days there was gathered in Baltimore a group of scholars and productive intellectual workers that would have been exceptional in any scholarly community. Their names would suggest the notable contributions of American scholarship in their generation. Yet among them the impression of Mr. Peirce stands forth most prominently primus inter pares. The impression that I retain of his analyses of logical and philosophical problems is that of observing a plummet line descending through troubled waters foot by foot, sounding the depths, avoiding the weeds and the shoals, and reaching an undiscovered bottom; for to the student many of the problems in a controversial sea seemed bottomless. It was not argument, but discovery.

It was Mr. Peirce who introduced me to the possibility of an experimental study of a psychological problem. He provided the problem, the instruments

which I set up in my room, the method, and the mode of reaching the results; these were printed over our joint names. He also introduced me to the mode of attack upon larger psychological problems by methods of statistical inquiry. He gathered about him a group of five or six students and proposed a study of "great men." He drew up the questionnaire; we gathered and collated the results. The work was not finished; though I was permitted to publish one or two aspects of the material in brief papers. My personal indebtedness gave me the opportunity to gauge the measure of the man. Only one other produced upon me an equal impression of original greatness. I refer to William James. The two men may well be associated, for each held the other in high regard. Professor James recognized in Charles Peirce the true founder of Pragmatism, a way of thinking which James made popular, the significance of which he expounded. If, in addition, it be remembered that these logical, psychological, and philosophical pursuits were in a sense avocational, and that Mr. Peirce was for a long time actively connected with the Geodetic Survey, was a physicist and mathematician by profession, the scope of his attainments will be more truly perceived. In a sense he represents the American Helmholtz.

I do not know that Mr. Peirce ever held any academic position other than the lectureship for a few years at Johns Hopkins University. That his was the personal temperament that may well be called difficult may be admitted; such is the disposition of genius. It cannot but remain a sad reflection upon the organization of our academic interest that we find it difficult, or make it so, to provide places for exceptional men within the academic fold. Politically as educationally, we prefer the safe men to the brilliant men, and exact a versatile mediocrity of qualities that makes the individual organizable. All this has its proper place and is doubtless more or less inevitable, even sound; but the penalty paid for safety is too heavy, when it excludes the use of rarer gifts, the choice product of exceptional power of sustained thought. Of this lamentable lack of efficiency--to turn the sting of an abused word against itself--Mr. Peirce is not the only example. Other master minds knocked in vain at academic portals, and were refused as too elect. Or, more truly stated, the small group of their liberal-minded friends within the hallowed precincts failed to persuade the authorities to adjust methods to men. Certainly it remains true for all time that no more effective stimulus to promising young minds can be found than to give them the opportunity of contact with master minds in action. The service that a small group of such men can perform is too fine, too imponderable, to be measured; and likewise too intangible to impress its value upon the judgment of those with whom these issues commonly lie. Yet nothing would have shown better the greatness of a great University than to find a place in it for rare men like Charles S. Peirce. His memory invites not only the personal tribute, but is a reminder of our neglect of the true worth of genius.

JOSEPH JASTROW.

The University of Wisconsin, Madison, Wis., May 6.

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).

†[*] For example: "Before leaving Singapore I wrote a long letter to my old fellow-traveller and companion, Henry Walter Bates, then collecting on the upper Amazon, almost wholly devoted to entomology and especially giving my impressions of the comparative richness of the two countries." This is far from being one of Wallace's worst sentences; but it illustrates the Latinity (i. 349).